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Worms, Farms and Schools

Three Essays on Investment Choices in Uganda

Benedetta Lerva

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Academic dissertation for the Degree of Doctor of Philosophy in Economics at Stockholm University to be publicly defended on Thursday 27 August 2020 at 10.00 in Nordenskiöldsalen, Geovetenskapens hus, Svante Arrhenius väg 12.

Abstract

Quantifying Externalities in Technology Adoption: Experimental Evidence from Ugandan Farmers

I investigate how positive externalities contribute to underadoption of agricultural technology among sub-Saharan African farmers. I focus on the adoption of pest-control technologies. A farmer can benefit in two ways if another adopts: i) she can learn about the technology, a *knowledge externality* and ii) she can face a lower infection probability, a *contagion externality*. My approach develops in four steps. First, I measure the value farmers attach to adopting pest-control technologies, and establish that pest-control technologies are desired. Second, I measure the value a farmer attaches to another farmer adopting pest control technologies, and document that farmers anticipate positive spillovers. Third, I show that farmers are responsive to changes in positive externalities. I generate random variation in a farmer's beliefs over knowledge and contagion externalities and find changes in beliefs affect the value she assigns to others' adoption. Finally, I estimate that the private benefit of technology adoption is lower than the cost of the technology, but the social benefit is larger than the cost. My results support the view that local economies may be caught in a low-adoption equilibrium which hinders agricultural productivity growth.

A Multifaceted Education Program for the Poor and Talented

I study the impacts of the MasterCard Foundation Scholars Program - a support program for pupils entering secondary school in Uganda. The program provides school fees, placement into a top-100 secondary education institution in the country, school inputs, a teacher mentor and a stipend. The program targets high-achieving students from a disadvantaged economic background. Exploiting the randomized assignment of eligible students to the program, I document program impacts on both individual recipients and their households. I find that the program has large positive effects on enrollment and the test scores of recipients. The program is also beneficial to a recipient's household as consumption, assets, psychological wellbeing and nutrition increase; the impact on siblings is ambiguous. Overall, the two-year impacts of the program are in line with anti-poverty interventions that provide a similar monetary value to recipients.

Moral Hazard: Experimental Evidence from Tenancy Contracts

Agricultural productivity is particularly low in developing countries. Output sharing rules that make farmers less-than-full residual claimants are seen as a potentially important driver of low agricultural productivity. We report results from a field experiment designed to *estimate* and *understand* the effects of sharecropping contracts on agricultural input choices, risk-taking, and output. The experiment induced variation in the terms of sharecropping contracts. After agreeing to pay 50% of their output to the landlord, tenants were randomized into three groups: (i) some kept 50\% of their output; (ii) others kept 75%; (iii) others kept 50% of output and received a lump sum payment at the end of their contract, either fixed or stochastic. We find that tenants with higher output shares utilized more inputs, cultivated riskier crops, and produced 60% more output relative to control. Income or risk exposure have at most a small effect on farm output; the increase in output should be interpreted as an incentive effect of the output sharing rule.

Keywords: *field experiments, randomized evaluations, agriculture, education, externalities, technology adoption, financial aid, cash transfers, school inputs, moral hazard, sharecropping.*

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Doctoral dissertation
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Abstracts

Quantifying Externalities in Technology Adoption: Experimental Evidence from Ugandan Farmers

I investigate how positive externalities contribute to underadoption of agricultural technology among sub-Saharan African farmers. Understanding whether and which types of externalities affect adoption is important both to shed light on human behavior and to formulate optimal policies regarding subsidies and targeting. I focus on the adoption of pest-control technologies. A farmer can benefit in two ways if another adopts: i) she can learn about the technology, a *knowledge externality* and ii) she can face a lower infection probability, a *contagion externality*. My approach develops in four steps. First, I measure the value farmers attach to adopting pest-control technologies, and establish that pest-control technologies are desired. Second, I measure the value a farmer attaches to another farmer adopting pest control technologies, and document that farmers anticipate positive spillovers. Third, I show that farmers are responsive to changes in positive externalities. I generate random variation in a farmer's beliefs over knowledge and contagion externalities and find changes in beliefs affect the value she assigns to others' adoption. Finally, I estimate that the private benefit of technology adoption is lower than the cost of the technology, but the social benefit is larger than the cost. My results support the view that local economies may be caught in a low-adoption equilibrium which hinders agricultural productivity growth.

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gram is also beneficial to a recipients household as consumption, assets, psychological wellbeing and nutrition increase. The impact on siblings' education is ambiguous; younger siblings experience no change, while older siblings experience an improvement in school quality. Therefore, program benefits are not equally distributed among household members, potentially affecting within-household inequality. Overall, the two-year impacts of the program are in line with anti-poverty interventions that provide a similar monetary value to recipients.

Moral Hazard: Experimental Evidence from Tenancy Contracts

Agricultural productivity is particularly low in developing countries. Output sharing rules that make farmers less-than-full residual claimants are seen as a potentially important driver of low agricultural productivity. We report results from a field experiment designed to *estimate* and *understand* the effects of sharecropping contracts on agricultural input choices, risk-taking, and output. The experiment induced variation in the terms of sharecropping contracts. After agreeing to pay 50% of their output to the landlord, tenants were randomized into three groups: (i) some kept 50% of their output; (ii) others kept 75%; (iii) others kept 50% of output and received a lump sum payment at the end of their contract, either fixed or stochastic. We find that tenants with higher output shares utilized more inputs, cultivated riskier crops, and produced 60% more output relative to control. Income or risk exposure have at most a small effect on farm output; the increase in output should be interpreted as an incentive effect of the output sharing rule.

To Giulia, Maria Pia and Riccardo

Acknowledgments

I owe all I have to the generosity of the people I have been fortunate enough to meet along the way.

In the spring of 2012 when I was Andreas Madestam's research assistant at Bocconi, he suggested I apply to a position within an organization I had never heard of. Selim Gulesci, also a professor at Bocconi, and his coauthors Konrad Burchardi and Munshi Sulaiman were looking to hire someone to run their project with Brac Uganda, based in Kampala. I was equal parts starstruck and terrified when I got the job, and fast forwarding two years, I grew in ways I could never have anticipated while I was there. It was a privilege to learn how to run large field research projects from Munshi. I also met the colleagues whose work is the backbone of this thesis during my time in Uganda: Esau Tugume, Apollo Tumusiime, Sunday Dismus, Winnie Aol, amongst other researchers at Brac Uganda and Metajua.

The fact that Konrad has been my *de facto* supervisor for the past 8 years starting in Uganda before I even entered the PhD is in itself a testament to his patience. I owe him more than I will ever be able to give back. He is not only an econ rockstar, he is also extremely supportive, generous and caring. As his thinking is clear and structured, I felt continually compelled to attempt the same when talking to him. I will miss his signature response to my not-well-thought-through arguments - ”no-no-no-no-no, Benni”.

Jon de Quidt has been a role model for me since he joined the IIES. I have often suspected he was sent from an advanced civilization able to bend the laws of physics, because he seems to accomplish tasks in negative time. He is an impressively quick thinker, knows all the literature, and always finds the time to graciously engage with colleagues. He has been there for me through highs and lows, providing clever solutions, words of encouragement and a listening ear every time I hit a wall.

My journey at the IIES started with an email from Jakob Svensson, inviting me to join the institute as a research assistant. I was excited both for the opportunity and because he knew my name as I had been a fan of his since reading his paper on local capture of central government transfers in Uganda. One of the most precious things I have learned from Jakob is to see an opportunity where others would see a flaw. An unexpected development in a research project can lead to a more interesting finding and open up a broader conversation.

I felt greatly supported by faculty at the IIES during the PhD as well as on the job market. Mitch and Tessa helped me navigate the market and checked in with me to see how I was doing during my most stressful times. David, Ingvild, Peter and Yimei sat with me through slide revisions and mock interviews. Arash, John, Kurt, Per, Timo and Torsten provided much appreciated feedback and advice throughout.

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The brilliant students in the African chapter of the development group have taught me that everything is possible. I am looking forward to reading the great work that Francesco, Marijo, Mattia, Stefano and Tillmann will produce. The fellow market warriors were essential to maintain sanity: Has, Karin, Kasper, Markus, Richard and Selene. Regarding Richard, I am concerned I managed to extract more utility from him than he did from me during the two years we shared offices (did I though?). It is also quite unclear if I will be able to make it to the office at my next job at all if I am not rewarded with a "look who is here!" remark upon each arrival.

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Arianna, Audinga, Eleonora and Francesca are amazing women. I feel so lucky that our paths crossed in Milan and Stockholm, and I am so proud of how strong our friendship has become. I grew up with Serena, Francesco, Luciano, Paolo, Cristiano and Angela and saw them become top students, then turn into brilliant professionals, and now formidable lockdown chefs. Florence and Ia taught me how to be a hero. I would not have survived my PhD without taking breaks to travel the world with Rachel, who is there 100% of the time regardless of the distance and the time difference. While Nacho, Shiro and Sirius White may not be able to read how essential their contribution has been to this thesis, an honourable mention is definitely in order.

I owe my most cherished memories of Sweden to my Stockholm family. Marina is a real-life example of what economists refer to as a ”benevolent social planner” - I expect her to rule at least a few countries in the near future. Andreas, Dario, Luca and Tracy are the most formidable partners in crime I could ask for.

Some say doing a PhD is the earthling equivalent of carrying the One Ring from the Shire to Mount Doom. Alex has been my Samwise Gamgee. The journey was fun with him by my side. He kept me alive at least a few times, and dared to hold my hand as we stare right into the boiling lava lake of the job market conferences (we can be quite a dramatic team at times).

Finally, my family is the best thing in my life. What a coincidence that the three most amazing people on the planet would end up being my mother, father and sister. Being able to share my life with you is the greatest gift.

Benedetta Lerva
May 2020
Stockholm

Contents

Introduction

This thesis consists of three chapters with two elements in common. The first is the general research question they attempt to answer: what drives investment decisions? In each chapter I explore a different factor affecting investment decisions: externalities, constraints, and contractual arrangements. The second common element is the methodology I use: randomized field experiments. Randomized field experiments are a helpful tool to establish causality in complex contexts with many unobservables when carefully designed and meticulously implemented.

In Chapter 1, titled *Quantifying Externalities in Technology Adoption: Experimental Evidence from Ugandan Farmers* I investigate how positive externalities affect the adoption of agricultural technology. Positive externalities occur when agents do not internalize the benefits of their actions have on others, resulting in an equilibrium adoption lower than socially optimal. The chapter connects the dots between positive externalities and low technology adoption in sub-Saharan Africa by showing that farmers are aware they benefit from others' adoption through positive externalities. The results allow me to characterize the ideal policy that harnesses externalities to improve technology adoption.

In the experiment, farmers are presented with the opportunity to purchase a pest control technology in the form of an agricultural training on maize pests. Farmers can either buy the training for themselves or for another specific farmer in their village, I measure farmers willingness-to-pay to buy the technology for themselves or for another farmer; a farmer's willlingness-to-pay for another farmer is a monetary measure of how much a farmer expects to benefit from the other farmer having the technology. To understand whether the benefit is due to positive externalities, I generate random variation in two types of externalities that the farmer would benefit from if the other gets the technology, and measure how much this affects their willingness-to-pay for another farmer. When a farmer adopts a pest control technology, others benefit from two types of externalities: contagion and knowledge externalities. Contagion externalities occur when a farmer reduces the pest load in her environment and others benefit from a lower risk on pest infection. Knowledge externalities occur when others learn about the technology by talking to or observing the adopter.

First, I find that pest-control technologies are desired, as farmers are willing to pay to receive the training. Second, farmers are willing to pay for others to receive the training; this means that they anticipate positive spillovers from others adoption. Third, I find that farmers respond to contagion externalities. When positive contagion externalities from another specific farmer are lower, farmers decrease their willingness-to-pay for the other farmer. My experiment does not provide conclusive evidence over knowledge externalities, but I provide suggestive evidence that farmers attach more values to training farmers they are more likely to learn from. Fourth, I use valuation data and a simple model to calculate the private and social benefits of adoption. I find that the social benefit is substantially higher than the private benefit, but the social benefit of adoption is heterogeneous across farmers. Hence, the optimal policy is nuanced. Because of large externalities, it is optimal to train some farmers; since externalities are heterogeneous and subsidies are costly, policy makers should target farmers who produce the largest positive externalities. The ideal targets are farmers who are both socially central and whose plots are geographically central.

In the second chapter, titled *A Multifaceted Education Program for the Poor and Talented*, I study the MasterCard Foundation Scholars Program, a large scholarship program for children entering secondary school in Uganda. The most efficient way to allocate educational resources is to target students that are very poor and very talented. Poorer households, though, may face several constraints to investment in education that need to be addressed simultaneously. The MasterCard Foundation Scholars Program equips talented poor students with the resources needed to attend secondary school; it couples efficient program targeting with the intention to remove all constraints to investment in education at the same time. The MasterCard Foundation Scholars Program is innovative from a program design perspective because it includes components of education and anti-poverty interventions that have not been implemented altogether before within the same program, but proved individually effective.

I exploit the randomized assignment of eligible students to a treatment or a control group during the beneficiary selection process to study the impacts of the program. I focus on the short term (2-year) effects on the schooling outcomes of the beneficiaries and the socioeconomic wellbeing of their households.

I find that the MasterCard Foundation Scholars Program increases both enrollment and the test scores of its beneficiaries. The impact on test scores is larger than most other similar education programs. The program impacts the economic lives of other family members besides the direct beneficiary. Two years after enrolling in the program, households accumulate productive assets such as land, durables, small animals and monetary savings. Nutrition improves in terms of dietary diversity, number of meals and protein consumption. The mental health of respondents improves as measured by reduced depression and increased life satisfaction and happiness. These effects are comparable in size with unconditional cash transfers or out-of-poverty-graduation programs implemented in other developing countries. My findings show that one intervention combining education and antipoverty measures performs similarly to interventions focused on either alleviating poverty or improving education. This suggests that the two policy objectives are not in competition, but complement each other.

In the third chapter, titled *Moral Hazard: Experimental Evidence from Tenancy Contracts*, coauthored with Konrad Burchardi, Selim Gulesci and Munshi Sulaiman, we study the impact of sharecropping contracts on agricultural input choices, risk-taking, and output. Sharecropping contracts require a tenant to pay a share of their output to the landowner. Economists have long believed that such contracts induce inefficient behavior by the agent as long as the agent is less than full residual claimant. It is challenging to investigate this claim with observational data. First, because unobservable characteristics may influence both contractual choices and farmers investment decisions. Second, because a larger tenant share induces an incentive effect, an income effect and higher risk exposure, which are difficult to separate in a non-experimental setting.

We carry out an experiment that induces variation in real-life tenancy contracts. Our implementation partner acts as landowner in selected villages in Uganda, offering farmers a 50% sharecropping contract. After signing the contract, villages were randomized into three groups. In the first group (C), tenants received 50% of output. In the second group (T1), tenants were offered to keep 75% of the output. This variation is key for estimating the incentive effect of the sharecropping contract. Tenants in a third group (T2) kept the same output share as in control (50%) but received an additional fixed payment which was independent of their output level. The comparison of T2 with C allows us to test for the presence of an income effect on agricultural productivity. Within the third group, half of the tenants (T2A) received the fixed payment as a risk-free cash transfer while the other half received part of their additional payment as a lottery (T2B), the expected payment in T2A and T2B being the same. The comparison of T2B with T2A allows us to test for the presence of a risk exposure effect.

With respect to output, we find that the fields of tenants with 75% output share generated higher agricultural output than tenants with a 50% share (T1 vs. C). We do not find that tenants who received a higher income produced significantly more (or less) output compared to tenants in the control group (T2 vs. C). Regarding input choices, we find that the tenants who retained a higher share of their output (T1) invested more in capital inputs to cultivate their plots (in terms of fertilizer, agricultural tools and unpaid labor). In contrast, tenants who received higher income (T2) did not invest more in capital or labor inputs relative to the control group. Regarding risk-taking, we find evidence of higher risk-taking amongst tenants with a higher output share (T1), mildly higher risk-taking amongst tenants who receive a risk-free income (T2A), and mildly lower risk-taking amongst tenants who received the risky income transfer (T2B), all relative to control (C).

Quantifying Externalities in Technology Adoption

Experimental Evidence from Ugandan Farmers

I. Introduction

Agricultural technology adoption is strikingly low in developing countries, causing agricultural productivity to stagnate (World Bank, 2007). One factor that can generate low adoption in equilibrium is the presence of positive externalities - if agents do not internalize the social benefit of their actions, the equilibrium adoption rate will be lower than what would be socially optimal (Pigou, 1920). Are farmers aware of these externalities, do externalities affect their adoption decisions, and can externalities explain the low technology adoption rates in developing countries?

I study these questions in the context of the Fall Armyworm infestation in Africa. This maize pest was first detected in Nigeria in 2016 and reached Uganda, the setting of this study, in early 2017, causing harvest losses of about 1% of Ugandan GDP in 2017 alone.[1] The Fall Armyworm infestation provides a useful context to study how policy makers can foster adaptation to changing environmental conditions and encourage mitigation practices. My approach unfolds in five steps.

First, I measure how much a farmer values receiving a training service on pest-

[1] www.fao.org

control technologies, conditional on no one else receiving training in that village. I do so by eliciting a farmer's willingness-to-pay to receive the training using an incentive-compatible mechanism. I find that the median value of receiving the training is about $17 at purchasing power parity, or the local wage earned in four days of agricultural labor. This is important because it shows that pest-control technologies are desired.

Second, I measure how much the farmers value that others in their village receive the training. I do so by eliciting a farmer's willingness-to-pay for another farmer to be trained, conditional on no one else receiving training in that village. I find that the median value a farmer attaches to another farmer receiving the training is about $9 at purchasing power parity, or the local wage earned in two days of agricultural labor. This is more than half the median value farmers attach to receiving the training themselves, and it implies that farmers anticipate positive spillovers from others.

Third, I identify two types of externalities. I carry out an intervention that allows me to introduce random variation in farmers' beliefs over the size of each type of externality. The design of the intervention allows me to investigate how farmers' willingness-to-pay for others responds to exogenous changes in beliefs regarding the two types of externalities. The first type of externality I study is a *contagion externality* - when a farmer controls the pest population in her farm and others benefit through a lower probability of infection. Here, contagion externalities are a function of distance between plots - the closer the distance, the higher the contagion externalities (Li et al., 2019; Pannuti et al., 2016). I randomize whether or not a farmer receives truthful information about the distance between her plot and the plot of another farmer before she formulates her valuation for the other farmer to be trained. If provided, the distance information causes a farmer to update her beliefs over the perceived contagion externalities between herself and the other farmer. I find that participants respond to contagion externalities. When they learn that contagion externalities with a specific farmer are lower than they expected, they decrease their valuation for that farmer to receive the training. This is important because my findings indicate that the policy maker should keep farmers' spatial distribution into account when targeting training services - targeting farmers that have central plots will increase the social benefits of the adoption of the technology.

The second type of externality I study is a *knowledge externality* - occurring when valuable knowledge spills over from a farmer who receives training to others in the village. When a farmer learns about a technology or adopts it, others can acquire that knowledge at low cost by talking to the farmer or observing her implement the pest-control practices. In my setting, knowledge externalities are a function of how much farmers interact - the more they interact, the likelier it is that knowledge is transferred. I generate variation in how much farmers interact by organizing a meeting between a farmer and another randomly selected farmer. The farmer is told about the meeting right before she formulates her valuation for the other farmer to be trained. This causes a farmer to update her beliefs over the knowledge externality between herself and the other farmer. While the belief update intervention does not indicate large knowledge externalities on average, I provide suggestive evidence that participants place a higher value on training farmers that are more likely to pass knowledge on to them. This is important, because it lends some evidence to policies that target socially central individuals.

Fourth, I use valuation data and a simple model to calculate the private and social benefits of adoption. I find that in my most conservative estimates, social benefit is 30 times higher than the private benefit and 18 times higher than the service cost. I conclude that in this context subsidies could be an effective policy option to foster technology adoption, and in light of my results, they should be targeted towards socially central farmers with centrally located plots.

Fifth, I exploit the random assignment of the training service to one farmer per village to investigate the effects of training on technology adoption. I find that adoption of pest-control technologies increases both among trained and non-trained farmers in the village. This is important because it shows that technologies spill over onto other farmers.

My analysis relies on the measurement of positive externalities in monetary terms. I collect the valuations of 6 to 9 sampled farmers in 103 villages in Eastern Uganda. Farmers are asked to form valuations for one person in the village to receive training on pest management techniques for the Fall Armyworm. Because the pest is new to the continent and both its biology and behavior is different from local pests, practices to handle the Fall Armyworm are a new technology. A sampled farmer formulates one valuation for herself and one for every other sampled farmer in her village, under the assumption that only one of them could be trained. As there are 6 to 9 sampled farmers in each village, each participant formulates five to eight valuations for others (one for each other sampled farmer), and one for themselves. The elicitation technique I use is a variant of the BDM mechanism (Becker et al., 1964) and incentive-compatible. The BDM mechanism requires that a participant formulates her maximum willingness-to-pay for an item. The researcher then draws a random price. If the willingness-to-pay is equal to or larger than the random price, the participant has to buy the item paying the random price. If the willingness-to-pay is lower than the random price, the participant cannot buy the item. Just like in a second price auction, it is in the best interest of the participant to state her maximum willingness-to-pay truthfully. My elicitation exercise is similar to the one performed by Berry et al. (2019), who implemented the BDM mechanism in the field to estimate willingness-to-pay for a household water filter in Ghana. They also compared BDM to take-it-or-leave-it (TIOLI) offers experimentally, and find that the two techniques perform similarly in terms of demand estimation.

Before the elicitation, participants are informed that a maximum of one of them could be trained. We explain to participants that one potential trainee will be selected randomly in two steps. First, one of the sampled farmers will be randomly drawn to be the decision maker; only the valuations made by the decision maker matter ultimately. Then, one of the valuations made by the decision maker will be randomly drawn. If that valuation is at least as large as the random price, the decision maker will buy the training for the farmer the valuation was made for, which may be herself. If not, the decision maker cannot buy the training and no farmer will be trained in that village.

In order to understand the importance of different types of externalities, I generate random variation in contagion and knowledge externalities *within-participant*, at the *valuation level* . For each farmer, I assign each of her five to eight valuations for others to one of three experimental conditions. In the control condition, I only elicit a farmer's valuation for another farmer to be trained. In the first treatment condition, I allow the farmer to update her beliefs over the contagion externalities between her plot and another farmer's plot by providing truthful distance information, and then elicit her valuation for that farmer. In the second treatment condition, I allow the farmer to update her beliefs over the knowledge externalities between her and another farmer by promising to organize a meeting with the other farmer, and then elicit her valuation for that farmer. This implies that each participant is exposed to both a control and two different treatment conditions. Of the five to eight valuations for others that each farmer formulates, a maximum of two are assigned to each of the two treatment conditions, and the remaining one to four are assigned to control.

Five features of this experimental design are key to quantify total externalities and identify the mechanisms at play. First, the incentive-compatible elicitation technique I use ensures that valuations are truthful. In addition, valuations obtained with this technique are equivalent to revealed preferences as they have real stakes. Second, informing participants that only one randomly selected farmer can receive the training ensures each valuation is formulated independently. This mechanism, a random lottery incentive, allows me to measure the willingness-to-pay of a participant to change one farmer's actions at a time, *ceteris paribus*. It does so by ruling out complementarities or substitutabilities in demand that would arise if more than one farmer could be trained or the trainee would not be selected randomly.[2] Third, I use a within-participant design. This allows for improved precision in the estimates and an efficient use of resources for data collection. Fourth, the belief update interventions I carry out aim at affecting *perceived* externalities, which are ultimately what matters when participants formulate their valuations.

[2]Complementarities or substitutabilities in demand are central concepts in the study of externalities, and a key element to guide policies such as optimal subsidies. Gautam (2018) and Guiteras et al. (2019) provide recent evidence on the impact of providing subsidized access to sanitation, a technology with high contagion externalities.

Fifth, the random selection of one person to receive the training in a village, conditional on the willingness-to-pay of the decision maker, means that training is randomly assigned at the village level. This allows me to evaluate experimentally the impact of training on technology adoption of the training recipient and the other farmers.

My paper mainly contributes to the empirical literature on the effects of externalities on technology adoption. Miguel and Kremer (2004) study the impact of a deworming program for school children in Kenya, and find that contagion externalities affect take-up. Individual take-up decreases in the share of nearby schools that are offered a free deworming program, conditional on the number of schools. My work builds on their findings and brings forward evidence of the decision-making process of the adopter. As Miguel and Kremer (2004) establish that contagion externalities affect take-up behavior, the objective of my work is to show that individuals are aware of these externalities to the extent of attaching monetary value to them, and their decision-making process also involves weighing how the identity of the adopter will affect their own final outcomes. Summaries of the medical and epidemiological literature on contagion externalities and take-up behavior can be found in Boulier et al. (2007) and White (forthcoming).

Knowledge externalities have been studied in the context of social learning, a term used to indicate that individuals learn from each other. Foster and Rosenzweig (1995) is a seminal study on social learning in the context of agriculture, and finds that farmers do not fully incorporate the positive effects of their learning on others' learning when making adoption decisions. Their results are based on testing the implications of a model that incorporates learning by doing and learning from others on household-level data on rice farmers in India. Conley and Udry (2010) test a learning model on survey data from pineapple farmers in Ghana, using staggered planting as identifying variation. I contribute to these and other studies on social learning (Bandiera and Rasul, 2006; Munshi, 2004) by investigating knowledge spillovers in an experimental context that allows me to measure their monetary value and separate knowledge spillovers from contagion externalities. My findings, that farmers anticipate learning from others and attach positive monetary value to it, are in line with these studies. Similarly, BenYishay and Mobarak (2018) and Vasilaky and Leonard (2018) find that training farmers who are similar to the average farmer - in terms of land holdings, cropping patterns, social standing and other characteristics - increases technology adoption more than training wealthier farmers. But the literature also provides examples in which social learning does not seem to take place. Duflo et al. (2011) find little impact of learning on fertilizer adoption; contact farmer systems studied by Kondylis et al. (2017) in Mozambique, in which the extension agent interacts with a contact farmer who passes on knowledge to other farmers, have showed limited impacts on the behavior of other farmers. Recently Caeiro (2019) found that knowledge diffuses through social networks and weak ties play a major role in the diffusion, but networks have limited impacts on adoption behavior. One of the factors that slows down social learning, and could explain why social learning patterns are not consistently detected in the literature, is heterogeneity in farming conditions. Tjernström (2017) finds that soil quality heterogeneity is an obstacle to learning from others among Kenyan farmers.

More broadly this paper contributes to the literature that seeks to understand the puzzle of underadoption in developing countries. Many factors contribute to low adoption and have been explored by the literature. Bold et al. (2017) provide evidence of input market failures, showing that the low quality of seeds and fertilizer available on the market decreases both returns to technology and adoption. Karlan et al. (2014) investigate the role of uninsured risk and liquidity constraints in depressing farm investment, and find that providing insurance increases investment more than cash grants. Duflo et al. (2011) show that behavioral biases induce Kenyan farmers to procrastinate the purchase of fertilizer and miss profitable fertilizer investment opportunities. In the context of the underadoption puzzle, this paper contributes to the literature that connects the dots between lack of access to high-quality and reliable information about agricultural technologies, missed opportunities of high-return investments, and ultimately low agricultural productivity.

The fact that information is a non-rival good, expensive to produce and cheap to reproduce, has led governments to step in and provide information on agricultural technology at no cost to farmers. Uganda, the country where this study is set, established its first nationwide extension service - the *Unified Extension System* - in 1990, and currently has several government- and donor-funded extension services operating in parallel (Barungi et al., 2016). The most common approach in Uganda is Training & Visit (T & V), a model that has been criticized for being financially unsustainable (Gautam, 2000), as the service is expensive to provide and causes at best modest improvements in technology adoption. Programs that leverage on Information Technology to spread extension messages can instead be cost-effective when SMS and phone-based information exchange platforms are cheap to provide, even though impacts are limited (Fabregas et al., 2017; Cole and Fernando, 2012). My project contributes to this literature by showing that farmers value extension services by carefully measuring farmers' willingness-to-pay for them. This feature also allows me to back out demand at different price points, that could be valuable for policy makers who want to know whether demand for certain information services exists before committing public funds. In addition, because of the random variation in training provision, I can add to the evidence on the effectiveness of training programs on technology adoption.

This study also adds to a small but growing literature on adaptation to changing environmental conditions (here due to the Fall Armyworm) - a topic of central concern in many countries as a response to global warming (Dell et al., 2014; Fishman et al., 2015)

Finally, this article contributes to the literature on mechanism design that exploits truthful reporting mechanisms to target development interventions. Both Rigol and Roth (2016) and Hussam et al. (2018) study the feasibility of truth-telling mechanisms in the field. The latter shows that such mechanisms can deliver truthful peer reports on who would be the most successful entrepreneur in a community. Similarly, I rely on an incentive-compatible mechanism to identify those farmers whose adoption decisions generates the highest social benefit.

In what follows I provide some background information (Section II), a theoretical framework (Section III), details on data and field experiment (Section IV and

V), and results (Section VI). Section VII concludes.

II. Setting

The Fall Armyworm (*Spodoptera Frugiperda*) is originally from the Americas and feeds on several crops, but in Africa, where it appeared for the first time in 2016, it has mainly affected maize. This study takes place in communities of smallholder maize farmers in Uganda, one of the countries that has been affected by the pest most severely. According to FAO, the Fall Armyworm destroyed an estimated 450,000 metric tonnes of maize during the first cropping season of 2017 in Uganda (equivalent to $192 million) and the crop loss affected 3.6 million people directly (9% of the population). Maize is the first cereal and second most cultivated crop after banana and beans in the country. It is one of 9 priority crops for the Government (ASDP 2015/16-2019/20) and the Eastern region, where this study is set, produces over half of the total maize produced in Uganda every year (UBOS, 2010). Among both international and local organizations working in agriculture, tackling the Fall Armyworm infestation is considered a policy priority.

The length of the life cycle of Fall Armyworm depends on climate. In tropical climates the life cycle is shorter than elsewhere and lasts about one month. This means that the pest reproduces faster and is thus more invasive and difficult to contain. Given that local maize cultivars have a life cycle of 110 to 120 days, the pest goes through four life cycles during a maize plant's life cycle. The pest has five development stages: egg, larva, caterpillar, pupa and moth. It spreads geographically mainly during the moth stage, as females fly during the night to lay eggs.

Practices to handle Fall Armyworm are pest-specific but not dramatically different from practices regarding other pest types, making it plausible that insights from my study generalize to pest management in general. As an example, farmers are instructed to scout the field every three days because Fall Armyworm eggs hatch every two to three days; farmers are taught to spray insecticide into the maize whorl because it is the preferred hiding place of grown caterpillars during the day. But learning how to scout the field efficiently applies to other maize pests, and knowing that pesticide should be sprayed where the pest hides (rather than on the whole plant) is a cost-saving technique that farmers can use with other pests and diseases.

Prior to the Fall Armyworm infestation there were already indications that some farmers may not have been following local extension officers recommended practices to handle pests. Further, during the infestation farmers reported adopting practices that had not been recommended by the government, such as spraying plants with laundry detergent or paraffin to kill the pest. Qualitative work suggests that farmers had had seldom contact with government extension officers in the research area, and that this contact was almost exclusively linked to seeds distribution.

III. Conceptual Framework

This section develops a simple model to highlight the parameters that determine willingness-to-pay for self and for others. A farmer i chooses whether to take or not a binary action $l_i \in \{0, 1\}$ to maximize net utility of avoiding a pest infestation. In an economy with no externalities farmer i solves:

$$\arg\max_{l_i} \Pi_1 = (U_i - c - k_i) l_i \tag{1}$$

Where U_i is the utility of avoiding infection, c is the cost of acquiring the knowledge-based technology and k_i is the individual cost of implementing the recommended pest-management techniques. Information and contagion externalities allow i to learn from other farmers and face lower exposure to infestation. All farmers in the village take adoption decisions simultaneously.

The parameter governing *knowledge* externalities is p^L, the probability that i learns from at least another farmer in the village,

$$p^L = 1 - \sum_{j \neq i} (1 - p^l_{ij}) l_j \tag{2}$$

where p^l_{ij} is the probability that i learns from j. The utility value of the knowledge externality is measured by $(U_i - k_i)p^L$, the product of the net utility of avoidance times the probability of learning.

The parameter governing *contagion* externalities is $(1 - p^T)$, the probability that i is not infected by any other farmer j,

$$(1 - p^T) = \prod_{j \neq i} (1 - p^t_{ij}(1 - l_j)) \tag{3}$$

where p^t_{ij} is the reduced-form probability that i is infected by j, and trivially (1-p^t_{ij}) is the probability that i is not infected by j. Every adopting farmer j generates a positive contagion externality for i by reducing the pest load of the environment and increasing the probability that i avoids infection (or equivalently, decreasing the probability that i is infected). The utility value of the contagion externality is measured by $U_i(1 - p^T)$. It is the product of avoidance utility U_i times the probability that farmer i does not get infected by any other farmer j. If $l_j = 0$, i avoids infection from j with probability $(1 - p^t_{ij})$. If $l_j = 1$, i avoids infection from j with probability 1.

I make the two following assumptions. First, the technology works perfectly.

A1: If a farmer implements the technology, she is immune from the pest.

Second, once a farmer has learned the technology from another farmer, the utility from implementing the technology is higher than the utility from contagion externalities.

A2: The utility of implementing the technology is larger than the utility of contagion externalities

$$U_i - k_i \geq U_i(1 - p^T)$$

Trivially, if no $j \neq i$ farmer adopts the technology in the village, $\sum_{j \neq i} l_j = 0$, there are no externalities and farmer i's problem is just (1). If at least another farmer in the village chooses to adopt, such that $\sum_{j \neq i} l_j \geq 1$, farmer i's problem is

$$\max_{l_i} \Pi_2 = l_i(U_i - c - k_i) + (1 - l_i)[(U_i - k_i)p^L + (1 - p^L)U_i(1 - p^T)] \tag{4}$$

In the experiment, the setting above is modified in the following ways:

1. There is a set of farmers $F = (i, j, \ldots, N)$
2. A maximum of 1 farmer can acquire the technology.
3. A participant i is either in a state of the world in which only i can acquire the technology ($l_{-i} = 0$), or in a state of the world in which only j can acquire it ($l_{-j} = 0$),
4. A participant i decides whether to acquire technology for herself in the state of the world $l_{-i} = 0$; she decides whether to acquire technology for j in the state of the world $l_{-j} = 0$,
5. A participant chooses her maximum willingness-to-pay for herself to adopt, w_{ii}, in the state of the world $l_{-i} = 0$, or chooses her maximum willingness-to-pay for j to adopt, w_{ij}, in the state of the world $l_{-j} = 0$.
6. There is no cost to acquiring the technology, $c = 0$

Then equations 1 and 4 become

$$\max_{w_{ii}} \Pi_1 = U_i - k_i, \quad \text{if } l_{-i} = 0 \tag{5}$$

$$\max_{w_{ij}} \Pi_2 = (U_i - k_i)p_{ij}^l + (1 - p_{ij}^l)U_i(1 - p^T) \text{ if } l_{-j} = 0 \tag{6}$$

Suppose the state of the world is $l_{-i} = 0$. Then a rational farmer will set her w_{ii} equal to the difference between the profits she would obtain if she chooses to adopt, $\Pi_1(l_i = 1|l_j = 0)$, and the profits she would obtain if she chooses not to adopt, $\Pi_1(l_i = 0|l_j = 0)$,

$$w_{ii} = \Pi_1(l_i = 1|l_{-i} = 0) - \Pi_1(l_i = 0|l_{-i} = 0) = U_i - k_i \tag{7}$$

Suppose the state of the world is $l_{-j} = 0$. Then a rational farmer will set her w_{ij} equal to the difference in profits if she chooses that j should adopt versus not adopt.

$$\begin{aligned} w_{ij} &= \Pi_2(l_j = 1|l_{-j} = 0) - \Pi_2(l_j = 0|l_{-j} = 0) \\ &= (U_i - k_i)p^l_{ij} + (1 - p^l_{ij})U_i[\prod_{m \neq i,j}(1 - p^t_{im}) - (1 - p^t_{ij})\prod_{m \neq i,j}(1 - p^t_{im})] \\ &= (U_i - k_i)p^l_{ij} + (1 - p^l_{ij})U_i\prod_{m \neq i,j}(1 - p^t_{im})p^t_{ij} \end{aligned} \tag{8}$$

In (8), $\prod_{m \neq i,j}(1 - p^t_{im})$ describes the background infection environment. Setting $\prod_{m \neq i,j}(1 - p^t_{im}) = E$ and substituting (7) into (8) yields

$$w_{ij} = w_{ii}p^l_{ij} + (1 - p^l_{ij})(w_{ii} + k_i)Ep^t_{ij} \tag{9}$$

Which shows that willingness-to-pay of i for j depends on the parameters governing knowledge and contagion externalities, respectively p^l_{ij} and p^t_{ij}, willingness-to-pay for self, an individual implementation cost k_i, and the background infection environment E.

IV. Data and Sample

The sample includes 799 farmers from 103 villages located in the districts of Manafa, Mbale and Tororo in Eastern Uganda. This sample was originally selected for an experimental project detailed in Burchardi et al. (2019). In that project we first chose which districts to target by ranking them according to their maize suitability indicator provided by the FAO GAEZ Dataset[3], and choosing contiguous districts for ease of implementation. Then, we selected villages using national census data[4]. We kept only villages with a population density lower than 100 households per square kilometer and randomly selected 103, stratifying by population density. In each village, we enlisted all residing households through a researcher-run census survey. Finally, we selected 6 to 9 household heads as participants according to the following criteria: *i)* engaging in commercial farming; *ii)* having cultivated or planning to cultivate maize; *iii)* reporting land holdings from 2 to 6 acres; *iv)* having a mobile money account. The participants in my study were enrolled at the end of 2016 and, as part of the joint project activities, had been exposed to survey interviews and one willingness-to-pay elicitation exercise.

The data I use come from the four surveys listed below. All surveys have been written by the research team and approved by the competent Ugandan ethics authorities. Surveys have been administered through tablets running the SurveyCTO software. The enumerators who administered the survey have at least a high school diploma, speak the Eastern local languages (Ateso, Japadhola, Luganda, Lugisho, Swahili) and have been recruited through Metajua Uganda Ltd, a research and software company based in Kampala.

Appendix B provides details about how I constructed each variable and in which survey one can find the raw data used for the construction.

- **In-person Baseline Survey** carried out in July-August 2018 as part of field activities for Burchardi et al. (2019) ;

- **Willingness-To-Pay Elicitation Survey** carried out in September 2018, including the BDM elicitation exercise and questions over a farmer's network. Questions used to elicit willingness-to-pay and administer the two belief shocks are available in Appendix ;

[3]www.gaez.fao.org

[4]Villages are commonly referred to as LC1 (Local Council 1) in Uganda, and they are the smallest administrative unit, governed by a chairperson (LC1 chairperson) and nine executive committee members.

- **Phone Followup Survey** carried out in December 2018 and including questions on learning and adoption;
- **In-person Followup Survey** carried out in July 2019 and including questions on learning, adoption, beliefs over Fall Armyworm infection function, beliefs over own and others' information sharing behavior.

The timeline of data collection is summarized in Figure I.

V. Experimental Design

V.A. Willingness-To-Pay Elicitation

Enumerators carried out home visits to elicit participants' willingness-to-pay for a three-hour training session about Fall Armyworm. Each participant formulates her willingness-to-pay to receive training herself, and also her willingness-to-pay for each other sampled farmer to receive the training. These valuations are elicited in a way that makes it clear that they are mutually exclusive events: only one farmer in the end can actually receive training.

The elicitation technique I employ is a multiple-price list variation of that proposed by Becker et al. (1964), in which a participant formulates a bid b for an item and the bid is compared with a random price draw p. If the bid is equal to or larger than the random price, the participant buys the item, otherwise she does not. This elicitation technique is incentive-compatible and has real stakes: it allows participants to actually purchase an item if $b \geq p$.

Before the exercise, participants are asked if they know what the Fall Armyworm is and informed that it spreads by proximity, so that the closer a plot is to pest-infected plants, the likelier it is that the pest spreads to the plot. Participants are also informed that one decision maker would be randomly selected in their village. The decision maker is the one who actually gets the opportunity to purchase the training, and participants do not know who the decision maker is before the elicitation. Participants are also informed that they will make N bids, choosing their willingness-to-pay for themselves and for other farmers to receive the training in that village, but only one of these N choices will be actually implemented if they are the decision maker. If they are not the decision maker, none of their choices will be implemented.

In the multiple-price list variant I employ, the participant is presented with 21 amounts, ranging from 0 to 40,000 UGX ($ 34.6) and increasing in steps of 2 000 UGX ($ 1.7): (0, 2 000, 4 000, ..., 40 000)[5]. At every step, the participant is asked if she would be willing to pay amount x to purchase the training. If the respondent accepts to pay x, she is asked if she would be willing to pay $x + 2,000$. If she does not accept to pay x, the enumerator stops and records her bid as $b = x - 2,000$. Note that negative prices and prices above 40,000 are not allowed in the experiment. There are no participants who refuse to pay 0, and if a participant is willing to pay 40,000 the enumerator records $b = 40,000$.

The enumerator follows this procedure to obtain a participant's willingness-to-pay for herself to receive the training, b_1, and then to obtain a participant's willingness-to-pay for each of the other sampled farmers in the village to receive the training, $b_2, b_3, \ldots, b_N$. After the respondent has made N willingness-to-pay bids, the enumerator gives her a sealed plastic card. The card contains a random price p drawn from a uniform distribution, $p \sim \mathcal{U}(0, 40\,000)$. The card also indicates if the participant is the decision maker or not. If the respondent is the decision maker, the card also indicates which of the N choices made binds, so which of the $b_1, \ldots, b_N$ bids matters. The enumerator does not know p or any other information included in the card. Suppose a participant is the decision maker and her card says that choice 3 is the one that binds. Then if her bid for farmer 3 is larger than the random price, $b_3 \geq p$, she buys the training for farmer 3 at price p. If $b_3 < p$, she does not buy the training. Note that if the decision maker does not manage to buy the training, no other farmer can purchase the training for anyone else in the village. All participants from the same village are interviewed over the course of one day to minimize the chance that the identity of the decision-maker becomes

[5]All US Dollar amounts are expressed in PPP terms. I calculate amounts in $PPP using the 2018 conversion rate of $1=1157.27 LCU (Local Currency Units), retrieved by the World Bank Databank. Numbers in $PPP are rounded to the first decimal digit.

known in the village before the elicitation exercise is completed.

Design Rationale of the Elicitation In the experiment I exploit a random-lottery design and perform three lotteries,

1. a *price lottery* that assigns a price $p \in \{0, 2\,000, \ldots, 40\,000\}$ to each participant;

2. a *choice lottery* that determines which of the N choices should be implemented;

3. a *decision-maker lottery* that determines which of the N participants in the village has the opportunity to buy the training for the choice selected in 2).

This design implies that a maximum of 1 farmer can receive the training in each village.

The *price* lottery ensures that a participant's valuation is truthful; the *choice* and *decision-maker* lotteries shut down strategic complementarity or substitutability channels between farmers. More specifically, assume it was possible for a participant to purchase training for two or more farmers. Then, when making a choice, the participant would have to consider elements such as the pros and cons of each n-person combination of farmers (unobservable to the econometrician), their joint decision processes, and how their efforts would complement or subtitute each other. Such considerations could distort a participant's valuations and add a layer of complexity that compromises the tractability of the problem for both the participant and the researcher. The same reasoning applies if the experiment allowed for two or more decision-makers. If another person's choice were to be implemented together with her own in a 2 decision-maker scenario, a participant would have to consider a much larger set of states of the world than if she was the only decision-maker, potentially distorting her choices to exploit the complementarity or substitutability with the other decision-maker's choices (or expectations over them). Obviously, if information is made available outside of a controlled experimental setting, more than one person per village can obtain it and demand complementarities or substitutabilities kick in. This makes the decision over the value of others' learning much more complex. While my experiment cannot speak to this by design, it also provides a scenario in which adoption is bounded from below and the valuation decision has high stakes. If none of the participants buys the training, no one in the village will adopt the techniques.

V.B. Belief Update Intervention

My willingness-to-pay elicitation survey included a belief update experiment to understand how concerns for *i)* information externalities and *ii)* contagion externalities affect a farmer's valuation of information held by others.

Information externalities between two farmers depend on the probability that the two farmers share agricultural information with each other p^L (I restrict my analysis to direct communication between farmers and exclude indirect communication that involves other members of the community). The enumerator caused the participant to update her belief over p^L by informing her that the research team would organize a meeting between the participant and that specific farmer after training had taken place. I call this the "meeting treatment".

Contagion externalities between two farmers depend on the physical distance between their plots, because the pest spreads by proximity. The enumerator caused the participant to update her belief over p^T_{ij} following a two-step procedure. First the enumerator asked the participant how long it would take to walk from her largest maize plot to the closest plot of a specific farmer j, d^P_{ij}. Then the enumerator informed the participant i of the true as-the-crow-flies walking distance between those plots, d^T_{ij}. I call this the "distance" treatment. Appendix C reports the exact wording used in the experiment. Figure VI shows the joint distribution of perceived distance d^P_{ij} and true distanced^T_{ij} in walking minutes. From the figure one can see that in most cases the true and perceived distances are within 20 walking minutes, and never above 100. This means that the plots considered lay within a distance of 5 miles or 8 kilometers from each other. Figure VII shows the distribution of the difference between true and perceived distance in the control group, $d_{ij}^T - d_{ij}^P$. The average is very close to zero , so farmers' beliefs are roughly correct on average. In addition, the distribution is well-behaved. This is encouraging, as it suggests that plot distance is something farmers are aware of. It also suggests that farmers are putting effort into answering plot distance questions, which is not a given as they were not incentivized.

Both types of belief update interventions are a within-individual experiment in the following way. The respondent forms N valuations, for herself and 5 to 8 other farmers. Among these, I selected a maximum of 2 for whom the enumerator would propose a meeting, a maximum of 2 for whom the enumerator revealed distance, and a maximum of 4 controls. Each participant was exposed to the meeting treatment, the distance treatment, and the control treatment.

Rationale of Belief Update Intervention The ideal experiment to study information externalities would consist in randomly adding a new node (person) to a participant's farmer network, where an identical node has either received training or not, and study how much a respondent values information held by the new node. A feasible experiment cannot exploit the "extensive" margin of the social network but can exploit the intensive margin by increasing the likelihood that the respondent interacts with another farmer. The meeting treatment tries to achieve exactly that. Ceteris paribus, I expect the meeting treatment to increase a participant's willingness-to-pay for the farmer she is going to meet.

For the meeting treatment to be effective, the participant needs to know that she is going to meet a farmer before formulating her willingness-to-pay for that farmer. Additionally, for the meeting treatment to be exogenous, whether the meeting happens needs to be independent from a participant's willingness-to-pay for that farmer. If the participant could reduce the meeting probability by reducing her willingness-to-pay for that farmer, treatment would be endogenous. Hence, before the elicitation, the enumerator informs the participant that the meeting will take place i) regardless of her willingness-to-pay; ii) if the participant is the decision-maker; iii) if that choice is selected in the choice lottery.

With respect to the distance treatment, the ideal experiment would consist in randomly adding a new node (plot) to a participant's plot network, where the new plot is at random distance. By comparing the valuation of participants for farmers with identical plots at a different random distance, one could gauge the importance of contagion spillovers. A feasible experiment can exclusively exploit the existing plot network, which is why my distance treatment reveals the true distance to randomly selected plots and compares valuations for farmers whose plot is surprisingly further versus surprisingly closer. Similarly to the meeting treatment, the participant needs to learn the true distance to a farmer's plot before formulating her willingness-to-pay for that farmer. An important caveat is that I revealed the true as-the-crow-flies distance between any two plots in the experiment - because I had baseline GPS coordinates of each farmer's plots together with the crops cultivated on each plot. I could not reveal the actual walking distance because I do not have data on rural paths that connect plots, but the as-the-crow-flies distance is the relevant information since the pest spreads by flying when it is in the moth stage.

V.B.1. Training and Researcher-Organized Meeting

One week after the elicitation exercise, a team of three agronomists visited the villages in which the decision-maker had managed to buy the training. In each village, one agronomist collected payment from the decision-maker and trained the farmer for whom training was bought. The agronomists were recruited for the project and had not had previous interactions with the participating farmers before this project. Training was carried out in 64 out of 103 experimental villages; all 64 farmers who committed to pay (5 for themselves and 59 for another farmer) did, in fact, pay. Each individual training session lasted about three hours and took place at a respondent's homestead. A training session included a theory module and a practice module. Agronomists taught the theory module with the aid of a color-printed leaflet (available upon request) that they developed together with the author. The leaflet contains basic information about how to recognize, prevent, monitor and control a Fall Armyworm infestation, and the trainee is encouraged to keep it for future reference. The practice module was typically taught in the respondent's closest maize plot. The cost of providing training to one farmer in the experiment is 34000 UGX or $ 29.4. This is to be interpreted as the marginal cost of training, and it is flat. If the government were to implement the training, marginal costs would most likely be lower (government wages are typically lower than wages paid in the experiment, and economies of scale are larger for governmental operations). Fixed costs of training are negligible in the experiment and would be so if a larger organization were to implement the training as well. The only fixed costs in my experiment consist in a 3-day training of the agronomists.

Once all training sessions had been carried out, the research team arranged meetings for 24 farmers. Meetings took place at a location agreed upon by both farmers, and a member of the research team attended the meeting acting as a facilitator.

VI. Results

VI.A. The Training Program is Beneficial

I start by providing evidence that pest-control technologies are desired. I do so by showing that receiving training increases adoption of pest-control practices. To this end, I exploit the fact that the elicitation exercise provides random variation in training provision at the village level.

Because of the *price*, *choice* and *decision-maker* lotteries, a decision-maker cannot affect the price that she pays for training, the identity of the recipient or the likelihood that she herself is the decision-maker. But her willingness-to-pay choice can affect the probability that a *choice-lottery* selected recipient z receives training. In other words, the higher the willingness-to-pay of a decision-maker for z, the more likely it is that that potential trainee z receives training. For this reason, whether training takes place in a village is a random event, conditional on the willingness-to-pay of the decision maker D for randomly selected potential trainee z. This allows me to employ the following empirical specification to evaluate the impact of training one farmer on the adoption levels of the other sampled farmers in the village:

$$\begin{aligned} Adoption_{iv} = \beta_0 + \beta_1\, Training_v \times Spillover\ Farmer_i \\ + \beta_2\, Training_v \times Trained\ Farmer_i + \sum_p \delta_p \mathbb{1}(w_{Dz,v} = p) + \varepsilon_{iv} \end{aligned} \quad (10)$$

The dependent variable is a dummy equal to 1 if farmer i in village v has adopted any of the following recommended practices to control Fall Armyworm: scouting the plot, crushing eggs or caterpillars, spraying pesticides, pouring ash, sand or soil, and applying fertilizer. $Training_v$ is a dummy equal to 1 if training has taken place in village v. $Trained\ Farmer_i$ is equal to 1 if i has received training. $Spillover\ Farmer_i$ is equal to 1 if $Trained\ Farmer_i$ is zero. Indicator $\mathbb{1}(w_{Dz,v} = p)$ is equal to 1 if the willingness-to-pay of decision-maker D for potential trainee z is equal to p, where $p \in \{0, 2\,000, \ldots, 40\,000\}$ [6].

Results from estimating 10 are presented in Table IV. The first column shows mid-season effects, about 2-months after we delivered training. The second and third columns show effects at the end of the season in which training took place (February 2019) and at the end of the following season (July 2019), respectively. The omitted comparison group in this case is that of spillover farmers in villages where training did not take place. Adoption increases by nearly 14% among spillover farmers in villages in which training took place, while it increases by nearly 15% among farmers who received training themselves. By the end of the season (Column 2), 96% of the farmers report adopting at least one of the recommended techniques in non-trained villages and treatment effects are not detectable anymore.

[6] The OLS estimator β_1 in this case is a weighted estimator, where weights are given by the conditional variance of the treatment variable. The conditional variance of $Training_v$ (conditional on each possible value of $w_{Dz,v}$) is highest when, within a bin of $w_{Dz,v}$, the share of treated and untreated is equal. So bins of $w_{Dz,v}$ in which everyone is treated or everyone is not treated are going to receive lower weights than bins in which treated and non treated have similar shares, and hence higher conditional variance. See Chapter 3 in Angrist and Pischke (2009).

I observe a similar pattern with respect to knowledge about FAW. In July 2019 I administer an incentivized knowledge test to all farmers in the sample. The test is carried out as part of the in-person follow-up survey and it consists of 15 multiple-choice questions about the FAW and how to prevent an infestation. After the respondent has answered all questions, one of them is randomly selected and if the answer is correct, the respondent is awarded a small prize (a pack of salt). Results are displayed in Figure IX. On average, respondents answer correctly 9 times out of 15, with no difference if they received training themselves (left bar, *Trained Farmer*), someone else in the village received training (center bar, *Spillover Farmer*), or no training took place in their village (right bar, *Non-trained Village*).

An important caveat to bear in mind with estimates in Table IV is that the training is randomized at the village level among 103 villages. Under standard assumptions, this sample size is not adequate to detect effects of the training service on adoption. Powering this part of my experiment was not a first order concern since adoption is not the main outcome of my analysis, but an identical program could fail to replicate these results.

The estimates reported in Table IV provide causal evidence that when one farmer learns about technology, others adopt it. This behavior could be triggered by several factors, including lower adoption costs due to a decrease in information barriers or larger value of adoption due to the decrease in pest population generated by adopters. Evidence of the effects of the training program, by itself, is not informative of whether farmers value neighbors' information. Investigating the value placed by farmers on a neighbor's information status, and understanding which factors affect it, is the objective of the next section.

VI.B. Farmers Value Training Others

After having established that training is beneficial to village adoption, the next step is to verify whether farmers are aware of the benefits of the training, both for themselves and for others. To do so, I collect farmers' valuations for themselves to receive the training, and for others to receive the training. I use an incentive-compatible mechanism to elicit willingness-to-pay for the training. Figure II shows the demand curves for the training of self (top panel) or another farmer (bottom panel). In both cases, demand for training is non-negligible. A useful benchmark to interpret these curves is the median agricultural daily laborer wage in local markets, UGX 5,000 or $ 4.3. In the top panel, more than 90% of farmers would buy the training if the price was UGX 5,000. In the bottom panel, more than 60% of the farmers would buy training for another farmer at the daily wage price.

In the whole sample, median willingness-to-pay for self w_{ii} is UGX 20,000 or $ 17.3, so about 4 days of work as a daily farm laborer. The median willingness-to-pay for another farmer w_{ij} is UGX 10,000 or $ 8.6, the equivalent of two days of work as a farm laborer. Figure III plots w_{ii} on the y-axis against w_{ij} on the x-axis, and the 45-degree line is added for reference. To provide the reader with a measure of the density of observations at each point, dots are jittered. As Figure III shows, nearly all farmers display $w_{ii} > w_{ij}$. This is in line with an implication of the main assumption of the model. In the model, a farmer values learning positively and always prefers to learn than benefit from contagion externalities, which imply that a farmer always prefers to learn for sure than benefit from externalities
The average value of the ratio $\frac{w_{ij}}{w_{ii}}$ is 58%, so a farmer values another farmer's learning half to two thirds of her own.

VI.C. Valuations are Affected by Externalities

This section first presents the empirical strategy adopted to investigate the impacts of the belief update intervention on willingness-to-pay for others, and then comments on results.
Here the outcome of interest is the value attached to a 3-hour traning session about Fall Armyworm with an extension worker, that I refer to as a farmer's willingness-to-pay for information. The willingness-to-pay of farmer i for farmer j is w_{ij} and can take 21 monetary values that are multiples of 2,000 Ugandan Shillings, $w_{ij} \in$

$\{0, 2\,000, 4\,000, \ldots, 40\,000\}$.

I conjecture that the distance treatment should decrease w_{ij} if the distance information I provide is *good news* for i, meaning that j's plot is surprisingly *further away* from i's plot. Conversely, the distance treatment should increase w_{ij} if I provide *bad news*, meaning that j's plot is suprisingly closer than expected. The rationale is that if j's plot is farther (closer) than i expected, the benefit accruing to i of j's adoption is lower (higher). The key metrics here are

$$\begin{aligned} \textit{Potential Good News}_{ij} &= |d_{ij}^T - d_{ij}^P| \text{ if } d_{ij}^T > d_{ij}^P \\ \textit{Good News}_{ij} &= \textit{Potential Good News}_{ij} \times T_{ij}^d \end{aligned}$$

when i learns that j is surprisingly further away, and

$$\begin{aligned} \textit{Potential Bad News}_{ij} &= |d_{ij}^T - d_{ij}^P| \text{ if } d_{ij}^T < d_{ij}^P \\ \textit{Bad News}_{ij} &= \textit{Potential Bad News}_{ij} \times T_{ij}^d \end{aligned}$$

when i learns that j is surprisingly closer. Variable d_{ij}^T is the true distance between i's largest plot and j's closest plot in walking minutes. Specifically, d_{ij}^T is the as-the-crow-flies distance in miles between the GPS coordinates of the centroids [7] of the two plots, converted to walking minutes by assuming that the average adult takes 20 minutes to walk one mile. The variable d_{ij}^P is the answer given by participant i to the question of how long it takes to walk from her largest plot to j's closest plot (exact question wording can be found in Appendix C). Variable T_{ij}^d is a dummy equal to 1 if participant i is told the true distance to farmer j's plot. The estimating equation for the distance treatment is:

$$\begin{aligned} w_{ij} = {} & \beta_1 \textit{Good News} + \beta_2 \textit{Bad News} + \\ & + \beta_3 \textit{Potential Good News} + \beta_4 \textit{Potential Bad News} + \alpha_i + \varepsilon_{ij} \end{aligned} \tag{11}$$

[7] If a plot of land is a two-dimensional plane, its centroid is the arithmetic mean position of all points of the plane.

The coefficients of interest are β_1, for which I expect a positive sign, and β_2, for which I expect a negative sign. The coefficient of β_1 (β_2) should be interpreted as the change in w_{ij} for every extra minute of larger (closer) distance to j that the farmer learns about. It is informative of the change in value that i attaches to j's information when j's plot is less (more) important in determining i's infection status. Specification 11 includes fixed effects α_i for participant i.

The distance treatment I implement allows me to estimate the local average treatment effect (LATE) of an change in i's perceived plot distance between farmer i and j and i's willingness-to-pay for j to receive training. The size of the treatment, which is the difference between true and perceived distance in absolute value $|d_{ij}^T - d_{ij}^P|$, is a function of perceived distance d_{ij}^P. The more a farmer's perception d_{ij}^P is off, the larger the treatment she receives. This implies that there is self-selection in the treatment and I can only estimate the effect on those who have wrong beliefs.

The purpose of the meeting treatment is to ceteris paribus increase the perceived probability that i learns the information from j. During the elicitation exercise, participant i is told that the research team is going to organize a meeting with j, to take place after j has received training. Participant i is also told that the meeting takes place regardless of whether j receives training or not, to keep the meeting treatment truly exogenous (if the participant could determine whether the meeting happens or not through her willingness-to-pay decision, the meeting treatment would be endogenous). The estimating equation for the meeting treatment is:

$$w_{ij} = \delta T_{ij}^m + \alpha_i + \varepsilon_{ij} \tag{12}$$

Where T_{ij}^m is a dummy equal to 1 if participant i is invited to a researcher-organized meeting [8] with j. The exact words used to administer the meeting treatment are

[8] The meeting treatment is similar to Lowe (2019) since both treatments are aimed at increasing the probability that individuals interact with each other. But Lowe (2019) introduces economic incentives to communicate with cricket players of a different social caste and looks at how this type of contact affects discrimination, while the meeting treatment of my study is not (and should not) be incentivized.

reported in Appendix C.

Table V presents results from estimating specification 11 in Column (1) and specification 12 in Column (2). The coefficients of interest are *Good News* and *Bad News*, which measure the change in willingness-to-pay of i for j for a 1-minute increase in absolute net distance, or $|d_{ij}^T - d_{ij}^P|$. The coefficient on *Good News* is negative as expected. It should be interpreted as the decrease in i's willingness-to-pay for j's information for every extra minute that j's plot is surprisingly further away than i believed. The coefficient size is 67 UGX and at the mean level of *Potential Good News*, 8 minutes, it implies a decrease of 536 UGX, \$0.14 or about 10% of the daily wage of a local agricultural laborer. The coefficient on *Bad News* is positive as expected but small and insignificant at conventional levels. Overall, results in Column (1) show that farmers react to being told good news, that they can be less concerned about the contagion externalities with a specific farmer j who is actually more distant than believed.

Theory does not predict the difference between the coefficient size of *Bad News* and *Good News* that Table V displays. An experimental feature that would explain this difference is that a *Good News* treatment is *actually* good news, while a *Bad News* treatment is not necessarily bad news, for the following reason. Farmers are asked about the walking distance between plots, so their answer may take into account rural paths, obstacles or differences in altitude. The distance the enumerator treats them with, d_{ij}^T, is the as-the-crow-flies walking distance (between plot centroids) assuming a farmer walks at 3 miles per hour (20 minutes per mile). As-the-crow-flies is the relevant distance metric for pest spread because the pest spreads by flying to other plots during the moth stage. Hence, the walking distance stated by farmers d_{ij}^P can only be larger than the true walking distance between plots if the farmer has perfect information and walks at a speed of 3 miles/hour. For this reason, when the true distance is larger than the perceived distance,$d_{ij}^T > d_{ij}^P$, the farmer is undoubtedly receiving a *Good News* type of shock. When instead $d_{ij}^T < d_{ij}^P$, it may be that the farmer does not actually receive bad news because she knows that the walking path is actually much longer than the as-the-crow-flies path. Table VI shows results based on specification 11, but in which the perceived distance d_{ij}^P used to calculate *Bad News*, *Good News*, *Potential Bad News* and *Potential Good News* has been replaced with λd_{ij}^P. The scaling factor λ, $0 \leq \lambda \leq 1$ is calculated in four scenarios. In the first column of Table VI, λ is set to equalize the means of the distance variables: $\lambda = {}^{E(d^T)}/_{E(d^P)}$. In the second column, λ is set to equalize the medians of the distance variables: $\lambda = {}^{Med(d^T)}/_{Med(d^P)}$. In the third column, λ is set such that true distance is the hypothenuse of an isosceles right triangle and the perceived distance d_{ij}^P is twice each cathetus: $\lambda = {}^1/_{\sqrt{2}}$. This scenario covers a potential situation showed in Figure IV. In the fourth column, λ is set such that true distance is the diameter of a circle and the perceived distance d_{ij}^P is half a circumference: $\lambda = {}^2/_\pi$.

Results in Table V mask substantial heterogeneity. Tables VII and VIII repeat the same analysis dividing the sample in four subgroups of perceived or true distance, respectively. Table VII shows that the farmers who react the most to the distance treatment are those whose initial beliefs about distance lay between 10 and 30 minutes. A believed distance lower than 10 minutes means that farmer j will very likely affect i's infection outcome in i's perception. Similarly, a believed distance larger than 30 may mean that i knows that j is anyway too far to affect her infection outcome. But between these two extremes lays a set of perceived distances at which farmers are much more sensitive, as small "good news" belief shocks seem to drive a large decrease in w_{ij}, while the pattern for bad news remains noisy. Heterogeneity in true distance does not seem to be as important. The coefficient for *Good News* is still negative in 3 out of 4 subsamples while *Bad News* is positive in 3 out of 4 subsamples, but estimates are imprecise.

Column (2) of Table V shows results of the meeting treatment. The point estimate is surprisingly negative, and insignificant. The meeting treatment did not seem to work as intended by design, overall, and there are many potential reasons behind this result. Before speculating on the reasons behind the unexpected results of the meeting treatment, there are two points that are worth mentioning.

First, encouraging farmers to interact is a common approach to disseminate information in rural areas in developing countries. Previous work has shown that it has some positive effects on adoption and yields (BenYishay and Mobarak, 2018; Vasilaky and Leonard, 2018). It is then quite surprising that, in my experiment, farmers do not attach a positive value to interacting with others to exchange information. If this behavior replicates in other contexts, uncovering its causes will help identify when we can leverage human interaction to disseminate knowledge.

Second, while the belief shock on knowledge externalities did not affect valuations as anticipated, it is possible to exploit the richness of the elicitation data to shed some light on what drives farmers' valuations for others. To do so, I let three model selection algorithms choose which variables best explain w_{ij}. Results of this exercise are displayed in Figure V. The 16 variables I feed to the algorithms are: true plot distance, perceived plot distance, home distance, whether i knows j, whether i visits or receives visits from j, whether i and j are relative, whether i and j socialize, whether i borrows/lends money from j, whether i borrows/lends goods from j, whether i receives/gives advice to j, whether they speak about agriculture, whether they pray together. These variables measure the extent of social connection between two individuals in different life domains, and are a modified version of the measures used by Banerjee et al. (2013) to quantify social connection between Indian villagers. The model selection algorithms used in this exercise are Lasso, Forward Stepwise and Best Subset. I adopt the following procedure: i) standardize regressors; ii) residualize both w_{ij} and each regressor on fixed effects for i and j, such that the remaining variation is specific to the ij dyad; iii) run the algorithms such that each selects the model with the highest explanatory power; iv) regress w_{ij} on the three selected models. Figure V shows the coefficient sizes of regressors in each selected model, expressed in standard deviations. Whether farmers know each other is the predictor with the largest coefficient, followed by receiving advice. Distance measures are negatively correlated to willingness-to-pay, as one would expect. Being related and socializing also matter, but less than knowing and receiving advice. I interpret the selected coefficients as providing some suggesting evidence that farmers value training another farmer if that person is accessible and has been providing advice in the past. The findings are also consistent with altruistic motives, as we would expect a farmer to be altruistic towards relatives and villagers she socializes with.

One element that emerged during the elicitation exercise, and that is relevant here, is that 92% of all farmer dyads know each other already. It may be that farmers prefer to exchange information on their own, outside of a structured social interaction mediated by an enumerator; or it may be that farmers anticipate costs in term of lost labor or transport without any added benefits, given that they are anyway able to reach the recipient independently from the research team. Another option is that farmers are particularly adverse to meeting farmers that they do not know very well, while attaching a positive value to them receiving training. Or still, farmers may be unwilling to meet someone that is more knowledgeable than them for reputation purposes or because they anticipate feeling shameful. They may also be willing to pay for someone's training for reciprocity purposes (to give back to someone, or to trigger a symmetric reaction from them) which are orthogonal to the information content of the training. Finally, another potential explanation is that a participant infers that, because meetings will happen, the information content of the training is going to circulate in the village. This realization can trigger a free-riding behavior, where the participant decreases her willingness-to-pay for all the farmers she is asked about after learning that researchers will organize meetings.

VI.D. Social Benefit of Adoption is Larger than Private Benefit

My willingness-to-pay elicitation allows for a back-of-the-envelope calculation of the perceived private and social benefits of adoption. The perceived private benefit of adoption choices made by farmer i living in village v is measured by a participant i's willingness-to-pay for herself,

$$B^p_{i,v} = w_{ii}$$

The perceived social benefit of adoption choices made by farmer i living in village v is the sum of the perceived private benefit plus the perceived benefit of all other individuals in a certain reference group G of size N_G,

$$B^s_{i,v} = B^p_{i,v} + \sum_{j \in G} w_{ji} \tag{13}$$

So that ideally, to measure the perceived social benefit of adoption a researcher would need to elicit the willingness-to-pay of each participant for every other member of the reference group. In what follows, I will discuss the social benefit of adoption in different scenarios with the purpose of providing the reader with reasonable bounds.

Case 1: The researcher observes the whole reference group

Suppose that my sample includes the whole reference group G, or $N_G = N_S$. In this case I can calculate two objects. First, I can calculate the social benefit of providing the technology to one farmer in the group using (13) and characterize the individuals whose adoption increases the social benefit the most. Second, I can calculate the social benefit of providing the technology to all farmers in the group. Because in this case there are no externalities, the social benefit is just the sum of private benefits. In this setup it is not possible to calculate the social benefit when any intermediate number of farmers adopt, i.e. more than 1 and less than N_G farmers adopt. This is because, by design, my experiment cannot show how the size of the knowledge and contagion externalities changes with the share of farmers who adopt. Figure X displays the social benefit generated when all farmers are trained (left bar) together with two alternative scenarios. The central bar shows the social benefit of training the *best* farmer, the one whose adoption generates the highest social benefit; the right bar shows the social benefit of training the *worst* farmer, whose adoption generates the lowest social benefit. The difference between the top and bottom panels of Figure X is that the top panel shows the gross social benefit as in equation (13); the bottom panel shows the net social benefit, obtained by subtracting the training costs. Figure X suggests that it is not optimal to train all farmers; while the average gross benefit is large (20,000 Ugandan Shillings), the net social benefit would be negative (-13,000 Ugandan Shillings). But it is optimal to train *some* farmers, and the identity of the training recipient matters. Being able to target the best farmer yields twice the net social benefit of targeting the worst farmer (10,000 versus 5,000 Ugandan Shillings for the best and worst farmer respectively). Figure XI displays selected characteristics of the best farmers in comparison with the rest of the sample. Best farmers do not appear to differ from other farmers in terms of demographics, farm size or input use.

Case 2: The researcher observes a random sample of the reference group

In my case, I have a random sample $N_{s,v}$ of each village's population N_v. Assuming that the valuations formulated within sample are representative of the average farmer in the village, I can scale up the average valuations in different ways to obtain the distribution of perceived social benefits.

I start by taking the village as a reference group, such that the size of the reference group is the village size, $N_G = N_v$. The village is a reasonable reference group because collective decisions are typically taken by democratically elected village leaders, the village is the administrative unit for government interventions, and social aggregation typically happens at the village market or water well. Scaling up the average valuation by village size implies calculating the social benefit in the following way:

$$B^s_{i,v} = B^p_{i,v} + \frac{N_v}{N_{s,v}} \sum_{j \in N_{s,v}} w_{ji} \tag{14}$$

This yields the distribution of social benefit pictured in figure XII. It ranges between 150,000 and 8M Ugandan Shillings, equivalent to \$129.6 and \$6,913. Overall, the social benefit is substantially larger than the cost of providing the training service, 34,000 Ugandan Shillings or \$ 29.4. Figure XIII shows the ratio of private benefit to social benefit. On average the ratio is 1.5%, ranging from 0.09% to 2%.

A shortcoming of this approach is that it implicitly assumes that the social benefit is only confined to the village borders, while benefits of adoption may extend to farmers beyond the border (both in terms of knowledge spread and pest load decrease); an implication is that more populous villages will have higher perceived social benefit than less populous villages. To provide a distribution of $B^s_{i,v}$ that is independent from village size, I scale-up average valuations by a set of guesses for the size of the reference group G in the following way,

$$B^s_{i,v} = B^p_{i,v} + \frac{N_G}{N_{s,v}} \sum_{j \in N_{s,v}} w_{ji} \tag{15}$$

where $N_G = (50, 150, 250, 350, 450, 550)$. Minimum and maximum guesses, 50 and 550, are the minimum and maximum number of households recorded in a researcher-run village census [9]. The result is diplayed in figure XIV. The solid black line depicts the same distribution as in figure XII for comparison purposes. Dashed, dotted and dash-dotted lines in black or gray show the distribution of B^i_s for different sizes of the reference group. Using arbitrary reference groups shows that, even with a reference group of only 50 households, the social benefit is about 600,000 Ugandan shillings - almost 18 times the cost of service provision.

VII. Conclusion

Agricultural technology adoption is low in sub-Saharan Africa, and this has caused productivity to stagnate. This article investigates whether positive externalities can be an obstacle to adoption. I study the adoption of a set of new technologies, namely pest-control techniques to manage the pest Fall Armyworm. The pest, originally from the Americas, reached Africa in 2016 and has caused large harvest losses, especially among maize farmers.

I use a lab-in-the-field experiment to obtain truthful valuations from farmers to receive training on pest-control techniques. Farmers formulate valuations both for themselves to receive the training, and for other sampled farmers in the village to receive the training, conditional on the random selection of one farmer among

[9] Done as part of the activities of Burchardi et al. (2019) in September 2017.

them that could actually receive training. This feature allows me to calculate the total externalities that each participant anticipates as a consequence of training another farmer in their community. I then identify two types of externalities that matter in this context, namely *contagion* and *knowledge* externalities, and generate experimental variation in the two types of externalities to evaluate how they affect farmer's valuations. I do so by causing farmers to update (i) their belief over the probability of infection from another farmer's plot to theirs (and so generating random variation in the perceived size of the *contagion* externality between the two farmers), (ii) their beliefs over the probability that knowledge of the pest-control techniques spills over from another farmer to them (and so generating random variation in the *knowledge* externality between them).

My results support the view that externalities matter for adoption. First, I exploit a feature of the design that allows me to have random variation in training provision across villages. I verify that adoption increases in the villages where one farmer is trained and conclude that the training is beneficial and has spillovers on non-trained farmers. Second, farmers' valuations for another farmer to receive the training have substantial monetary value of about $ 9. This is evidence that farmers anticipate benefitting from others' adoption, Third, I find that farmers respond to contagion externalities, as they decrease their willingness-to-pay for farmers with lower contagion externalities. My experimental approach is not effective at capturing knowledge externalities, but I provide evidence that farmers value more the training of those that are more accessible to them and from whom they have received advice in the past. Fourth, I calculate the social and private benefits of adoption using farmers' valuations. I find that social benefit is at least 30 times larger than private benefit, but highly heterogeneous across training recipients. This suggests that the optimal policy is nuanced. It is not optimal for all farmers to adopt, as the net social benefit would be negative; but since positive externalities are large, it is optimal that some farmers adopt. The ideal adopters are farmers who are socially central in the social network, to maximize knowledge externalities, and whose plots are centrally located, to maximize contagion externalities.

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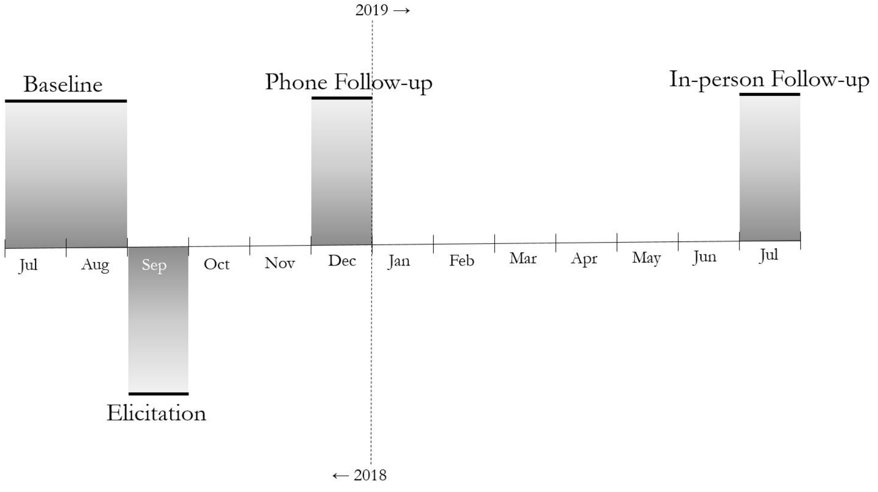
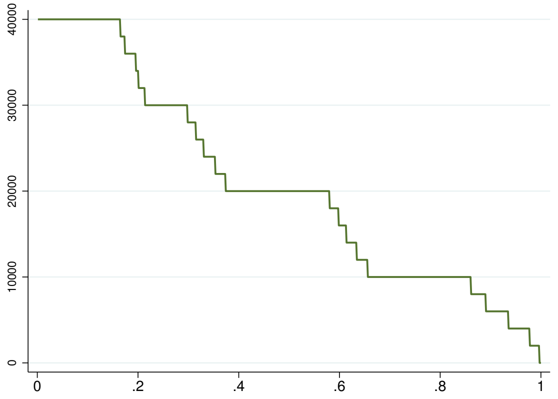
VIII. Figures

Figure I: DATA COLLECTION TIMELINE

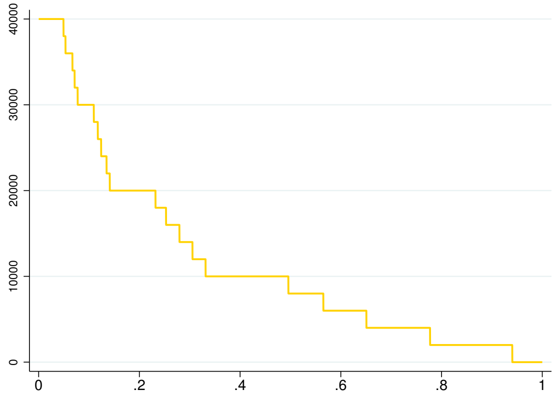


Notes: The figure shows the time schedule of the data collection for the project. Shaded bars indicate the months in which a survey happened, and the survey name lies on top of each bar.

Figure II: DEMAND CURVES



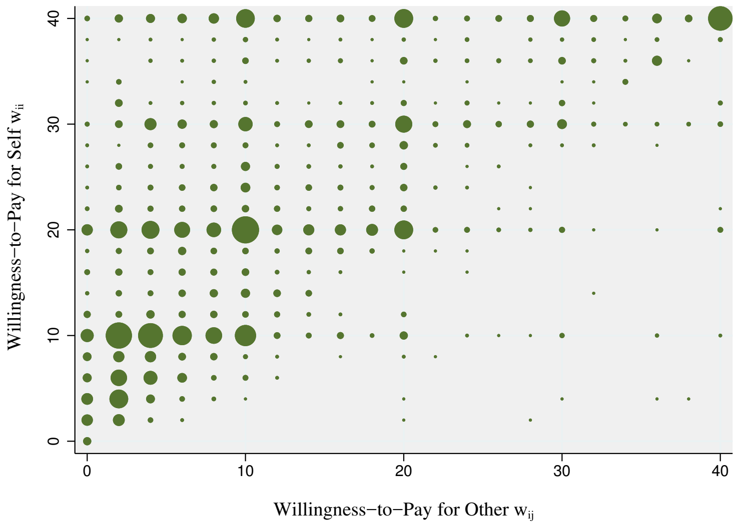
(a) FOR SELF



(b) FOR OTHERS

Notes: The figures depicts the share of participants (on the x-axis) that would purchase training for each of the 21 possible prices (on the y-axis). In panel (a), participants would be paying to receive the training themselves. In panel (b), participants would be paying for training received by another farmer in the village.

Figure III: JOINT DISTRIBUTION OF WTP FOR SELF AND OTHER

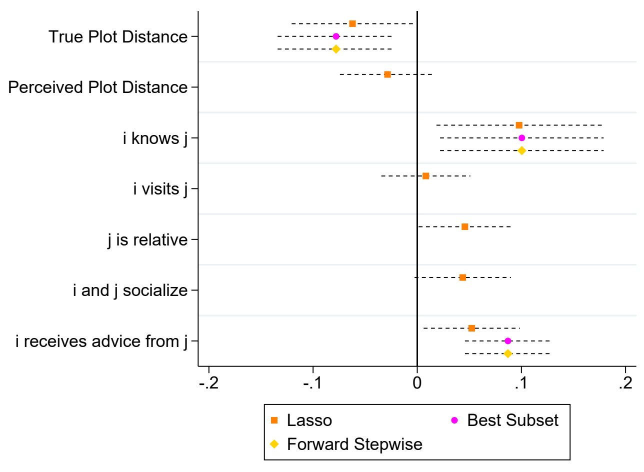


Notes: The figures depicts a scatterplot of the joint distribution of w_{ii}, the willingness-to-pay of i for herself to receive the training, against w_{ij}, i's willingness-to-pay for j to receive the training, for each of the 21 possible training prices.

Figure IV: EXAMPLE OF AS-THE-CROW-FLIES DISTANCE VS. WALKING DISTANCE

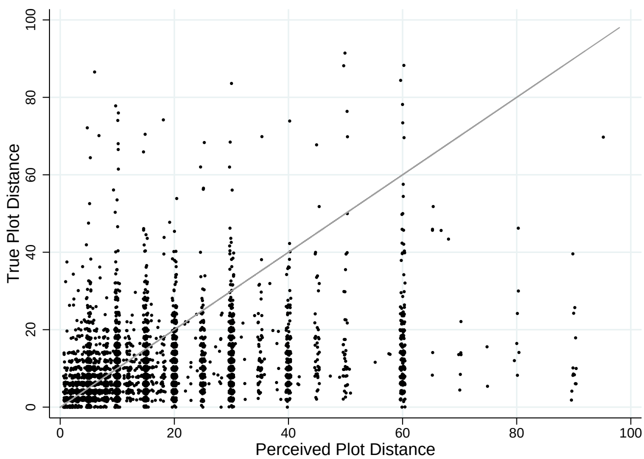
Notes: The figure depicts the distance between centroids of plots A and B if calculated as-the-crow-flies (solid line) or if taking into account rural paths (dotted line).

Figure V: CORRELATES OF WILLINGNESS TO PAY



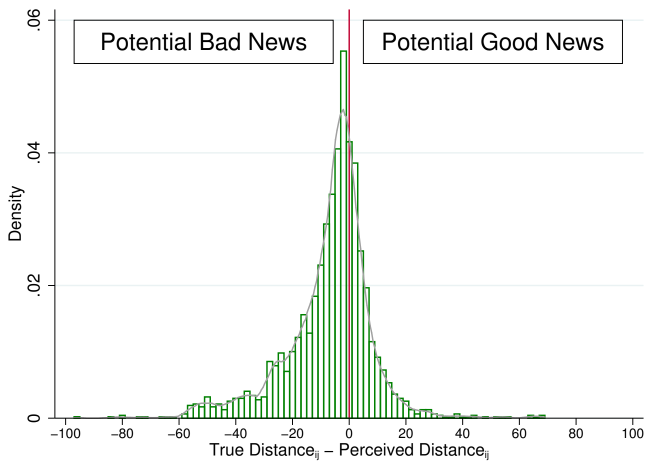
Notes: The figure depicts the standardized coefficients of a regression of w_{ij} on subsets of regressors generated by Lasso, Best Subset, and Forward Stepwise respectively. In each case, the algorithm is fed a set of 16 variables: *True Distance*$_{ij}$, *Perceived Distance*$_{ij}$, *Home Distance*$_{ij}$, *Know*$_{ij}$, *Receive Visit*$_{ij}$, *Pay Visit*$_{ij}$, *Relative*$_{ij}$, *Socialize*$_{ij}$, *Borrow Money*$_{ij}$, *Lend Money*$_{ij}$, *Borrow Goods*$_{ij}$, *Lend Goods*$_{ij}$, *Receive Advice*$_{ij}$, *Give Advice*$_{ij}$, *Speak Agriculture*$_{ij}$, *Pray*$_{ij}$. Each algorithm returns a subset of the 16 variables. The willingness-to-pay w_{ij} is then regressed on each subset, including fixed effects for participant i and recipient j.

Figure VI: JOINT DISTRIBUTION OF TRUE AND PERCEIVED PLOT DISTANCE



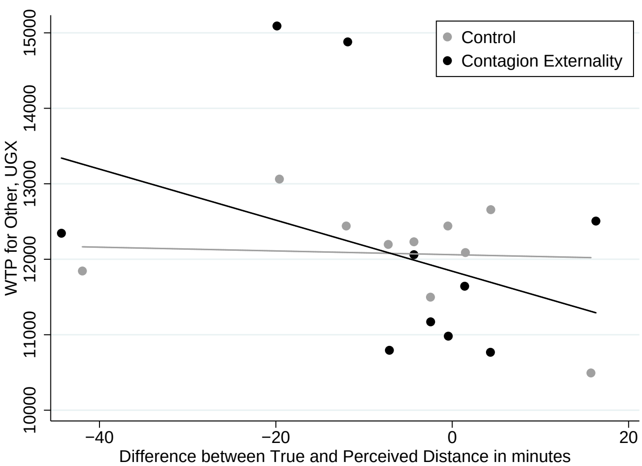
Notes: The figure depicts a scatterplot of the joint distribution of *True Distance*$_{ij}$ (d^T_{ij}) and *Perceived Distance*$_{ij}$ (d^P_{ij}). Both variables refer to the distance between *i*'s largest maize plot and *j*'s closest plot and are measured in walking minutes. *True Distance*$_{ij}$ is the as-the-crow-flies distance between coordinates of the plots' centroids, measured by a GPS tracker in miles and converted in walking distance assuming that the average adult takes 20 minutes to walk one mile. *Perceived Distance*$_{ij}$ is the answer provided by *i* when asked to estimate the distance between her largest plot and *j*'s closest plot. The 45-degree line is added for reference.

Figure VII: DISTRIBUTION OF NET PLOT DISTANCE



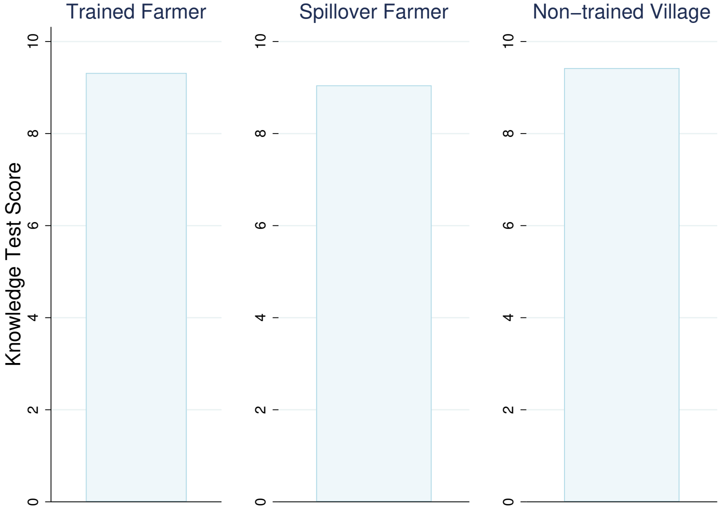
Notes: The figure depics a scatterplot a histogram of the probability density function of the difference between *True Distance*$_{ij}$ and *Perceived Distance*$_{ij}$ for each farmer dyad in the control group, where the control group is the set of dyads in which participant i had neither been exposed to the meeting treatment nor to the distance treatment with respect to farmer j. Both variables refer to the distance between i's largest maize plot and j's closest plot and are measured in walking minutes. *True Distance*$_{ij}$ is the as-the-crow-flies distance between coordinates of the plots' centroids, measured by a GPS tracker in miles and converted in walking distance assuming that the average adult takes 20 minutes to walk one mile. *Perceived Distance*$_{ij}$ is the answer provided by i when asked to estimate the distance between her largest plot and j's closest plot.

Figure VIII: EFFECT OF BEING TOLD DISTANCE, BY DECILES OF NET DISTANCE



Notes: The figure depicts the average willingness-to-pay of i for j, w_{ij}, for each bin of net distance, the difference between *True Distance*$_{ij}$ and *Perceived Distance*$_{ij}$. Two averages are shown for each bin of net distance. Grey points indicate the average w_{ij} of i for the control group, where the control group is the set of dyads in which participant i had neither been exposed to the meeting treatment nor to the distance treatment with respect to farmer j. Black points indicate the average w_{ij} of i for the distance group, where the distance group is the set of dyads in which participant i has been informed of the true distance between her largest plot and farmer j's closest plot.

Figure IX: Knowledge at Endline

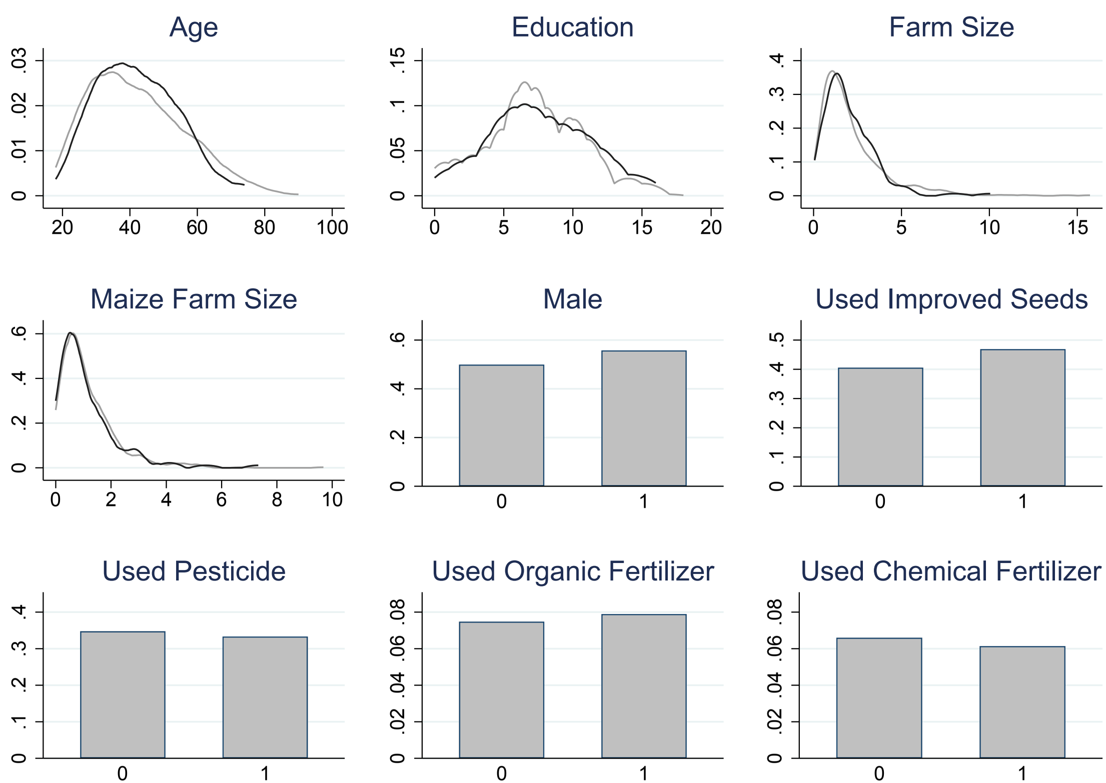


Notes: The figure depicts the average score in a FAW knowledge test obtained by three categories of participants. The left bar shows the average score of trained farmers; the central bar shows the average score of spillover farmers (non-trained farmers in trained villages); the left bar shows the average score of farmers in non-trained villages.

Figure X: GROSS AND NET SOCIAL BENEFIT OF ADOPTION

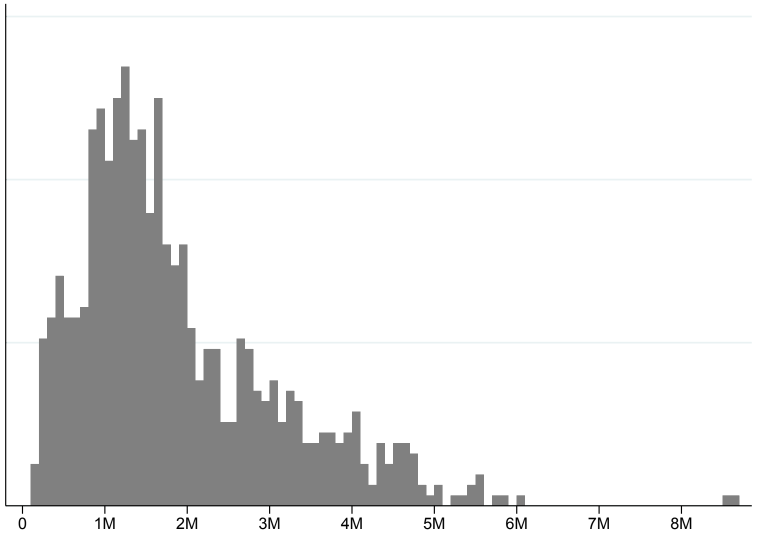
Notes: The figures depicts the average social benefit generated in a village in which all farmers are adopt (left bar), or only the best farmer adopts (center bar; the best farmer is the farmer whose adoption generates the highest social benefit relative to any other farmer adopting), or only the worst farmer adopts (right bar; the worst farmer is the farmer whose adoption generates the lowest social benefit relative to any other farmer adopting). The top panel displays gross social benefit. The bottom panel displays net social benefit, after subtracting training costs.

Figure XI: CHARACTERISTICS OF BEST FARMERS



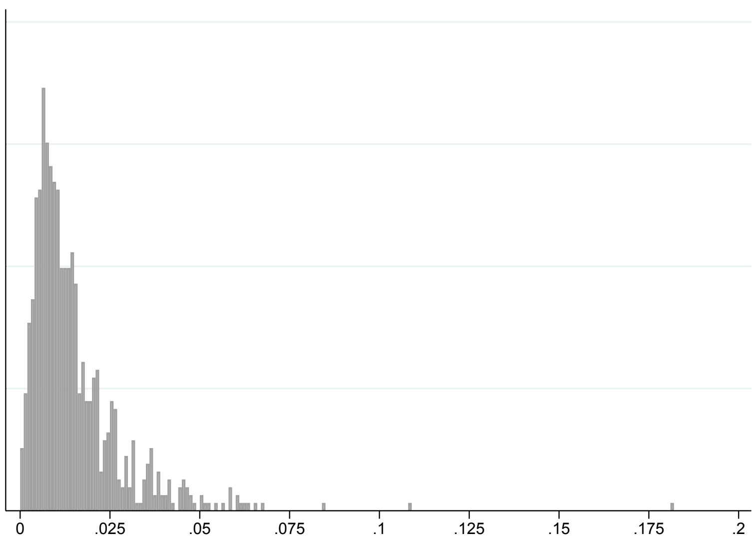
Notes: The figure shows the distribution of specified characteristics among best farmers (black solid line or right-most bar) and among all other farmers (solid grey line or left-most bar). Detailed variable definitions are provided in Section B.

Figure XII: SOCIAL BENEFIT SCALED BY VILLAGE POPULATION



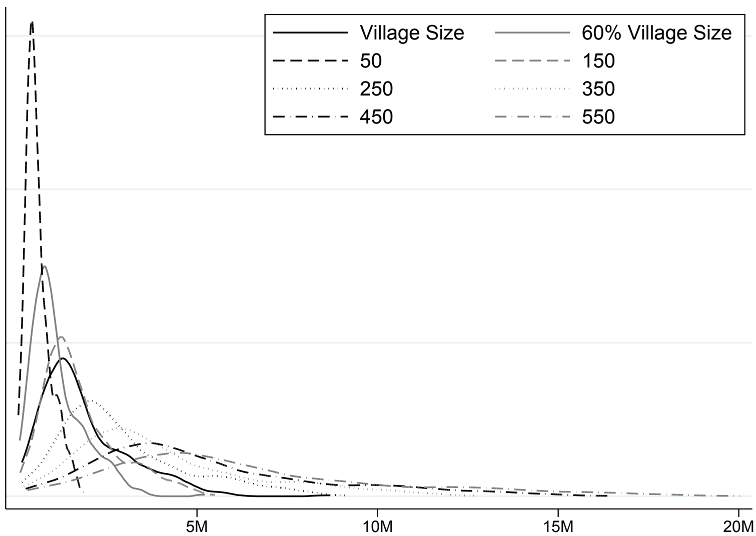
Notes: The figure depicts the distribution of the social benefit of a farmer i's adoption in village v, in Ugandan Shillings. It is calculated as the sum of the willingness-to-pay of i for herself to adopt, w_{ii}, plus the average willingness-to-pay of sampled co-villagers for i to adopt scaled up by village population size N_v, $B^s_{i,v} = w_{ii} + \frac{N_v}{S_v} \sum_{j \in S_v} w_{ji}$.

Figure XIII: PRIVATE/SOCIAL BENEFIT RATIO



Notes: The figure depicts the distribution of the ratio of the private benefit of i's adoption, $B^P_{i,v} = w_{ii}$, over the social benefit of i's adoption $B^S_{i,v} = w_{ii} + \frac{N_v}{S_v} \sum_{j \in S_v} w_{ji}$.

Figure XIV: SOCIAL BENEFIT SCALED BY GUESSES FOR NETWORK SIZE



Notes: The figure depicts the distribution of the social benefit of a farmer i's adoption in village v, in Ugandan Shillings,for different sizes N_G of the reference group G. The solid black line shows $B^s_{i,v} = w_{ii} + \frac{N_v}{S_v}\sum_{j\in S_v} w_{ji}$ as in Figure XII, where $N_G = N_v$. Dashed, dotted and dot-dashed lines show the social benefit calculated for arbitrarily chosen $N_G = (50, 150, 250, 350, 450, 550)$, $B^s_{i,v} = w_{ii} + \frac{N_G}{S_v}\sum_{j\in S_v} w_{ji}$.

IX. Tables

Table I: DYAD-LEVEL SUMMARY STATISTICS

Covariate X	$\overline{X}_C$ $N_C = 2783$	*Means* $\overline{X}_M$ $N_M = 1348$	$\overline{X}_D$ $N_D = 1379$	*t-statistics* M v. C	D v. C	*Normalized Diff.* M v. C	D v. C
True Plot Distance	10.88	10.78	10.81	-0.26	-0.18	-0.01	-0.01
Perceived Plot Distance	18.02	18.32	17.54	0.46	-0.79	0.02	-0.03
Home Distance	15.09	15.30	16.05	0.25	1.00	0.01	0.03
Connection	7.43	7.35	7.50	-0.54	0.50	-0.02	0.02
i knows j	0.92	0.92	0.92	0.39	0.17	0.01	0.01
i receives Visit	0.80	0.80	0.80	0.07	0.23	0.00	0.01
i visits j	0.77	0.77	0.78	-0.43	0.25	-0.02	0.01
j is relative	0.33	0.33	0.33	-0.50	-0.18	-0.02	-0.01
i and j socialize	0.79	0.79	0.80	0.22	1.21	0.01	0.04
Borrow Money	0.57	0.55	0.55	-0.89	-1.06	-0.03	-0.04
Lend Money	0.56	0.55	0.55	-0.26	-0.20	-0.01	-0.01
Borrow Goods	0.38	0.36	0.40	-1.05	1.33	-0.04	0.05
Lend Goods	0.38	0.36	0.40	-1.10	1.14	-0.04	0.04
i receives advice from j	0.63	0.62	0.64	-0.63	0.56	-0.02	0.02
Give Advice	0.67	0.65	0.67	-1.34	0.13	-0.05	0.00
Speak Agriculture	0.80	0.79	0.81	-0.31	0.84	-0.01	0.03
Pray	0.45	0.44	0.45	-0.66	-0.05	-0.02	-0.00
Recipient Bought Fertilizer	0.48	0.47	0.48	-0.93	0.11	-0.03	0.00
Recipient Won Lottery	0.38	0.39	0.36	0.86	-0.71	0.03	-0.02
Recipient is Male	0.50	0.50	0.52	0.15	1.02	0.01	0.03
Recipient's Age	42.41	41.81	41.78	-1.30	-1.39	-0.04	-0.05
Recipient Education	7.12	7.01	7.30	-0.94	1.43	-0.03	0.05
Recipient is Literate	0.80	0.78	0.80	-1.53	-0.02	-0.05	-0.00
Recipient's Farm Area	2.18	2.10	2.20	-1.25	0.25	-0.04	0.01
Recipient's Maize Area	1.14	1.11	1.15	-1.07	0.25	-0.04	0.01

Table II: BALANCE TEST AND DECRIPTIVES – DECISION MAKER LOTTERY

	(1) Regression Coefficient	(2) Mean of Not Selected	(3) N
Male	0.0138	0.510	692
	(0.0530)	(0.500)	
Years of Education	-0.138	7.150	692
	(0.350)	(3.710)	
Household Members	-0.0826	6.730	672
	(0.312)	(2.590)	
Plot Number	-0.109	3.310	672
	(0.157)	(1.510)	
Farm Area (Acres)	-0.0979	2.170	691
	(0.175)	(1.900)	
Maize Area (Acres)	-0.0457	1.150	691
	(0.0889)	(1.080)	
Used Improved Seeds	0.0338	0.410	677
	(0.0504)	(0.490)	
Used Pesticides	0.0239	0.340	695
	(0.0495)	(0.470)	
Used Organic Fertilizer	0.0271	0.0700	695
	(0.0301)	(0.260)	
Used Chemical Fertilizer	0.00456	0.0600	695
	(0.0234)	(0.250)	

Notes: Column 1 shows the difference between the treatment and control group obtained by regressing each characteristic on a dummy variable equal to one if the farmer was selected to be a decision maker by the *Decision Maker Lottery*. Column 2 shows the mean (and standard deviation in brackets) of each baseline characteristics among farmers who have not been selected as decision makers. Column (3) reports the number of observations. Robust standard errors are clustered at the village level and given in round brackets; *** (**) (*) indicates significance at the 1% (5%) (10%) level. Detailed variable definitions are provided in Section B.

Table III: BALANCE TEST AND DECRIPTIVES - CHOICE LOTTERY

	(1) Regression Coefficient	(2) Mean of Not Selected	(3) N
Male	0.0701	0.500	692
	(0.0526)	(0.500)	
Years of Education	0.650*	7.050	692
	(0.389)	(3.650)	
Household Members	-0.00955	6.720	674
	(0.287)	(2.620)	
Plot Number	0.140	3.270	674
	(0.172)	(1.500)	
Farm Area (Acres)	0.0572	2.150	690
	(0.186)	(1.880)	
Maize Area (Acres)	0.139	1.120	690
	(0.125)	(1.020)	
Used Improved Seeds	0.0454	0.410	677
	(0.0533)	(0.490)	
Used Pesticides	-0.0177	0.350	693
	(0.0478)	(0.480)	
Used Organic Fertilizer	0.0361	0.0700	693
	(0.0306)	(0.260)	
Used Chemical Fertilizer	0.0476	0.0600	693
	(0.0317)	(0.240)	

Notes: Column 1 shows the difference between the treatment and control group obtained by regressing each characteristic on a dummy variable equal to one if the farmer was selected to be a potential trainee by the *Choice Lottery*. Column 2 shows the mean (and standard deviation in brackets) of each baseline characteristics among farmers who have not been selected as potential trainees. Column (3) reports the number of observations. Robust standard errors are clustered at the village level and given in round brackets; *** (**) (*) indicates significance at the 1% (5%) (10%) level. Detailed variable definitions are provided in Section B.

Table IV: EFFECTS ON ADOPTION

Dependent Variable: $Adoption_{iv}$	(1) Within Season	(2) After 1 Season	(3) After 2 Seasons
Trained Village × Spillover Farmer	0.135**	0.002	0.018
	(0.064)	(0.017)	(0.022)
Trained Village × Trained Farmer	0.146*	0.003	0.027
	(0.085)	(0.026)	(0.034)
Control Mean	0.43	0.96	0.94
Observations	739	733	715

Notes: The table reports estimates based on specification (10). The dependent variable *Adoption* is a dummy variable equal to 1 if the participant reported having adopted any of the following recommended practices: scouting, crushing, spraying, pouring, fertilizing. *TrainedVillage* is a dummy variable equal to 1 if training has taken place in the village. *TrainedFarmer* is a dummy variable equal to 1 if farmer i has been trained and equal to 0 if i has not been trained and is a spillover farmer. Column (1) shows mid-season effects, 2 months after training. Column (2) shows effects at the end of the same season, while column (3) shows effects at the end of the following season. All specifications include fixed effects for the willingness to pay of the Decision Maker for the potential trainee in the village. Robust standard errors are clustered at the village level and given in round brackets; *** (**) (*) indicates significance at the 1% (5%) (10%) level.

Table V: EFFECTS OF BELIEF UPDATE INTERVENTION

Dependent Variable: w_{ij}	Contagion Externality	Information Externality
Good News	-67.033*	
	(35.355)	
Bad News	1.589	
	(8.932)	
Potential Good News	-2.569	
	(20.744)	
Potential Bad News	-14.327*	
	(8.017)	
Proposed Meeting		-175.357
		(145.828)
Control Mean w_ij	11,621	
Control Mean *Pot. Good News*	8	
Control Mean *Pot. Bad News*	14	
Observations	3,520	3,975

Notes: The table reports ordinary least squares estimates based on specification (11) in Column 1 and (12) in Column 2. The dependent variable w_{ij} is the willingness-to-pay of the participant i for farmer j. *Potential Good News* is equal to the absolute value of the difference between true and perceived distance in walking minutes if the difference is positive, $|d_{ij}^T - d_{ij}^P|$ if $d_{ij}^T - d_{ij}^P > 0$. *Potential Bad News* is equal to the absolute value of the difference between true and perceived distance in walking minutes if the difference is negative, $|d_{ij}^T - d_{ij}^P|$ if $d_{ij}^T - d_{ij}^P < 0$. *Good (Bad) News* is just *Potential Good (Bad) News* multiplied by a treatment indicator T_d equal to 1 if the true distance was revealed to the participant. *Proposed Meeting* is a dummy variable equal to 1 if participant i has been offered to meet with farmer j. Values are in Ugandan Shillings. All specifications include participant i fixed effects. Robust standard errors are given in round brackets; *** (**) (*) indicates significance at the 1% (5%) (10%) level.

Table VI: EFFECTS OF BELIEF UPDATE INTERVENTION WITH RESCALED DISTANCES

Dependent Variable: w_{ij}				
	Equality		Path	
	Of Means	Of Medians	Triangle	Half Circle
Scaling Factor λ:	$E(d^T)/E(d^P)$	$Med(d^T)/Med(d^P)$	$1/\sqrt{2}$	$2/\pi$
Good News	-49.265**	-49.227**	-49.452**	-49.288**
	(21.633)	(21.634)	(21.635)	(21.632)
Bad News	6.917	5.945	4.997	7.316
	(16.400)	(15.502)	(14.540)	(16.760)
Potential Good News	-9.662	-9.825	-9.740	-9.593
	(17.002)	(17.000)	(16.989)	(16.999)
Potential Bad News	-25.720*	-24.303*	-22.885*	-26.272*
	(13.770)	(13.050)	(12.280)	(14.056)
Control Mean w_{ij}	11,621	11,621	11,621	11,621
Control Mean *Pot. Good News*	7	8	8	7
Control Mean *Pot. Bad News*	8	9	9	8
Observations	3,586	3,586	3,586	3,586

Notes: The table reports ordinary least squares estimates based on specification (11). The dependent variable w_{ij} is the willingness-to-pay of the participant i for farmer j. *Potential Good News* is equal to the absolute value of the difference between true and rescaled perceived distance in walking minutes if the difference is positive, $|d^T_{ij} - \lambda d^P_{ij}|$ if $d^T_{ij}\lambda - d^P_{ij} > 0$. *Potential Bad News* is equal to the absolute value of the difference between true and rescaled perceived distance in walking minutes if the difference is negative, $|d^T_{ij} - \lambda d^P_{ij}|$ if $d^T_{ij} - \lambda d^P_{ij} < 0$. *Good (Bad) News* is equal to *Potential Good (Bad) News* multiplied by a treatment indicator T_d equal to 1 if the true distance was revealed to the participant. In the column marked by "Equality of Means" factor λ solves $E(d^T_{ij}) = \lambda E(d^P_{ij})$; so the perceived distance is rescaled to have mean equal to the true distance. In the column marked by "Equality of Medians" factor λ solves $Med(d^T_{ij}) = \lambda Med(d^P_{ij})$; so the perceived distance is rescaled to have median equal to the true distance. In the column marked by "Triangle Path" factor λ solves $d^T_{ij} = \lambda d^P_{ij}$, d^T_{ij} is the hypotenuse of an isosceles right triangle and d^P_{ij} is twice each cathetus, $(d^T_{ij})^2 = (\frac{1}{2}d^P_{ij})^2 + (\frac{1}{2}d^P_{ij})^2$. In the column marked by "Half-Circle Path" factor λ solves $d^T_{ij} = \lambda d^P_{ij}$, d^T_{ij} is the diameter of a circle and d^P_{ij} is half of a circumference, $2d^P_{ij} = 2\pi \frac{d^T_{ij}}{2}$. Then $\lambda = 2/\pi$. Values are in Ugandan Shillings. All specifications include participant i fixed effects. Robust standard errors are given in round brackets; *** (**) (*) indicates significance at the 1% (5%) (10%) level.

Table VII: HETEROGENEITY IN PERCEIVED DISTANCE

	Perceived Distance in Minutes d_{ij}^{P}			
Dependent Variable: w_{ij}	0-10	10-20	20-30	30-60
Good News	-4.50	-153.45***	-952.13*	12.65
	(58.95)	(51.51)	(504.02)	(25.92)
Bad News	-226.66	6.50	49.14	-3.10
	(185.49)	(63.54)	(85.83)	(23.99)
Potential Good News	24.03	11.58	24.83	-16.45
	(46.40)	(33.29)	(328.91)	(35.38)
Potential Bad News	91.33	54.56	43.65	-28.09
	(120.84)	(51.61)	(88.83)	(38.44)
Control Mean w_{ij}	11,725	12,233	11,947	12,903
Observations	1,127	1,186	440	579

Notes: The table reports ordinary least squares estimates based on specification (11) for different intervals of self-reported walking distance between the largest maize plot of participant *i* and the closest maize plot of farmer *j*. The dependent variable w_{ij} is the willingness-to-pay of the participant *i* for farmer *j* in UGX. *Potential Shock (if j is Farther* is equal to the absolute value of the difference between true and perceived distance in walking minutes if the difference is positive, $|d_{ij}^{T} - d_{ij}^{P}|$ if $d_{ij}^{T} - d_{ij}^{P} > 0$. *Potential Shock (if j is Closer)* is equal to the absolute value of the difference between true and perceived distance in walking minutes if the difference is negative, $|d_{ij}^{T} - d_{ij}^{P}|$ if $d_{ij}^{T} - d_{ij}^{P} < 0$. *Actual Shock* is just *Potential Shock* multiplied by a treatment indicator T_d equal to 1 if the true distance was revealed to the participant. The estimates cannot be provided for values of d_{ij}^{P} larger than 60 because of insufficient observations. Values are in Ugandan Shillings. All specifications include participant *i* fixed effects. Robust standard errors are given in round brackets; *** (**) (*) indicates significance at the 1% (5%) (10%) level.

Table VIII: HETEROGENEITY IN TRUE DISTANCE

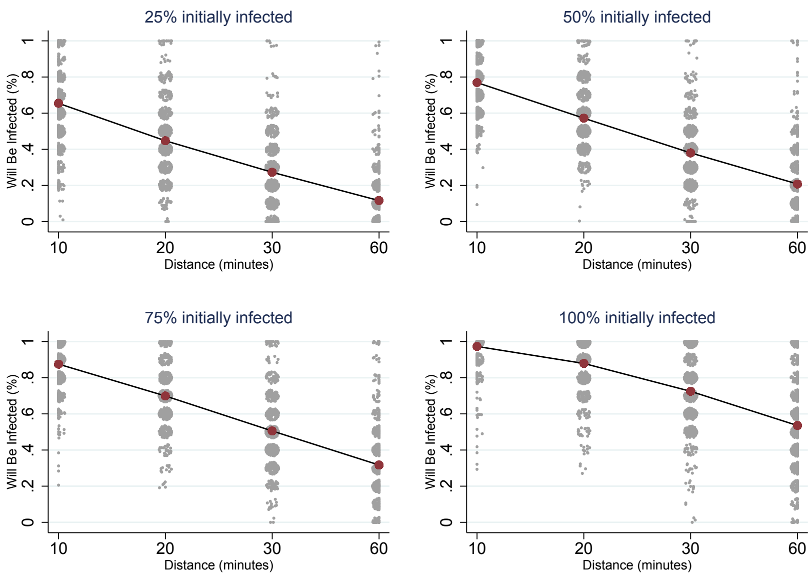
	True Distance in Minutes d_{ij}^T			
Dependent Variable: w_{ij}	0-10	10-20	20-30	30-60
Good News	-471.36	43.09	-48.38	-47.33
	(289.17)	(149.00)	(172.46)	(107.09)
Bad News	1.59	18.96	-114.80	69.46
	(14.05)	(21.73)	(137.90)	(196.18)
Potential Good News	23.77	108.12	-166.71	0.37
	(164.22)	(80.20)	(143.09)	(74.66)
Potential Bad News	-13.94	-2.42	-45.91	-130.85
	(12.17)	(20.23)	(61.07)	(133.78)
Control Mean w_{ij}	11,725	11,380	10,588	11,149
Observations	1,997	1,117	270	106

Notes: The table reports ordinary least squares estimates based on specification (11) for different intervals of GPS-measured walking distance between the largest maize plot of participant i and the closest maize plot of farmer j. The dependent variable w_{ij} is the willingness-to-pay of the participant i for farmer j. *Potential Shock (if j is Farther* is equal to the absolute value of the difference between true and perceived distance in walking minutes if the difference is positive, $|d_{ij}^T - d_{ij}^P|$ if $d_{ij}^T - d_{ij}^P > 0$. *Potential Shock (if j is Closer)* is equal to the absolute value of the difference between true and perceived distance in walking minutes if the difference is negative, $|d_{ij}^T - d_{ij}^P|$ if $d_{ij}^T - d_{ij}^P < 0$. *Actual Shock* is just *Potential Shock* multiplied by a treatment indicator T_d equal to 1 if the true distance was revealed to the participant. Values are in Ugandan Shillings. All specifications include participant i fixed effects. Robust standard errors are given in round brackets; *** (**) (*) indicates significance at the 1% (5%) (10%) level.

A. Extra Figures

Figure A.I: Farmers' Beliefs over Infection Probability

The figure depicts a scatterplot of the answers given by farmers to the question "How many plots at walking distance X will be infected if Y% of the plants in the central plot are affected by Fall Armyworm?". The x-axis reports four different distance values, 10, 20, 30 and 60 minutes while the y-axis reports the share of plots the farmer thinks will be infected if a plot situated in the middle is affected by Fall Armyworm. Each panel shows answers provided for a different level of initial plant infestation in the central plot, increasing clockwise. Grey dots show the raw data, jittered to show more frequent answers. Red dots mark the average answer at each value of walking distance. Red dots are connected with a straight line to show, on average, the relationship between infection rate and distance.



B. Variable List

Outcome Variables	
Trained Village	A dummy variable taking the value of one if the training took place in the village.
Trained Farmer	A dummy variable taking the value of one if the farmer received training.
Spillover Farmer	A dummy variable taking the value of one if the farmer did not receive training.
True Plot Distance d_{ij}^T	The physical distance between the centroids of i's largest plot and j's closest plot in walking minutes. The GPS-measured distance in miles is converted to walking distance in minutes assuming that it takes 20 minutes for an average adult to walk one mile.
Perceived Plot Distance d_{ij}^P	The respondent's guess on the time it takes to walk between two plots.
$Potential\ Good\ (Bad)\ News_{ij}$	The absolute value of the difference between the true plot distance d_{ij}^T and the perceived plot distance d_{ij}^P, if the difference is positive (negative).
T_d	A dummy variable taking the value of one if participant i was told the true distance between their largest plot and j's closest plot, d_{ij}^T.
$Good\ (Bad)\ News_{ij}$	The product of T_d and $Potential\ Good\ (Bad)\ News_{ij}$.
$Home\ Distance_{ij}$	The physical distance between the dwellings of participant i and j. The GPS-measured distance in miles is converted to walking distance in minutes assuming that it takes 20 minutes for an average adult to walk one mile.
Proposed Meeting	A dummy variable taking the value of one if participant i was offered to meet j.

$Connection_{ij}$	The extent of social connection between i and j, ranging from 0 to 13. It is the sum of 13 dummies; $Know_{ij}$, $Receive\ Visit_{ij}$, $Pay\ Visit_{ij}$, $Relative_{ij}$, $Socialize_{ij}$, $Borrow\ Money_{ij}$, $Lend\ Money_{ij}$, $Borrow\ Goods_{ij}$, $Lend\ Goods_{ij}$, $Receive\ Advice_{ij}$, $Give\ Advice_{ij}$, $Speak\ Agriculture_{ij}$, $Pray_{ij}$.
$Know_{ij}$	A dummy variable taking the value of one if respondent i reports knowing j.
$Receive\ Visit_{ij}$	A dummy variable taking the value of one if respondent i receives visits from j.
$Pay\ Visit_{ij}$	A dummy variable taking the value of one if respondent i pays visits to j.
$Relative_{ij}$	A dummy variable taking the value of one if respondent i is related to j.
$Socialize_{ij}$	A dummy variable taking the value of one if respondent i socializes with j.
$Borrow\ Money_{ij}$	A dummy variable taking the value of one if respondent i would borrow money from j.
$Lend\ Money_{ij}$	A dummy variable taking the value of one if respondent i would lend money to j.
$Borrow\ Goods_{ij}$	A dummy variable taking the value of one if respondent i would borrow goods from j.
$Lend\ Goods_{ij}$	A dummy variable taking the value of one if respondent i would lend goods to j.
$Receive\ Advice_{ij}$	A dummy variable taking the value of one if respondent i receives advice from j in matters about health, agriculture, school, personal, market prices.

Give Advice$_{ij}$	A dummy variable taking the value of one if respondent i gives advice to j in matters about health, agriculture, school, personal, market prices.
Speak Agriculture$_{ij}$	A dummy variable taking the value of one if respondent i speaks to j about agriculture.
Pray$_{ij}$	A dummy variable taking the value of one if respondent i goes to church or mosque with j.
Connectedness$_{ji}$	The average value of $Connection_{ij}$, $\forall j \neq i$. It measures the average connection between other farmers and i.
Baseline Variables	
Recipient Bought Fertilizer	A dummy variable taking the value of one if the potential training recipient bought fertilizer as part of a previous research project the author carried out with the same participant pool (Burchardi et al., 2018).
Recipient Won Lottery	A dummy variable taking the value of one if the potential training recipient won a lottery payment of 200,000 Ugandan Shillings as part of a previous research project the author carried out with the same participant pool (Burchardi et al., 2018).
Recipient is Male	A dummy variable taking the value of one if the potential training recipient is male.
Recipient's Age	The age of the potential training recipient in completed years.
Recipient's Education	The number of completed years of education of the potential training recipient.
Recipient is Literate	A dummy variable taking the value of one if the potential training recipient can read and write in any language.

Recipient's Farm Area	The total size of the land cultivated by the potential training recipient, in acres.
Recipient's Farm Area	The total size of the land cultivated by the potential training recipient in which maize is planted, in acres.
Male	A dummy variable taking the value of one if the participant is male.
Years of Education	The number of completed years of education of the participant.
Household Members	The number of people living in the participant's household.
Plot Number	The number of plots cultivated by the participant's household.
Farm Area	The area of the land cultivated by the participant's household, in acres.
Maize Area	The area of the land cultivated by the participant's household in which maize is planted, in acres.
Used Improved Seeds	A dummy variable taking the value of one if the participant's household used improved seeds on their farm in the previous agricultural season.
Used Pesticides	A dummy variable taking the value of one if the participant's household used pesticides on their farm in the previous agricultural season.
Used Organic Fertilizer	A dummy variable taking the value of one if the participant's household used organic fertilizer on their farm in the previous agricultural season.
Used Chemical Fertilizer	A dummy variable taking the value of one if the participant's household used organic fertilizer on their farm in the previous agricultural season.

Table B.I: LIST OF VARIABLES

C. Willingness-to-Pay Elicitation and Belief Shock Survey

Instructions

1. Did the Fall Armyworm affect your farm during 2018? (Yes, No)

   I would like to tell you something about the Fall Armyworm. It spreads by proximity. This means that if someone else's plot is attacked by Fall Armyworm and your plot is close-by, the Fall Armyworm will likely spread to your plot. The closer an attacked plot is to your plot, the more likely it is that the Fall Armyworm spreads to your plot too.

2. Would you be interested in learning more about the Fall Armyworm and how if affects maize and other crops? (Yes, No)

   Stockholm University is organizing a training about the Fall Armyworm. The training is about 3 hours long, it is given to a farmer individually, and the teacher is an agriculture extension worker that speaks your language. The training includes a practice session in the field. During the training, the agriculture extension worker will teach: 1) how to recognize a Fall Armyworm infestation; 2) how to prevent an infestation; 3) how to tackle an infestation; 4) how to apply pesticide and which pesticide to use.

   Maximum one farmer can receive the training about Fall Armyworm from Stockholm University in this village.

   I am now going to ask for your willingness to pay for receiving this training yourself, in the same way as we did before [Reference to practice session].

After that, I will ask for your willingness to pay for [Farmer 1] to receive the training. Then your willingness to pay for [Farmer 2] to receive the training, and then the same for[Farmer 3], [Farmer 4], [Farmer 5], [Farmer 6], [Farmer 7], [Farmer 8].

So in total, you will choose N willingness-to-pay for training. For yourself, for[Farmer 1], [Farmer 2],[Farmer 3], [Farmer 4], [Farmer 5], [Farmer 6], [Farmer 7], [Farmer 8].

You can choose from a list of 21 possible prices for the training and we will ask you whether you would be willing to pay each possible price for it. The prices range from 0 to 40,000 UGX and increase by 2,000 UGX each time. For example we will ask: "Would you buy the training for 8,000 UGX?", "Would you buy the training for 10,000 UGX?", and so on.

After you have told me your N willingness-to-pay choices, for yourself and the other farmers, I will give you a price card with your price, like this one: [Enumerator Shows Card]

This price may be 0, 2000, 4000, ..., up to 40000 UGX. The price has been randomly selected by the computer and I DO NOT KNOW IT. Note that all training prices are equally likely.

Only one farmer in this village is selected to be a Decision Maker. If you are selected to be a Decision Maker, we will randomly select one of your N choices to be the one that matters for purchasing the training.

Let's suppose you are selected to be the Decision Maker. If for example your choice that matters is your willingness-to-pay for yourself, you will purchase the training ONLY if your willingness-to-pay for training is equal or larger than the price on the price card. If your choice that matters is your willingness to pay for [Farmer 1] , you will purchase the training ONLY if your willingness-to-pay for [Farmer 1] is equal or larger than the price on the price card. Any of your choices could be selected to be the choice that matters.

[...] we understand that you may not have enough money with you today to pay for the training. Instead, after opening the price, if you can purchase the training we will ask you to sign a document promising that you will pay for the training.

You can pay using cash on training day. We will contact you 3 days before training day to inform you that we are going to collect the money. We can also give you telephone numbers you can call if you have any concerns. Notice that buying is mandatory if you are selected to be the decision maker. If the price you agreed to pay is higher than the price on the price card, you HAVE TO purchase the training paying the price you opened.

Willingness-to-Pay for Self

1. Would you buy the training for yourself 0 UGX? (Yes, No)
2. If no: are you sure you would not buy the training for 0 UGX? (Yes, No)
3. If no: would you buy the training for 0 UGX? (Yes, No)
4. Would you buy the training for yourself for 2,000 UGX? (Yes, No)
5. If no: are you sure you would not buy the training for 2,000 UGX? (Yes, No)
6. If no: would you buy the training for 2,000 UGX? (Yes, No)
7. Would you buy the training for yourself for 4,000 UGX? (Yes, No)
8. If no: are you sure you would not buy the training for 4,000 UGX? (Yes, No)
9. If no: would you buy the training for 4,000 UGX? (Yes, No)

[These questions are asked for a training price up to 40,000 UGX]

Willingness-to-Pay for Other

Before we proceed, let me ask you some questions about your relationship with [Farmer 1].

1. Do you know [Farmer 1] ?
2. Does [Farmer 1] visit your home?
3. Do you visit [Farmer 1] 's home?
4. Is [Farmer 1] your relative?
5. Do you socialize with [Farmer 1] ?
6. Would you borrow money from [Farmer 1] ?
7. Would you lend money to [Farmer 1] ?
8. Would you borrow goods like fuel/paraffin, salt, hoes, etc. from [Farmer 1] ?
9. Would you lend goods like fuel/paraffin, salt, hoes, etc. to [Farmer 1] ?
10. Do you receive advice from [Farmer 1] (health, agriculture, school, personal, market prices)?
11. Do you give advice to [Farmer 1] (health, agriculture, school, personal, market prices)?
12. Do you speak to [Farmer 1] about agriculture?
13. Do you go to church/mosque with[Farmer 1] ?
14. I would like to ask you some information about the walking distance from your plot called [Plot Name] to the closest plot of [Farmer 1]. Think about the plot of [Farmer 1] that is closest to your plot [Plot Name].

[ENUMERATOR: PAUSE HERE. LET THE RESPONDENT THINK. THIS IS A DIFFICULT QUESTION.]

How long does it take to walk from your plot [Plot Name] to the closest plot of [Farmer 1] ?

15. (ONLY IF DISTANCE TREATMENT:) You said that it takes m minutes to walk from your plot [Plot Name] to [Farmer 1]'s closest plot. According to our information, [Farmer 1]'s closest plot is M Miles from [Plot Name], which means it actually takes about t minutes to walk from [Plot Name] to [Farmer 1]'s closest plot.

    I am now going to ask about your willingness-to-pay for [Farmer 1] to receive the training.

16. (ONLY IF MEETING TREATMENT:) Please bear in mind that if you are the Decision Maker and your choice about [Farmer 1] is selected, we will give you the opportunity to meet [Farmer 1]. If [Farmer 1] receives training, the meeting will take place after [Farmer 1] has received the training. At the meeting, you and [Farmer 1] will have the chance to discuss how to handle a Fall Armyworm infestation. Notice that the meeting will take place regardless of whether [Farmer 1] receives training or not.

17. Would you pay 0 for [Farmer 1] to receive the training? (Yes, No)
18. If no: are you sure you would not pay 0 for [Farmer 1] to receive the training? (Yes, No)
19. If no: would you pay 0 for [Farmer 1] to receive the training? (Yes, No)
20. Would you pay 2,000 for [Farmer 1] to receive the training? (Yes, No)

21. If np: are you sure you would not pay 2,000 for [Farmer 1] to receive the training? (Yes, No)

22. If no: would you pay 2,000 for [Farmer 1] to receive the training? (Yes, No)

23. Would you pay 4,000 for [Farmer 1] to receive the training? (Yes, No)

24. If no: are you sure you would not pay 4000 for [Farmer 1] to receive the training? (Yes, No)

25. If no: would you pay 4,000 for [Farmer 1] to receive the training? (Yes, No) [These questions are asked for a training price up to 40,000 UGX]

[These questions are asked for each other sampled farmer in the village]

A Multifaceted Education Program for the Poor and Talented

I. Introduction

Investment in education is a private decision with potentially large lifelong returns. To alleviate the constraints to investment in education typically faced by poor households, policy makers have implemented programs that provide families with resources (in cash or in kind) to incentivize school attendance and performance (Kremer et al., 2013). One way to efficiently allocate such resources could be to target students that are very poor and very talented; poorer households, though, may face several constraints to investment in education that need to be addressed simultaneously. This paper studies an intervention that equips talented poor students with a number of resources they need to attend secondary school: the MasterCard Foundation Scholars Program (MCFSP) in Uganda. The program, implemented by the global NGO Brac, provides students with admission to an elite secondary school, fees, school inputs, a teacher mentor, and a cash transfer.

From a development policy perspective, MCFSP is a transformative program whose format enables students whose large potential would otherwise remain untapped. From the program design standpoint, MCFSP is innovative because it includes components of education and anti-poverty interventions that have not been implemented jointly before, but proved individually effective. Because there were more students applying to the program than available slots, a subset of applicants was assigned to either a treatment or a control group. Applicants assigned to the treatment group could receive the full program, while applicants assigned to the control group could not receive any program component. In this article I exploit the randomized assignment of applicants to the treatment or control group to provide experimental evidence of the short-term (2-year) impacts of MCFSP on the schooling outcomes of the beneficiaries and on the socioeconomic wellbeing of their households.

After being in the program for two years, the total value of the benefits received by beneficiary households is \$6,735[1]. MCFSP provides admission and fees to enrol in an elite secondary school whose average 2-year value is \$4,090. Research evidence from low to middle income countries shows that this is beneficial for students' test scores (Jackson, 2010; Dustan et al., 2017). It also provides students with textbooks, that mostly benefit the best students (Glewwe et al., 2009), and inputs such as notebooks, backpacks, calculators and others for a total value of \$658. Students who enrol in MCFSP also have access to a teacher mentor, similarly to the Balsakhi program evaluated by Banerjee et al. (2007) that found large improvements in the test scores of under-performing students. The value of the extra tutor hours students have access to through MCFSP is \$405. The cash transfer component of MCFSP is conditional on enrolment and represents a substantial share of the beneficiaries' family income, with a 2-year value of \$1,582. This is similar to conditional cash transfer programs (CCT) implemented in other developing countries such as Progresa/Oportunidades in Mexico or Bolsa Escola in Brazil, which have improved child health, education, and the economic wellbeing of their households (Baird et al., 2014; Fiszbein et al., 2009). The cash is delivered by bank transfer to a savings account that households are required to open when signing up to the program; it is co-signed by the beneficiary and one guardian, typically the mother, who then have full access to the account.

Besides resembling a collection of successful education programs, MCFSP also shares similarities with graduation-out-of-poverty programs (henceforth graduation programs), pioneered by Brac with the "Targeting the Ultra-Poor" (TUP) ini-

[1] All US Dollar values are expressed in 2018 PPP values from the World Bank Databank.

tiative (Bandiera et al., 2017). Graduation programs include provision of a productive asset (typically, livestock), consumption support, skill training, access to a savings account and other health and education services. Evidence discussed by Banerjee et al. (2018) shows that multifaceted programs like TUP effectively graduate the poor out of poverty by combining interventions targeted at relaxing multiple constraints simultaneously. Banerjee et al. (2018)[2] also discuss evidence that relaxing one constraint at a time with targeted interventions may not be enough to set the poor on a trajectory of capital accumulation. Similarly to graduation programs, MCFSP provides a productive asset, in the form education for one child, and consumption support through a cash transfer (deposited on a savings account participants are required to open, similarly to some of the graduation programs studied by Banerjee et al. (2018)). A key question this study tackles is whether policy makers can effectively combine anti-poverty components with large human capital accumulation interventions.

All potential beneficiaries file an application to MCFSP. When Brac receives the application, applicants go through a 2-step selection process. In the first step, applications are shortlisted according to three criteria (primary school mock exam score, head teacher's assessment of their family income, and head teacher stamp on the application). Brac then collects further information on shortlisted applicants and advances them to the second stage. In the second stage, an expert committee evaluates shortlisted applicants according to three criteria (primary school exam score, a researcher-assessed poverty indicator, and whether applicants showed up for an in-person interview). The RCT sample is selected after the shortlisting step; of all shortlisted applicants, a subset is selected to be part of the experiment. Within the RCT sample, applicants in the treatment group are advanced to the second step of the selection process, while students in the control group are not. This means that students assigned to the control group cannot receive the program, while students assigned to the treatment group can receive the program if they pass the second selection step (48% of the students originally assigned to the treatment

[2]They include interventions in microfinance, education, business training and capital grants to entrepreneurs.

group win the scholarship). The second selection step can be almost perfectly replicated on the control group. Two of the three criteria used in the second selection step are available for the control group (primary school exam score and researcher-assessed poverty indicator); but applicants in the control group were not invited for a district interview, so the researcher cannot know whether they would have showed up to the interview if invited. Since showing up for the interview is an endogenous choice, it may induce differential sample selection and make the treatment group less comparable to the control group. To prevent sample selection, I restrict my analysis to the group of *eligible students*, defined as the applicants who pass the replicable part of the second selection step, both in the treatment and the control group. For this reason, the estimates in the main body of the article are intention-to-treat (ITT) estimates. When I replicate the second selection step on the treatment group, I see that 83% of the eligible students actually win the scholarship: predicted take-up aligns well with actual take-up. I also provide instrumental variable (IV) estimates in which treatment assignment is an instrument for winning the scholarship. Instrumental variable estimates are based on the sample of eligible students as well, and provided in appendix.

I find that MCFSP is effective at increasing enrolment and test scores of beneficiaries. Enrolment increases by 10 percentage points in the treatment group, and test scores in a national exam increase by 0.36 standard deviations. The impact on test scores is larger than most other education programs (see Baird et al. (2014) and Kremer et al. (2013) for comprehensive reviews). As a point of comparison, Dustan et al. (2017) find that being admitted to an elite high school in Mexico city increases test scores by 0.17 standard deviations on average; Baird et al. (2011) find that a conditional cash transfer in Malawi increases math test scores by 0.12 standard deviations and English test scores by 0.14 standard deviations; Glewwe et al. (2009) find that providing textbooks to students in Kenya increases test scores of the top quintile students by 0.22 standard deviations; Banerjee et al. (2007) find that a remedial education program increases test scores by 0.28 standard deviations.

The program has an impact on the economic lives of other family members besides the direct beneficiary. Two years after the program began, households accumulate productive assets such as land (the effect size is $1213), durables ($109), small animals ($8 chickens, $18 goats, sheep and pigs) and monetary savings ($22). Nutrition improves in terms of dietary diversity (0.16 standard deviations), number of meals (0.09 standard deviations) and protein consumption (6 percentage points more likely to consume protein rich foods). The impacts on nutrition are especially important due to its positive short and long-term consequences on physical and cognitive ability (Strauss, 1986; Chakraborty and Jayaraman, 2019; Baird et al., 2019). Mental health of respondents improves in terms of reduced depression (0.25 standard deviations), increased life satisfaction (0.29 standard deviations) and happiness (0.33 standard deviations). These effects are comparable in size with those obtained by a large unconditional cash transfer implemented by GiveDirectly in neighboring Kenya (Haushofer and Shapiro, 2016) and a set of graduation programs implemented in Ethiopia, Ghana, Honduras, India, Pakistan, and Peru (Banerjee et al., 2018).

MCFSP has large positive impacts on many aspects of the socio-economic lives of the beneficiaries. To understand how MCFSP fares compared to other education interventions, I perform a cost-effectiveness analysis. Following Kremer et al. (2013), I calculate the number of additional standard deviations of impact that the program achieves with $100. If I focus on the impact on test scores, which allows for cross-program comparisons, I find that $100 afford a 0.01 standard deviations increase in test scores. This makes MCFSP the most expensive program among the education interventions for which cost-effectiveness data is available. MCFSP, though, affects many dimensions of a beneficiary's life besides school outcomes; assuming that the main impact of MCFSP is captured by test scores understates its large impacts on child and household welfare.

This study has a number of shortcomings. First, the experimental design does not allow me to investigate the contribution of each program component individually. An alternative design assigning different subsets of components to randomly selected participants would have allowed to gain a better understanding of the contribution of each to the final results, and to test for complementarities across components. Power concerns made such an alternative design infeasible. Second, the households in this study are a selected sample of the Ugandan population: I only observe households who applied to the MCFSP program. It is reasonable to imagine that such households differ from those who did not apply to the program along dimensions such as value placed on children's education or knowledge of Brac's programs. Further, because applicants have to visit a Brac office and fill an application form to qualify for the program, poor households may self-select out of the process. Third, I find evidence for differential attrition by treatment status. RCT participants assigned to the treatment group are more likely to be found and interviewed at follow-up than controls. This may be due to a number of factors, including participants in the treatment group being more willing to engage with program staff out of reciprocity, being easier to locate for program staff, being less likely to migrate or more likely to have an active cellphone connection because of the program.

In what follows I provide some background information on the education system in Uganda (Section II), describe the MasterCard Foundation Scholars Program (Section III), provide details on the experimental design, the empirical strategy and the data (Section IV, V and VI respectively), and discuss results (Section VII). Section VIII describes the cost-effectiveness analysis. Section IX concludes.

II. Setting: Education in Uganda

Uganda is a landlocked country in Eastern Africa bordering with DR Congo, Kenya, Rwanda, Tanzania and South Sudan. It has a GDP per capita of $643[3] (1% of US GDP per capita) growing at 2.2% yearly (same as the US). Its population size is 42.7 million, of which 47.4% is below 15 years of age, making it the third country in the world in terms of share of youth (World Bank).The high youth-to-population ratio makes education a top priority for the country's future growth, as acknowledged by the government (State House of Uganda, 2019[4]).

[3]Macroeconomic data and enrollment data are from the World Bank Databank.
[4]statehouse.go.ug

Primary school is free in government-run schools since 1997. Enrollment is 91%, substantially higher than neighboring DR Congo, Kenya, South Sudan and Tanzania (at 37, 82, 32 and 80% respectively) but lower than Rwanda (94%), with a 51% completion rate. Secondary school enrollment is much lower at 22% (higher than DR Congo and South Sudan but lower than Kenya, Rwanda and Tanzania), and secondary school completion rate is 25%. Tertiary enrollment, in university and vocational/technical institutions, is 5% and comparable to its neighboring economies (with the exception of South Sudan).

Typically, the school year starts in February and ends in December. Ugandan pupils undergo seven levels of primary education (Primary 1 to Primary 7) ending with the Primary Leaving Examination (PLE). The PLE is a national test graded 4 (best score) to 36 (worst score); successful PLE writers are classified in 4 divisions depending on their score. Scoring 4 to 12 in the PLE examination grants a First Division PLE, typically achieved by about 10% of exam writers.

Secondary school has two stages. The Ordinary levels extend from Senior 1 to Senior 4 and ends when students obtain the Uganda Certificate of Education (UCE) after passing a national exam at the end of Senior 4; the Advanced levels extend over Senior 5 and Senior 6, ending with the Uganda Advanced Certificate of Education (UACE) exam.

Tertiary education includes universities and vocational/technical institutions. Universities award degrees, while vocational/technical institutions award certificates (3-year programs) or diplomas (4-year programs). UACE is necessary to enter programs offering a degree or diploma, while typically UCE is required to attend programs awarding a certificate.

III. The MasterCard Foundation Scholars Program

The MasterCard Foundation Scholars program (MCFSP) [5] aims to achieve positive social transformation in Africa by supporting talented students with a disadvantaged background throughout their post-primary education. The Program deliv-

[5] mastercardfdn.org

ers financial, social and academic support to Scholars throughout their secondary and university education, and post-graduate transition. Currently the program is active in several African countries including Cameroon, Ethiopia, Ghana, Kenya, Malawi, Rwanda, Senegal, South Africa, Tanzania, Uganda. It is supported by 32 implementing institutions, including top performing Universities in Africa, North and Central America, and the Middle-East.

III.A. Program Implementation in Uganda

In Uganda, the program is implemented by Brac through dedicated managers, officers and program assistants. It enrolled about 5,000 students between 2013 and 2017, and at the time of writing is in the phase of supporting scholars transition to tertiary education. The total funds committed for MCFSP in Uganda is $47.8M. About 70% of the enrolled students have been admitted to the program in their Senior 1 year after passing the PLE exam, and are financed for six years (S1 to S6). The remaining 30% entered in their Senior 5 year after passing the UCE exam, and are financed for two years (S5 and S6).
The program has the following components :

- **Admission to top-100 secondary school** guaranteed by ad-hoc agreements between Brac and each institution;

- **Full tuition coverage** whose specific amount depends on the school where the student is placed. Tuition is paid to the schools directly from Brac accounts;

- **School inputs** including notebooks, pens, pencils, a math set, a calculator, a backpack, sanitary pads, t-shirts, a mattress, textbooks, for a total value of 759,800 Ugandan Shillings ($658);

- **A teacher mentor** that the student can meet once a week;

- **A monthly stipend** of $66 PPP (50,000 Ugandan shillings) until completion of secondary school. The stipend is transferred quarterly on a savings account co-signed by the student and one parent, typically the mother or other female guardian.

III.B. Data

For selection and evaluation purposes, Brac collects applicant data in four ways: through an application form, a survey, a district interview, and by obtaining test score data from the ministry. Data is sometimes collected differently for students in the RCT sample, as specified below in parentheses. The beneficiary selection process - starting when the applications are open and ending with the announcement of the beneficiaries - takes place between September and Mid-February for every intake year.

The data for this study comes from the following sources:

1. **Application Form:** Students and/or their guardians apply to MCFSP by filling a free application form available at all Brac field offices (see Appendix D) between September and November.

2. **Household Survey (non-RCT sample) or Baseline Survey (RCT sample):** A student's guardian is interviewed at home by a Brac enumerator. Female guardians are given priority, and a male guardian is interviewed only if a female could not be found. The main purpose of this survey is to collect household poverty indicators. The household survey is administered to the non-RCT sample and is shorter than the baseline survey administered to the RCT sample. The survey is administered between December and January.

3. **District Interview (non-RCT sample and treatment group in RCT sample):** Students sit an interview with education officials in the district capital. Officials judge students on four criteria: academic qualification, financial need, leadership ability, and other. Each of the first three categories is scored out of ten, while the "other" category is scored out of five. Each official scores the student on the four categories, and then the scores are averaged across officials. A student's final score is the sum of average scores across these

categories, the highest possible score being 35. Interviews take place in January.

4. **PLE Score Retrieval:** Brac retrieves students' Primary Leaving Examination (PLE) scores from the Ministry of Education using the 9-digit unique code (PLE index) students are assigned when they sit the PLE. Students provide Brac with the PLE index in the application form. Students sit the PLE in December; results are published by the Ministry during the following February and retrieved as soon as they are available.

5. **Follow-up Survey (RCT sample only):** Brac collects two-year follow-up data for RCT participants in both intake cohorts. The follow-up survey is identical to the baseline survey, with the addition of psychological wellbeing measures. It is administered in January.

6. **UCE Score Retrieval (RCT sample only):** Brac retrieves students' Uganda Certificate Exam (UCE) scores from the Ministry of Education using the 7-digit unique code (UCE index) students are assigned when they sit the UCE. Students provide Brac with this code in a phone survey. Students sit the exam in December at the end of the Senior 4 year of high school. Passing the UCE exam allows students to complete their Ordinary levels and progress to Senior 5.

III.C. Selection

All students who have recently completed primary education in Uganda can apply to the program. Since the number of applicants largely outnumbers the yearly intake capacity of the program, the data collected from applicants informs the following two-stage selection process.

1. **Shortlisting:** Applications are digitized and shortlisted according to the following criteria:

    a) PLE mock score of 12 or below;

   b) Application form has been stamped by the Head Teacher;

   c) Applicant is rated as "needy" or "very needy" by the Head Teacher in the application form;

2. **Committee Selection:** A committee composed of education experts from the Ministry of Education and high profile education stakeholders meets and selects the final scholarship winners, whose names are published on the three main national newspapers the day after. Selection is done according to the following criteria:

   a) PPI® score[6] below 60 for boys and below 65 for girls in 2014, below 59 for both in 2015;

   b) PLE score 12 or lower for boys and 17 or lower for girls in 2014, 12 or lower for both in 2015;

   c) The applicant showed up for the district interview.

Figure III provides a detailed selection timeline. The next section provides further details about the RCT sample selection.

IV. Design

The evaluation is designed as a randomized controlled trial (RCT) through a three party agreement between the funder MCF, Brac [7], and the research organization Mathematica Policy Research. Brac and Mathematica agreed to evaluate the program using the same RCT methodology, sample and treatment assignment, but have performed data collection independently. This article is based on data collected by Brac.

RCT participants are selected from the pool of applicants who passed the *Shortlisting* stage in the intake rounds of 2014 and 2015 (see III.B). The randomization

[6]The PPI® is a country-specific tool developed by Mark Schreiner to measure the likelihood that a household lives below a certain poverty line (see www.progressoutofpoverty.org). To calculate a household's PPI® score, we ask the main respondent 10 questions specified in the PPI scorecard and sum the points assigned to each answer.

[7]The author was a research associate at Brac when the evaluation was designed in 2013.

algorithm assigns a subset of shortlisted applicants to a treatment or control group, stratifying by district of origin and gender. As Figure I shows, 2,328 applicants are part of the RCT sample. Typically, only one child per household can apply to the program and I do not observe siblings in the pool of shortlisted applicants.

After randomization, applicants in the RCT sample are administered a baseline survey. Applicants assigned to the treatment group are invited to sit a district interview while applicants in the control group are not, for ethical and budget considerations. Applicants in the treatment group then undergo the *Committee Selection* stage, while applicants in the control group do not. Members of the committee do not examine the files of applicants assigned to the control group, so they cannot be selected by design. Students in the treatment group who are selected in the *Committee Selection* stage receive the program. The screening happening in the *Committee Selection* stage can be partially replicated on the control group. Criteria (a) and (b) in section III.B can be applied to the control group because Brac has collected the poverty scores and PLE scores of both the treatment and the control group. Criterion (c) cannot be applied to the control group, because members of the control group were not invited for a district interview. Showing up for the interview is an endogenous choice, so failing to apply this selection criterion to the control group may make it less comparable to the treatment group. To prevent sample selection arising from an endogenous interview show-up choice, my analysis includes all RCT participants who satisfy the poverty score and PLE score criteria, disregarding whether they showed up for the interview or not. I refer to the RCT participants in the treatment and control group who pass the replicable part of the second selection step as *eligible students*; as shown in Figure I, there are 1,425 eligible students in total across cohort. My analysis is carried out on the 1,325 eligible students that Brac is able to locate at follow-up. Hence, the estimates in the body of the article are intention-to-treat (ITT) estimates. I provide IV estimates in Appendix B, in which I instrument actual program assignment (receiving the scholarship) with treatment assignment.

V. Data and Sample Characteristics

For each of the intake cohorts included in the evaluation (2014 and 2015), Brac collects application data, baseline survey, interview scores, PLE official scores from the Ministry of Education and a two-year follow-up survey. UCE official scores were collected for the first intake cohort only.

Table I shows descriptive statistics and the results of balance tests in the sample of eligible students. Column 1 reports the mean and standard deviation of selected baseline characteristics. The average family in the sample has seven members, and about half of these are children of schooling age. The average parent has at least some primary education; mothers have 6.3 years of education and fathers 8.6. The labor income of household members amounts to $130 per month in total, and the household spends $157 per month on average. The typical household lives below the $2,50 per day poverty line (the average poverty score is 29, translating into a 96% likelihood to be living below the $2,50 per day poverty line). In 11% of the households the applicant was working before applying to the scholarship program, and 26% of homes have a thatch roof. These poverty indicators are in line with the targeting objectives of the donor to reach poor households that value education. In terms of program outreach, it seems that the communication strategy of the program successfully reached potential applicants outside the Brac network. Only about 10% of the applicants come from households involved in Brac anti-poverty programs (microfinance, adolescent empowerment, agriculture).

Column 2 of Table I reports the results of a regression of each characteristic on a treatment indicator, and column 3 reports the standardized difference between treatment and control group. The only significant difference between the two groups is the applicant's mother's age; mothers of applicants in the treatment group are one year younger than in the control group.

Table II investigates whether the treatment groups exhibit differential attrition. I regress an indicator equal to one if a household was not found at follow-up on a treatment indicator. The first column shows results on the whole RCT sample for which I have baseline data, while the last three columns report results on eligible students only. Column 2 shows that eligible students in the treatment group are 12% less likely to not be found at follow-up. Column 3 and 4 document that differential attrition is larger in the second cohort (14.4%) than in the first cohort (8.6%). To address the potential bias induced by differential attrition, I provide two sets of results. First, Table XII shows that including non-responders in my balance tests does not affect the composition of the sample at baseline. Second, in Tables A.I to A.VIII I repeat my analysis including imputed values for non-responders. I show that my estimates are robust to different assumptions about where non-responders lay in the outcome distribution.

VI. Empirical Strategy

Following Mckenzie (2012), the preferred specification is the following ANCOVA:

$$y_{hf} = \alpha + \beta_1 \, T_h + \beta_2 y_{ib} + g_h + \sum_{j=1}^{n} d_j + c_t + \varepsilon_h \tag{1}$$

where y_{hf} and y_{hb} are outcome values for household h at follow-up f and baseline b, and T_h is a dummy equal to 1 if the applicant from household h was assigned to the treatment group.

As suggested by Bruhn and Mckenzie (2009) I control for stratification variables by adding fixed effects for applicant gender g_h and district d. I also include cohort fixed effects c_t, $t = (2014, 2015)$ to control for potential differences between the intake of 2014 and that of 2015 due to time, differences in selection criteria or learning by Brac staff. Since randomization is at the household level, I do not cluster standard errors when the outcome is at the household level. When the outcome is at the household-member level I cluster standard errors at the household level. Some outcomes are measured only at follow-up, in which case the estimating equation is:

$$y_{hf} = \alpha + \beta_1 \, T_h + g_h + \sum_{j=1}^{n} d_j + c_t + \varepsilon_h \tag{2}$$

which does not control for baseline outcomes y_{ib} but is otherwise identical to (1). In instrumental variable estimates I instrument the endogenous "Won Scholarship" indicator with the randomized treatment assignment indicator T_i using the following first stage equation:

$$Won\ Scholarship = \alpha + \beta_1 T_h + g_h + \sum_{j=1}^{n} d_j + c_t + \varepsilon_h \tag{3}$$

Many outcomes have a long right tail, and some of these large values are potentially due to enumeration errors. Since large outliers will affect treatment effects, I top-code all monetary values at the 99th percentile.

VII. Results

Figure II presents a summary of results in standard deviations of the control group using specification (2). Two years after the winning child is enrolled in the program, households in the treatment group have experienced substantial improvements in their wellbeing. The value of land, durables and savings are significantly larger. Expenditures per capita increase, although not significantly, and households register a large improvement in their food consumption as measured by indices of food access (measured by the dietary diversity score) and number of meals consumed. The program has large impacts on the mental health of mothers: their psychological wellbeing index increases by 0.33 SD. The program does not seem to affect conflict or violence at home, which were relatively low at baseline. Women's ability to decide on how to spend household resources (Expenditure Decision Index) and their empowerment (Empowerment Index) are also not significantly affected by the program. When it comes to impacts on education, the program has large effects on the beneficiary child while the evidence is mixed for siblings. Scholarship winners have higher enrollment rates than children in control group (0.28 SD) and perform better in the national O-level exam (0.36 SD).

Table III to Table X show program impacts grouped by category. There are three kinds of outcome variables: monetary values, z-scores, and binary variables. Monetary variables are expressed in \$PPP terms; the coefficient size (shown in the "Treatment" column) measures the average increase in the value of the outcome experienced by the treatment group. Z-scores are calculated by subtracting from each outcome variable the mean of the control group and dividing by the standard deviation of the control group. I calculate z-scores for outcomes that are originally categorical variables, to make results more interpretable. Outcomes in z-scores always have mean equal to zero and standard deviation equal to one; their labels in the tables always includes the word "Score" for clarity. The coefficient size of outcomes expressed in z-scores measure the average increase in standard deviations of the outcome variable in the treatment group.

Unless otherwise specified, the coefficients reported in the "Treatment" column of Table III to Table X are β_1 coefficients from the estimation of specification (1). The impacts on outcome variables marked with $(^+)$ result from estimating specification (2). I estimate specification (2) if the outcome was not collected at baseline (it is the case for the variables measuring psychological wellbeing of mothers in Table IX and whether the student was attending a boarding school in Table X), or if the outcome had not realized yet at baseline (as is the case for the UCE exam score in Table X), or if the outcome does not vary at baseline (as is the case for the enrollment indicator in Table X, since all applicants need to be enrolled in primary school at baseline).

Table A.I to Table A.VIII report, for each outcome variable, bounds that adjust for differential attrition between the treatment and control group. For non-responders I impute, within experimental group (treatment or control), the mean minus (plus) a specified standard deviation multiple of the observed distribution of each outcome in that experimental group. I then re-estimate the treatment effects in the sample including imputed data to find their lower (upper) bounds. Across all outcomes, estimates maintain their size and significance levels when I perform a 5, 10 or 20% imputation.

Finally, Table B.X to Table B.XVII report instrumental variable estimates. Within the sample of 1,325 eligible applicants, I use the randomized treatment assignment as an instrument for winning the scholarship. The first stage, presented in Table

B.IX, shows that being assigned to the treatment group increases the likelihood to receive the scholarship by 83 percentage points ($F - stat = 47.18$).

Table III reports the effects on land, durables, livestock and savings value. The largest impacts are on reported land value with a $1,209 increase compared to controls, significant at 10% level. The value of durables increases by $112, mainly driven by an increase in the value of vehicles, furniture and appliances as Table IV reports. The effect on livestock values is small ($34) and insignificant overall, but Table V reports that households in the treatment group invest significantly more in small animals ($18) and chickens ($8). This behavior suggests that households may be shifting their livestock composition to be more business-oriented similarly to what Banerjee et al. (2018) observe. Savings kept in banks, cooperatives or at home increase by about 30% compared to the control group.

The program does not seem to affect expenditures substantially. Table VI reports expenditures of the past 30 days on food, personal items and fuel. There are modest increases in food ($9) and personal ($3) expenditures (such as clothing, hairdressing, airtime etc.). Fuel (including electricity) expenditures are a very small part of the monthly consumption bundle for the control group ($7) and they are not significantly affected by the program.
Even though the effect on reported food expenditure is not large, treatment households' diet improves substantially. Table VIII reports a 9% standard deviations increase in the number of meals eaten by the household and a 16% standard deviations increase in dietary diversity. Here I measure dietary diversity with a tool created by the Fanta project at USAID[8]. Dietary diversity is a proxy for food access; the non-standardized variable ranges from 0 to 12. It is calculated by adding up the number of unique food categories consumed by household members over a given period out of 12 possible categories (grains and cereals, roots and tubers, vegetables, fruits, meat, eggs, fish and seafood, pulses/legumes and nuts, milk and milk products, oils and fats, sugar and honey, condiments and coffee/tea). I also investigate whether households consume more meat, fish, and vitamin A-rich

[8] fantaproject.org

foods, since these foods contain nutrients that are particularly important for long term nutrition. Looking at dummies for each of the 12 categories reveals a significant 6% increase in the number of households consuming fish and meat over the reference period, but no change in vitamin A-rich foods. Table VII reports program impacts on the economic activities run by household members. After two years of being in the program, there is no change in the number of activities households run or in their revenues; coefficients are positive but not significant. If anything, the amount of hours worked by the survey respondent in household businesses decrease.

Table IX reports the effects of the program on measures of psychological wellbeing of the respondent, namely depression, happiness and life satisfaction. Impacts are expressed in standard deviations and are quite large in magnitude. The program generates a 25% decrease in depression measured by the CES-D, a 20-item measure recognized by the American Psychological Association and used to identify individuals at risk for clinical depression. I also observe a significant increase in self-reported happiness and life satisfaction[9] by 33% and 29% respectively. These three measures are combined in a psychological wellbeing index (reported in Figure II) following Anderson (2008) and Haushofer and Shapiro (2016).

Regarding schooling outcomes, I report impacts on education of the applicant in Table X and on education of siblings in Table XI. Both baseline and follow-up surveys collected information over the enrollment status and school characteristics for children of schooling age. As shown in Table X, the program increases enrollment in secondary school by 10 percentage points. 87% of control applicants are enrolled in secondary schools compared to 97% of treatment applicants, and 95% of treatment applicants are attending boarding school compared to 70% of control applicants (boarding school is usually considered of higher quality compared to day school in Uganda). There is no impact on the type of school attended (private or government-run). In addition to the survey data on enrollment and school characteristics, I have information on UCE scores (the Ordinary levels exam) of

[9] The life satisfaction measure has been included in the follow-up survey of the intake cohort of 2015, so it is missing for the previous cohort.

previous applicants. Brac contacted previous applicants by phone and was able to retrieve the score obtained in the UCE exam of a subset of pupils of the 2014 intake cohort, who sat the exam at the end of 2017. Even though I could match only 372 out of 569 RCT participants from that cohort, I observe a significant 36% SD increase in test scores among applicants in the treatment group. These indicators reveal that the program is largely effective at improving the education of participants. Program effects on employment and enrollment in tertiary education are not available because participants from the first intake cohort completed secondary education at the end of 2019 at the earliest.

Table XI reports program's effects on the siblings of the applicant aged 6 to 18 who were students at baseline. Panel A reports results for the whole sample of 1,695 siblings, Panel B and C report results for siblings enrolled in primary or secondary school at baseline, respectively (1,476 siblings were in primary school and 219 in secondary school). Panel A shows that over the whole sibling sample, enrollment does not change. Siblings are also no more (or less) likely to attend private school at follow-up. School fees paid for siblings are larger at follow-up, increasing on average by $41 (significant at the 10% level). The pooled results hide heterogeneity across the two types of siblings. Panel C of Table XI shows that older siblings benefit much more than younger siblings (panel B) in terms of amount of fees paid ($207 more than comparable siblings in the control group) and whether the school is private or government-run. Overall, results suggest that older siblings of applicants who receive the scholarship are attending higher quality schools than older siblings in the control group. I explore this pattern further in Figure V, showing the impacts on school fees by school grade attended by the sibling. Estimates get noisier for higher school grades because of fewer observations, but nonetheless all point estimates for secondary school students are positive while primary school students have substantially lower (if not negative) impacts.

Regarding child work, I do not observe a substantial change in child labor in either the pooled or the splitted sample (not reported). Child labor was anyways low at baseline (less than 3%).

VIII. Cost-effectiveness Analysis

In addition to the absolute size of program impacts, policy makers and donors may be interested in the relative performance of a program compared to other policy options. Cost-effectiveness analysis allows for a comparison of programs carried out in different contexts and points in time. The final goal of a cost-effectiveness analysis is to calculate how much impact can be obtained with a certain amount of costs, or how much does it cost to achieve a certain impact (see Dhaliwal et al. (2012) for a general framework on how to perform a cost-effectiveness analysis). The purpose of this section is to give an overview of the process and results; detailed calculations are available upon request.

Cost-effectiveness analyses can be performed on one outcome at a time. This makes cost effectiveness an ideal metric for interventions that are designed to address one specific issue; it is less ideal for interventions that aim at having impacts in different dimensions of their beneficiaries' lives like MCFSP. The outcome of choice here is test scores (UCE scores), because it allows for a direct comparison with the education programs analysed in Kremer et al. (2013)[10]. The cost-effectiveness of a program is measured as:

$$Additional\ SD\ per\ \$100 = \frac{100}{Cost\ Per\ Additional\ SD} \tag{4}$$

where,

$$Cost\ Per\ Additional\ SD = \frac{Total\ Cost}{Impact\ Estimate\ *\ Total\ Beneficiaries} \tag{5}$$

Total Cost includes costs born by the beneficiaries and costs born by the implementer. Since the program does not have application fees and does not require collection of fees from participants at any stage, the costs born by beneficiaries only include the imputed foregone income of parents due to time-consuming program-related activities (such as preparing the application, sitting the household inter-

[10]I use the same cost-effectiveness methodology as Kremer et al. (2013), based on documentation available at www.povertyactionlab.org.

view, accompanying their child to the selection interview, and attending program onboarding meetings). The data used for imputation comes from the baseline survey. Implementer costs include costs sustained to select beneficiaries, amount of school fees provided, value of the school inputs provided, value of cash transfers, value of teacher mentor hours provided, and salaries paid to Brac staff. Research costs are not part of the implementation, so are not included. The base year for my calculations is 2014, the year the program started; the year of analysis is 2018. All cost items are converted to their present value in 2014 USD using a 10% social discount rate, and then converted to 2018 USD.

The $Impact\ Estimate$ I use is the instrumental variable estimate of the impact on test scores, measured by $UCE\ Score$, in Table B.XVII. The coefficient size is 0.39 with a standard deviation of 0.10. The upper bound of the 90% confidence interval of the estimate is 0.56, the lower bound is 0.23.

The number of $Total\ Beneficiaries$ is equal to 2,934; in my calculations I also employ the number of total applicants (31,731) and the number of shortlisted applicants (7,026).

With the cost information available at the time of writing, the cost for one additional standard deviation of test scores is \$9,661, so additional \$100 provide 0.01 additional standard deviations of impact. Figure IV shows how the cost effectiveness of MCFSP compares to similar education programs with a significant effect on test scores analysed in Kremer et al. (2013). All the programs I include here have at least one feature in common with MCFSP. The intervention referred to as *Minimum Cash Transfer, Malawi* is studied by Baird et al. (2011) and its similarity with MCFSP is the cash transfer component. The intervention referred to as *Girl Merit Scholarships, Kenya* is studied by Kremer et al. (2009) and its similarity with MCFSP is the school input component and the fees component. The intervention referred to as *Books for Top Students, Kenya* is studied by Glewwe et al. (2009) and its similarity with MCFSP is the textbooks component. The intervention referred to as *Extra Teacher + Tracking, Kenya* is studied by Duflo et al. (2011, 2015) and its similarity with MCFSP is the access to smaller class sizes. The intervention referred to as *Remedial Education, India* is studied by Banerjee et al. (2007) and its similarity with MCFSP is the extra tutoring hours component. The top panel of Figure IV shows each program's impact on test scores in standard deviations with a 90% confidence interval. The bottom panel shows the cost per additional standard deviation of each program. The top row of each panel refers to MCFSP. The top panel shows that MCFSP is the best performing program in terms of absolute impact, with a point estimate about twice as large as most other programs. The bottom panel shows that MCFSP is the worst performing program in terms of cost effectiveness, which varies substantially across programs (note that here I use an arithmetic scale, while Kremer et al. (2013) use a logarithmic scale). Overall, the short-term impacts of MCFSP suggest that multi-faceted programs are not a cost-effective option to improve test scores compared to other policy options.

IX. Conclusion

This article has investigated two research questions. The first asks whether an education program that relaxes multiple constraints simultaneously actually outperforms programs that relax one constraint at a time. The second investigates whether it is effective to combine education and antipoverty programs in one intervention.

To answer these questions, I study the MasterCard Foundation Scholars Program in Uganda, a scholarship program for students entering secondary school. The program has several components: admission to an elite secondary school, fees, school inputs, a teacher mentor, and a cash transfer. Typically, beneficiaries enroll in the program when they start secondary school and stay in the program until graduation, for six years in total. To join the program, students have to go through a strict two-step selection process aimed at selecting very talented students (top 10% performers) coming from a disadvantaged economic background (the household is very likely to live under the $2.50 per day international poverty line).

To identify causal impacts, I exploit the randomized assignment of a subset of the applicants to a control or a treatment group during the two-step selection process. Both groups go through the first step of the selection process, but only students in the treatment group are allowed to go through the second step of the selection process. The implication is that some students in the treatment group eventually win the scholarship, while students in the control group cannot win it by design. The second step of the selection process is based on a set of objective measures; this allows me to replicate it on the control group and identify which students would have won the scholarship had they not been assigned to the control group. My analysis compares the outcomes of these students to those in the treatment group that have passed the selection process, 1,325 students in total. The experimental sample includes two contiguous cohorts of students: those who were supposed to start secondary school in 2014, and in 2015.

I present the short-term program impacts based on two-year follow-up data. Students who have been in the program for two years have received the equivalent of $6,735 in program benefits (including the value of school fees, school inputs, mentor hours and cash transfers). I find that the program substantially improves students' enrollment and O-level exam scores; impacts are larger than similar single-component education interventions providing either cash transfers, school inputs, or a teacher tutor. I replicate the cost-effectiveness analysis carried out by Kremer et al. (2013) and find that MCFSP is not as cost-effective as single-component education interventions. Additional $100 spent on MCFSP yield a 0.01 additional standard deviations increase in test scores, while other similar single-component programs yield 0.06 to 3.07 additional standard deviations increase in test scores. Cost effectiveness at increasing test scores may not be the ideal metric for a multiple-component program like MCFSP, though. The program aims at positively transforming the lives of its beneficiary and has, indeed, large impacts on family welfare. Beneficiary households accumulate productive assets such as land, durables and animals, and report larger savings. Nutrition, a proxy for long term health, improves substantially. The psychological wellbeing of the household's main female increases significantly in terms of reduced depression, higher levels of reported happiness and higher levels of reported life satisfaction.

My results indicate that a multi-component education program was successful at both improving education outcomes of children and the socioeconomic conditions of their households. Further research on the long-term economic impacts of the program is needed to understand whether the program effectively sets beneficiary households on a sustained capital accumulation path. In addition, policy makers would benefit from a deeper understanding of the complementarities between education and antipoverty interventions. If complementarities exist, they could be harnessed to achieve different development objectives simultaneously and with potentially lower costs than individual interventions.

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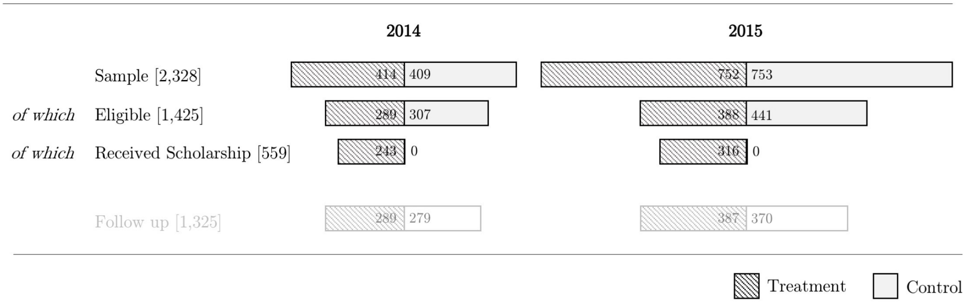
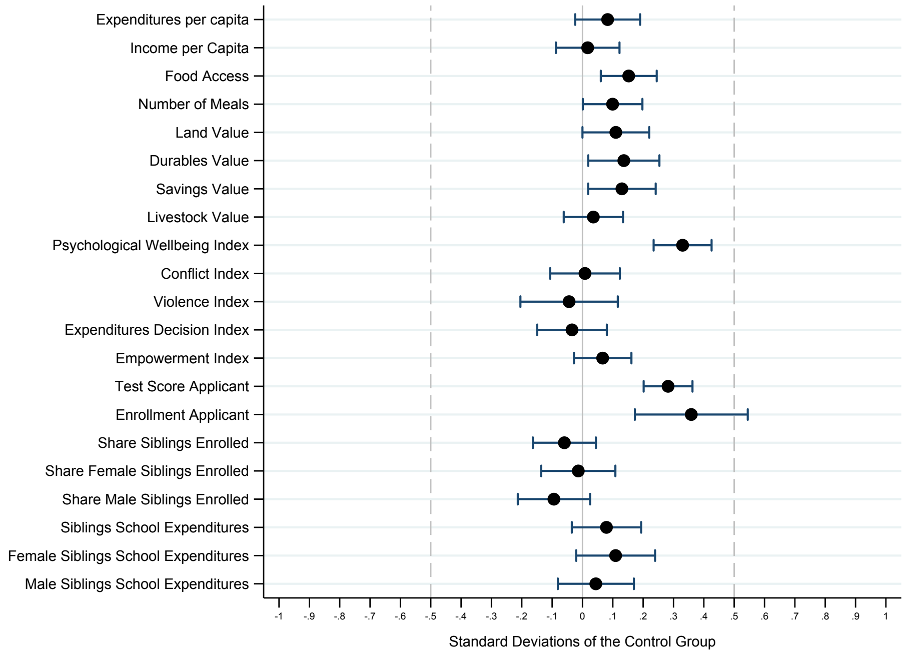
Figures

Figure I: SAMPLE SIZE BY COHORT



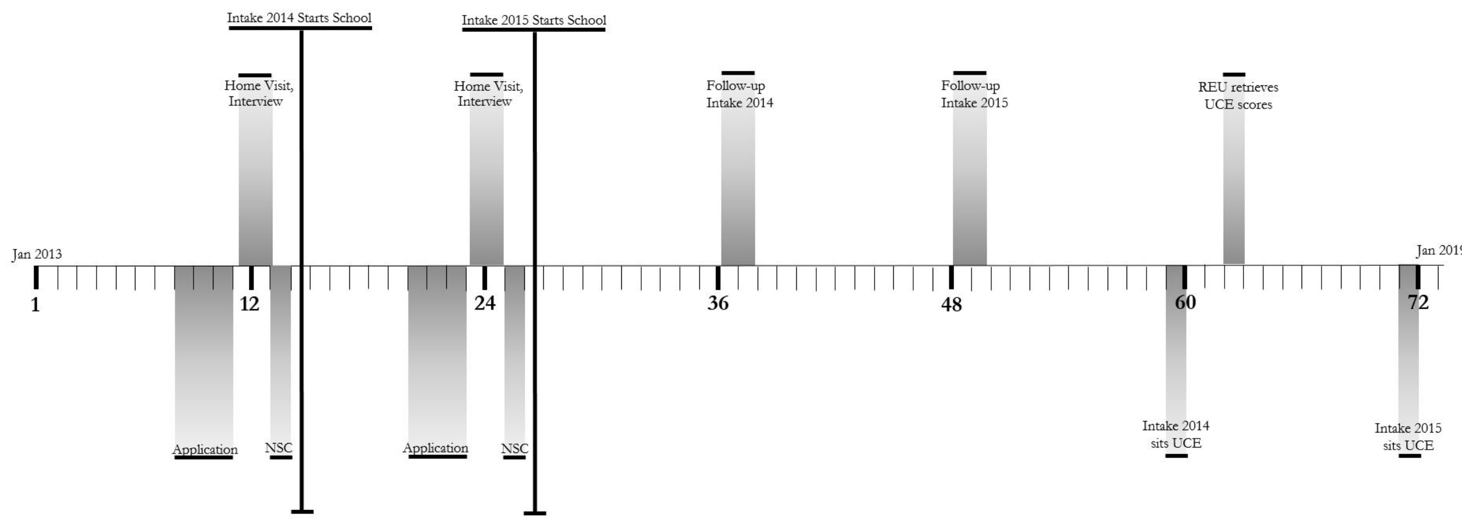
Notes: The figure displays the number of participants in each phase of the experiment, by cohort. Darker bars report the number of participants in the treatment group, lighter bars report the number of participants in the control group. Bars labelled as "Sample" report the number of participants assigned by the randomization algorithm. Bars labelled as "Eligible" report the number of participants who pass the replicable part of the second selection step. Bars labelled as "Received Scholarship" report the number of participants who actually received the scholarship. The shaded out bars at the bottom of the figure report the number of participants found at follow-up.

Figure II: AVERAGE INTENT-TO-TREAT EFFECTS AT 2-YEAR FOLLOWUP



Notes: The figure shows the difference between the treatment and control group obtained by regressing each outcome on a dummy variable for treatment status, controlling for applicant gender, cohort and district fixed effects (specification 2). Coefficients and 95% confidence intervals are reported. Outcomes are expressed in z-scores, so the unit of the coefficients is standard deviations.

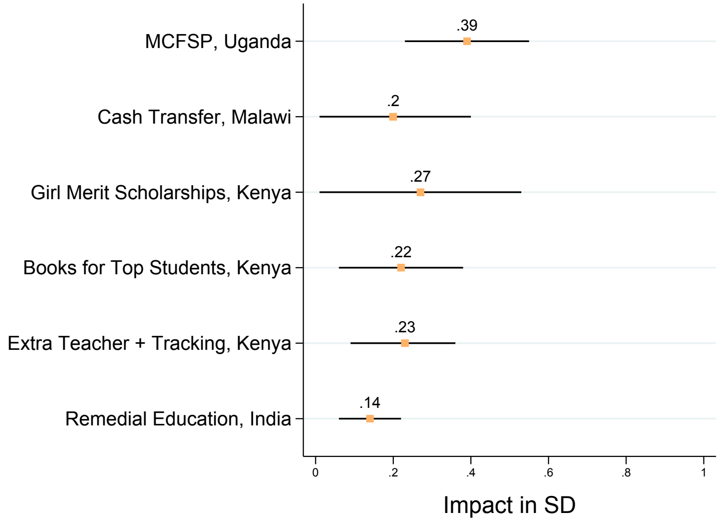
Figure III: PROJECT TIMELINE



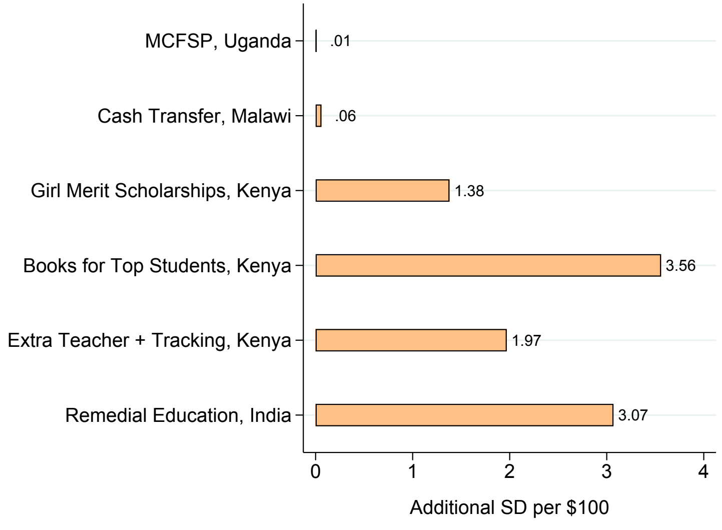
Notes: The figure shows the timeline of project activities. Shaded bars indicate the months in which an activity happened, and the activity name is specified on top of each bar.

Figure IV: Comparison With Other Education Interventions

(a) Impact on Test Scores

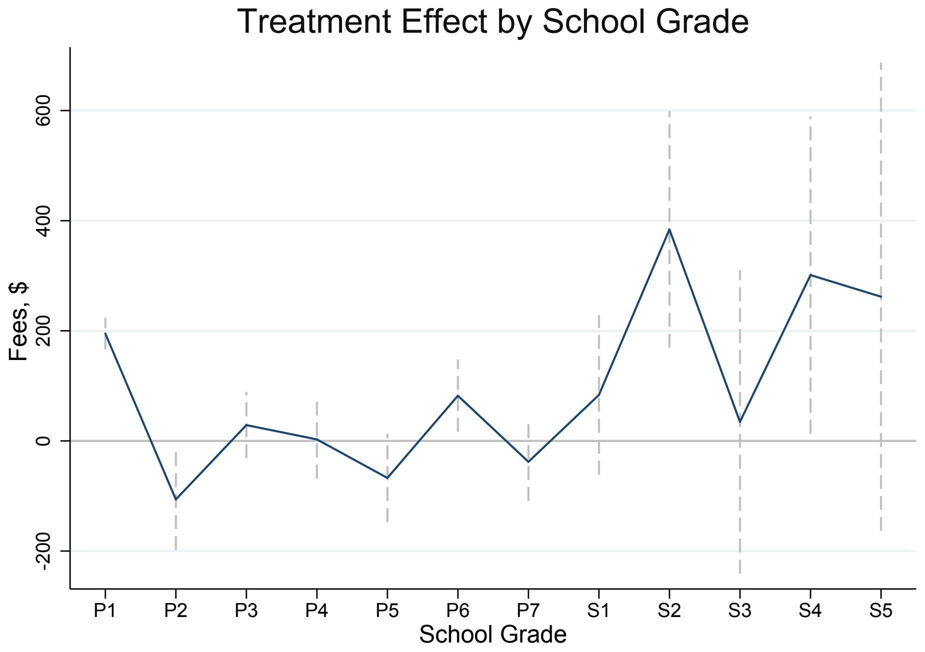


(b) Cost Effectiveness



Notes: The figure compares the impact on test scores and cost effectiveness of MCFSP to similar education programs evaluated in Kremer et al. (2013). For each program, the top panel displays the point estimate of the impact on test scores and the 90% confidence interval; the bottom panel displays how many additional standard deviations of impact each program achieves with $100, as in equation (4). MCFSP estimates are provided by the author, estimates for other programs are provided by Kremer et al. (2013).

Figure V: Effect on Siblings' Education by School Grade



Notes: The figures shows the difference between the treatment and control group obtained by regressing the *School Fees* paid for an applicant's sibling on a dummy variable for treatment status, controlling for applicant gender, cohort and district fixed effects (specification 1), and reported by school grade. Grades P1 to P7 indicate year 1 to 7 of primary school; grades S1 to S5 indicate year 1 to 5 of secondary school. Coefficients and 95% confidence intervals are reported.

Tables

Table I: DESCRIPTIVES CHARACTERISTICS AND BALANCE TESTS

	(1)	(2)	(3)	(4)
		Comparison T v. C		
Household Characteristics	C	Regression Coefficient	Standardized Difference	N
Household Size	7.240	0.011	0.018	1,325
	(2.887)	(0.157)		
Mother's Age	40.651	-1.021	-0.135	846
	(7.779)	(0.541)*		
Mother's Years of Schooling	6.336	0.119	0.052	846
	(4.324)	(0.276)		
Father's Age	47.050	-0.987	-0.200	483
	(10.916)	(0.945)		
Father's Years of Schooling	8.562	0.063	0.020	482
	(4.246)	(0.382)		
Number of Dependents	3.643	0.118	0.060	1,325
	(1.925)	(0.108)		
Children of Schooling Age	3.773	0.095	0.062	1,325
	(1.872)	(0.103)		
Labor Income (USD)	130.231	-12.466	-0.036	1,325
	(206.494)	(10.727)		
Cash Savings (USD)	73.468	14.826	0.065	1,325
	(203.246)	(12.151)		
Consumption (USD)	156.871	-1.470	0.016	1,325
	(177.600)	(9.944)		
Household Assets (USD)	12,840.887	1,453.751	0.047	1,325
	(29,276.004)	(1,619.013)		
Household Owns Business	0.305	-0.004	0.015	1,325
	(0.461)	(0.023)		
Poverty Score	28.909	-0.136	0.017	1,325
	(15.808)	(0.427)		
Home Has Thatch Roof	0.257	-0.012	-0.051	1,325
	(0.437)	(0.016)		
Applicant Works	0.111	0.009	0.009	1,322
	(0.315)	(0.017)		
BRAC Beneficiary	0.102	0.013	0.044	1,323
	(0.303)	(0.017)		

Notes: Column 1 shows the mean (and standard deviation in brackets) of each baseline characteristics in the control group. Columns 2 shows the differences between the treatment and control group obtained by regressing each characteristic on a dummy variable for treatment status, controlling for applicant gender, cohort and district fixed effects. Column 3 shows the standardized difference between the treatment and control group for each characteristic. All monetary values are in PPP USD. Detailed variable definitions are provided in Section C.

Table II: ATTRITION AT FOLLOW-UP

Sample:	All	Pooled	Cohort 2014	Cohort 2015
		Eligibles Only		
	(1)	(2)	(3)	(4)
T	-0.159	-0.120	-0.086	-0.144
	(0.013)***	(0.013)***	(0.017)***	(0.019)***
Mean Outcome (C)	0.18	0.13	0.09	0.16
Observations	2,012	1,425	596	829

Notes: The table reports ordinary least square estimates based on specification 2. The dependent variable is an indicator variable that is equal to 1 if the household was not surveyed at follow-up. T is a dummy variable equal to 1 if the household is in the treatment group. All specifications control for applicant gender, cohort and district fixed effects. Robust standard errors are given in round brackets; *** (**) (*) indicates significance for the test of the null hypothesis of no effect at the 1% (5%) (10%) level.

Table III: EFFECTS ON ASSETS

Outcome	Treatment (1)	Control Mean (2)	N (3)
Land Value	1,209.41	5,970.13	1,325
	(644.05)*	(11404.36)	
House Value	-255.41	3,602.08	1,325
	(427.88)	(8,247.48)	
Durables Value	112.15	419.09	1,325
	(48.79)**	(815.13)	
Livestock Value	34.22	392.90	1,325
	(49.53)	(1,042.06)	
Cash Savings	22.42	72.31	1,325
	(10.78)**	(189.75)	

Notes: The table reports ordinary least square estimates based on specification (1). Column 1 shows estimation results. Column 2 reports outcome mean in the control group. Column 3 reports the number of observations. All specifications control for applicant gender, cohort and district fixed effects. Robust standard errors are given in round brackets; *** (**) (*) indicates significance for the test of the null hypothesis of no effect at the 1% (5%) (10%) level.

Table IV: EFFECTS ON DURABLES

Outcome	Treatment (1)	Control Mean (2)	N (3)
Furniture Value	21.12	163.15	1,325
	(11.75)*	(214.24)	
Agricultural Tools Value	2.18	21.90	1,282
	(1.60)	(27.46)	
Bicycle Value	5.03	22.75	1,224
	(2.24)**	(40.77)	
Motorbike/Car Value	52.78	84.16	1,325
	(30.26)*	(471.52)	
Appliances Value	12.79	46.99	1,325
	(6.77)*	(113.95)	
Mobile Phone Value	5.81	45.77	1,292
	(2.85)**	(45.94)	

Notes: The table reports ordinary least square estimates based on specification (1). Column 1 shows estimation results. Column 2 reports outcome mean in the control group. Column 3 reports the number of observations. All specifications control for applicant gender, cohort and district fixed effects. Robust standard errors are given in round brackets; *** (**) (*) indicates significance for the test of the null hypothesis of no effect at the 1% (5%) (10%) level.

Table V: EFFECTS ON ANIMALS

Outcome	Treatment (1)	Control Mean (2)	N (3)
Cows Value	-3.70	281.28	1,213
	(44.46)	(961.56)	
Sheep/Goat/Pig Value	18.10	74.79	1,325
	(8.59)**	(156.99)	
Chickens Value	7.87	25.94	1,250
	(2.95)***	(46.76)	

Notes: The table reports ordinary least square estimates based on specification (1). Column 1 shows estimation results. Column 2 reports outcome mean in the control group. Column 3 reports the number of observations. All specifications control for applicant gender, cohort and district fixed effects. Robust standard errors are given in round brackets; *** (**) (*) indicates significance for the test of the null hypothesis of no effect at the 1% (5%) (10%) level.

Table VI: EFFECTS ON EXPENDITURES

Outcome	Treatment (1)	Control Mean (2)	N (3)
Food Expenditures	8.65	87.27	1,296
	(5.25)*	(99.66)	
Personal Expenditures	3.38	13.33	1,325
	(1.36)**	(23.04)	
Fuel Expenditures	0.57	6.85	1,269
	(0.72)	(12.11)	

Notes: The table reports ordinary least square estimates based on specification (1). Column 1 shows estimation results. Column 2 reports outcome mean in the control group. Column 3 reports the number of observations. All specifications control for applicant gender, cohort and district fixed effects. Robust standard errors are given in round brackets; *** (**) (*) indicates significance for the test of the null hypothesis of no effect at the 1% (5%) (10%) level.

Table VII: EFFECTS ON INCOME-GENERATING ACTIVITIES

Outcome	Treatment (1)	Control Mean (2)	N (3)
Household Owns Business	0.02	0.25	1,325
	(0.02)	(0.43)	
Number Of Businesses	0.02	0.29	1,116
	(0.03)	(0.57)	
Hours Worked In Business	-53.28	409.65	1,304
	(55.21)	(1, 101.71)	
Business Revenues	46.68	6,016.59	1,278
	(1, 183.88)	(21515.42)	

Notes: The table reports ordinary least square estimates based on specification (1). Column 1 shows estimation results. Column 2 reports outcome mean in the control group. Column 3 reports the number of observations. All specifications control for applicant gender, cohort and district fixed effects. Robust standard errors are given in round brackets; *** (**) (*) indicates significance for the test of the null hypothesis of no effect at the 1% (5%) (10%) level.

Table VIII: EFFECTS ON NUTRITION

Outcome	Treatment (1)	Control Mean (2)	N (3)
Number of Meals Score	0.09	0.00	1,272
	(0.05)*	(1.00)	
Dietary Diversity Score	0.16	-0.00	1,325
	(0.05)***	(1.00)	
HH has Fish/Meat	0.06	0.24	1,325
	(0.02)***	(0.42)	
HH has Vitamin A-rich foods	-0.01	0.67	1,325
	(0.03)	(0.47)	

Notes: The table reports ordinary least square estimates based on specification (1). Column 1 shows estimation results. Column 2 reports outcome mean in the control group. Column 3 reports the number of observations. All specifications control for applicant gender, cohort and district fixed effects. Robust standard errors are given in round brackets; *** (**) (*) indicates significance for the test of the null hypothesis of no effect at the 1% (5%) (10%) level.

Table IX: EFFECTS ON PSYCHOLOGICAL WELLBEING

Outcome	Treatment (1)	Control Mean (2)	N (3)
Depression Score$^+$	-0.25	-0.00	1,325
	(0.05)***	(1.00)	
Happiness Score$^+$	0.33	0.00	1,325
	(0.05)***	(1.00)	
Life Satisfaction Score$^+$	0.29	0.00	757
	(0.07)***	(1.00)	

Notes: The table reports ordinary least square estimates based on specification (2). Column 1 shows estimation results. Column 2 reports outcome mean in the control group. Column 3 reports the number of observations. All specifications control for applicant gender, cohort and district fixed effects. Robust standard errors are given in round brackets; *** (**) (*) indicates significance for the test of the null hypothesis of no effect at the 1% (5%) (10%) level.

Table X: EFFECTS ON SCHOOL OUTCOMES

Outcome	Treatment (1)	Control Mean (2)	N (3)
Enrolled^{+}	0.10	0.87	1,325
	(0.01)***	(0.34)	
UCE Score^{+}	0.36	-0.00	372
	(0.10)***	(1.00)	
Private School	0.02	0.61	1,063
	(0.03)	(0.49)	
Boarding School^{+}	0.25	0.70	1,144
	(0.02)***	(0.46)	

Notes: The table reports ordinary least square estimates based on specification (1) (or based on specification 2 if marked by $^{+}$). Column 1 shows estimation results. Column 2 reports outcome mean in the control group. Column 3 reports the number of observations. All specifications control for applicant gender, cohort and district fixed effects. Robust standard errors are given in round brackets; *** (**) (*) indicates significance for the test of the null hypothesis of no effect at the 1% (5%) (10%) level.

Table XI: Effects On Siblings' Education

Outcome	Treatment (1)	Control Mean (2)	N (3)
Panel A:	*All Siblings*		
Enrolled	0.01	0.95	1,695
	(0.01)	(0.21)	
School Fees	41.15	374.57	1,622
	(23.40)*	(478.42)	
Private School	-0.01	0.39	1,647
	(0.03)	(0.49)	
Panel B:	*Siblings in Primary School*		
Enrolled	0.01	0.96	1,476
	(0.01)	(0.21)	
School Fees	20.28	312.41	1,415
	(22.12)	(418.38)	
Private School	-0.03	0.38	1,436
	(0.03)	(0.49)	
Panel C:	*Siblings in Secondary School*		
Enrolled	0.01	0.92	219
	(0.03)	(0.27)	
School Fees	206.83	781.85	207
	(107.83)*	(627.22)	
Private School	0.14	0.44	211
	(0.08)*	(0.50)	

Notes: The table reports ordinary least square estimates based on specification (1). Column 1 shows estimation results. Column 2 reports outcome mean in the control group. Column 3 reports the number of observations. All specifications control for applicant gender, cohort and district fixed effects. Robust standard errors are clustered at the household level and given in round brackets; *** (**) (*) indicates significance for the test of the null hypothesis of no effect at at the 1% (5%) (10%) level.

Table XII: DESCRIPTIVES CHARACTERISTICS AND BALANCE TESTS INCLUDING NON-RESPONDERS

	(1)	(2)	(3)	(4)
		Comparison T v. C		
Household Characteristics	C	Regression Coefficient	Standardized Difference	N
Household Size	7.239	0.018	0.018	1,425
	(2.877)	(0.152)		
Mother's Age	40.355	-0.845	-0.101	896
	(7.657)	(0.523)		
Mother's Years of Schooling	6.276	0.171	0.066	896
	(4.297)	(0.271)		
Father's Age	46.739	-0.998	-0.176	524
	(10.537)	(0.896)		
Father's Years of Schooling	8.565	0.085	0.020	523
	(4.257)	(0.373)		
Number of Dependents	3.645	0.101	0.051	1,428
	(1.924)	(0.105)		
Children of Schooling Age	3.761	0.103	0.058	1,428
	(1.917)	(0.101)		
Labor Income (USD)	133.746	-15.965	-0.056	1,428
	(213.886)	(10.608)		
Cash Savings (USD)	72.548	15.300	0.067	1,428
	(199.360)	(11.576)		
Consumption (USD)	163.287	-5.246	-0.019	1,425
	(188.740)	(9.907)		
Household Assets (USD)	12,877.563	1,570.057	0.045	1,428
	(29,442.006)	(1,553.120)		
Household Owns Business	0.308	-0.002	0.005	1,428
	(0.462)	(0.023)		
Poverty Score	29.487	-0.057	-0.017	1,425
	(15.727)	(0.416)		
Home Has Thatch Roof	0.263	-0.018	-0.067	1,428
	(0.440)	(0.016)		
Applicant Works	0.110	0.005	0.013	1,422
	(0.313)	(0.016)		
BRAC Beneficiary	0.103	0.012	0.040	1,423
	(0.304)	(0.016)		

Notes: Column 1 shows the mean (and standard deviation in brackets) of each baseline characteristics in the control group. Columns 2 shows the differences between the treatment and control group obtained by regressing each characteristic on a dummy variable for treatment status, controlling for applicant gender, cohort and district fixed effects. Column 3 shows the standardized difference between the treatment and control group for each characteristic. All monetary values are in PPP USD. Detailed variable definitions are provided in Section C.

A. Imputation

Table A.I: EFFECTS ON ASSETS

Outcome	5% Imputation (1)	10% Imputation (2)	20% Imputation (3)	N (4)
Land Value	[1,344.71**,1,216.33**]	[1,408.90**,1,152.14*]	[1,537.28**,1,023.76*]	1,425
House Value	[-40.04, -130.50]	[5.19, -175.74]	[95.66, -266.20]	1,425
Durables Value	[125.83***, 116.54**]	[130.47***, 111.90**]	[139.75***, 102.62**]	1,425
Livestock Value	[66.64, 54.90]	[72.51, 49.03]	[84.25*, 37.29]	1,425
Cash Savings	[23.03**, 20.89**]	[24.10**, 19.82*]	[26.24**, 17.68*]	1,425

Notes: The table reports ordinary least square estimates based on specification (1). *x% Imputation* provides estimates from imputing to the lower (upper) bound the mean minus (plus) a specified standard deviation multiple of the observed treatment group distribution to the nonresponders in the treatment group, and the mean plus (minus) the same standard deviation multiple of the observed control group distribution to the nonresponders in the control group. *** (**) (*) indicates significance for the test of the null hypothesis of no effect at 1% (5%) (10%) level. All monetary values are in PPP USD.

Table A.II: EFFECTS ON DURABLES - BOUNDS

Outcome	5% Imputation (1)	10% Imputation (2)	20% Imputation (3)	N (4)
Furniture Value	[25.12**, 22.68**]	[26.34**, 21.47*]	[28.78**, 19.03*]	1,425
Agricultural Tools Value	[2.15, 1.79]	[2.34, 1.61]	[2.70*, 1.24]	1,385
Bicycle Value	[6.09***, 5.52***]	[6.38***, 5.23**]	[6.95***, 4.66**]	1,326
Motorbike/Car Value	[58.29**, 52.86*]	[61.01**, 50.15*]	[66.44**, 44.72]	1,425
Appliances Value	[15.80**, 14.62**]	[16.39**, 14.03**]	[17.57***, 12.85*]	1,425
Mobile Phone Value	[6.49**, 5.94**]	[6.76**, 5.67**]	[7.30***, 5.13*]	1,391

Notes: The table reports ordinary least square estimates based on specification (1). *x% Imputation* provides estimates from imputing to the lower (upper) bound the mean minus (plus) a specified standard deviation multiple of the observed treatment group distribution to the nonresponders in the treatment group, and the mean plus (minus) the same standard deviation multiple of the observed control group distribution to the nonresponders in the control group. *** (**) (*) indicates significance for the test of the null hypothesis of no effect at 1% (5%) (10%) level. All monetary values are in PPP USD.

Table A.III: EFFECTS ON ANIMALS - BOUNDS

Outcome	5% Imputation (1)	10% Imputation (2)	20% Imputation (3)	N (4)
Cows Value	[23.51, 11.53]	[29.50, 5.53]	[41.49, -6.45]	1,310
Sheep/Goat/Pig Value	[22.35***, 20.50**]	[23.27***, 19.58**]	[25.11***, 17.73**]	1,425
Chickens Value	[8.83***, 8.22***]	[9.13***, 7.92***]	[9.73***, 7.31**]	1,349

Notes: The table reports ordinary least square estimates based on specification (1). *x% Imputation* provides estimates from imputing to the lower (upper) bound the mean minus (plus) a specified standard deviation multiple of the observed treatment group distribution to the nonresponders in the treatment group, and the mean plus (minus) the same standard deviation multiple of the observed control group distribution to the nonresponders in the control group. *** (**) (*) indicates significance for the test of the null hypothesis of no effect at 1% (5%) (10%) level. All monetary values are in PPP USD.

Table A.IV: EFFECTS ON EXPENDITURES - BOUNDS

Outcome	5% Imputation (1)	10% Imputation (2)	20% Imputation (3)	N (4)
Food Expenditures	[10.78**, 9.52*]	[11.41**, 8.89*]	[12.67**, 7.63]	1,399
Personal Expenditures	[3.61***, 3.32***]	[3.75***, 3.18**]	[4.03***, 2.90**]	1,425
Fuel Expenditures	[0.86, 0.70]	[0.94, 0.62]	[1.10, 0.46]	1,370

Notes: The table reports ordinary least square estimates based on specification (1). *x% Imputation* provides estimates from imputing to the lower (upper) bound the mean minus (plus) a specified standard deviation multiple of the observed treatment group distribution to the nonresponders in the treatment group, and the mean plus (minus) the same standard deviation multiple of the observed control group distribution to the nonresponders in the control group. *** (**) (*) indicates significance for the test of the null hypothesis of no effect at 1% (5%) (10%) level. All monetary values are in PPP USD.

Table A.V: EFFECTS ON INCOME-GENERATING ACTIVITIES - BOUNDS

Outcome	5% Imputation (1)	10% Imputation (2)	20% Imputation (3)	N (4)
Household Owns Business	[0.03, 0.02]	[0.03, 0.02]	[0.03, 0.02]	1,425
Number Of Businesses	[0.02, 0.01]	[0.02, 0.01]	[0.03, 0.00]	1,205
Hours Worked In Business	[-42.48, -56.21]	[-35.62, -63.08]	[-21.89, -76.81]	1,404
Business Revenues	[255.77, -16.84]	[392.07, -153.15]	[664.68, -425.75]	1,389

Notes: The table reports ordinary least square estimates based on specification (1). *x% Imputation* provides estimates from imputing to the lower (upper) bound the mean minus (plus) a specified standard deviation multiple of the observed treatment group distribution to the nonresponders in the treatment group, and the mean plus (minus) the same standard deviation multiple of the observed control group distribution to the nonresponders in the control group. *** (**) (*) indicates significance for the test of the null hypothesis of no effect at 1% (5%) (10%) level. All monetary values are in PPP USD.

Table A.VI: EFFECTS ON NUTRITION - BOUNDS

Outcome	5% Imputation (1)	10% Imputation (2)	20% Imputation (3)	N (4)
Number of Meals Score	[0.09*, 0.08]	[0.10**, 0.07]	[0.11**, 0.06]	1,371
Dietary Diversity Score	[0.17***, 0.16***]	[0.17***, 0.15***]	[0.19***, 0.14***]	1,425
HH has Fish/Meat	[0.07***, 0.07***]	[0.07***, 0.06***]	[0.08***, 0.06***]	1,425
HH has Vitamin A-rich foods	[0.01, 0.00]	[0.01, -0.00]	[0.02, -0.01]	1,425

Notes: The table reports ordinary least square estimates based on specification (1). *x% Imputation* provides estimates from imputing to the lower (upper) bound the mean minus (plus) a specified standard deviation multiple of the observed treatment group distribution to the nonresponders in the treatment group, and the mean plus (minus) the same standard deviation multiple of the observed control group distribution to the nonresponders in the control group. *** (**) (*) indicates significance for the test of the null hypothesis of no effect at 1% (5%) (10%) level. All monetary values are in PPP USD.

Table A.VII: EFFECTS ON PSYCHOLOGICAL WELLBEING - BOUNDS

Outcome	5% Imputation (1)	10% Imputation (2)	20% Imputation (3)	N (4)
Depression Score^{+}	[-0.25***, -0.26***]	[-0.25***, -0.27***]	[-0.23***, -0.28***]	1,425
Happiness Score^{+}	[0.34***, 0.33***]	[0.34***, 0.32***]	[0.36***, 0.31***]	1,425
Life Satisfaction Score^{+}	[0.28***, 0.27***]	[0.29***, 0.26***]	[0.31***, 0.25***]	829

Notes: The table reports ordinary least square estimates based on specification (2). *x% Imputation* provides estimates from imputing to the lower (upper) bound the mean minus (plus) a specified standard deviation multiple of the observed treatment group distribution to the nonresponders in the treatment group, and the mean plus (minus) the same standard deviation multiple of the observed control group distribution to the nonresponders in the control group. *** (**) (*) indicates significance for the test of the null hypothesis of no effect at 1% (5%) (10%) level. All monetary values are in PPP USD.

Table A.VIII: EFFECTS ON SCHOOL OUTCOMES - BOUNDS

Outcome	5% Imputation (1)	10% Imputation (2)	20% Imputation (3)	N (4)
Enrolled^{+}	[0.10***, 0.10***]	[0.10***, 0.09***]	[0.11***, 0.09***]	1,425
UCE Score^{+}	[0.39***, 0.32***]	[0.43***, 0.28***]	[0.50***, 0.21***]	596
Private School	[0.04*, 0.03]	[0.05**, 0.02]	[0.07***, -0.00]	1,309
Boarding School^{+}	[0.25***, 0.24***]	[0.26***, 0.23***]	[0.27***, 0.22***]	1,425

Notes: The table reports ordinary least square estimates based on specification (1) (or based on specification (2) if marked by $^{+}$). *x% Imputation* provides estimates from imputing to the lower (upper) bound the mean minus (plus) a specified standard deviation multiple of the observed treatment group distribution to the nonresponders in the treatment group, and the mean plus (minus) the same standard deviation multiple of the observed control group distribution to the nonresponders in the control group. *** (**) (*) indicates significance for the test of the null hypothesis of no effect at 1% (5%) (10%) level. All monetary values are in PPP USD.

B. Instrumental Variable Estimates

Table B.IX: FIRST STAGE

	Won Scholarship
T	0.827***
	(0.0145)
Observations	1325
F-stat	47.18
Adjusted R^2	0.72

Notes: The table reports ordinary least square estimates based on specification (3). All specifications control for applicant gender, cohort and district fixed effects. Standard errors are given in round brackets; *** (**) (*) indicates significance of that test at the 1% (5%) (10%) level.

Table B.X: EFFECTS ON ASSETS - IV ESTIMATES

Outcome	Treatment (1)	Control Mean (2)	N (3)
Land Value	1,462.78	5,970.13	1,325
	(749.83)*	(11404.36)	
House Value	-309.39	3,602.08	1,325
	(513.04)	(8,247.48)	
Durables Value	135.61	419.09	1,325
	(56.72)**	(815.13)	
Livestock Value	41.38	392.90	1,325
	(59.58)	(1,042.06)	
Cash Savings	27.11	72.31	1,325
	(12.86)**	(189.75)	

Notes: The table reports instrumental variable estimates based on specification (1). *Won Scholarship* is instrumented with the randomized treatment assignment *T*. *Won Scholarship* is a dummy equal to 1 if the applicant won the scholarship. *T* is a dummy equal to 1 if the applicant belongs to the treatment group. Column 1 shows estimation results. Column 2 reports outcome mean in the control group. Column 3 reports the number of observations. All specifications control for applicant gender, cohort and district fixed effects. Standard errors are given in round brackets; *** (**) (*) indicates significance for the test of the null hypothesis of no effect at the 1% (5%) (10%) level.

Table B.XI: EFFECTS ON DURABLES - IV ESTIMATES

Outcome	Treatment (1)	Control Mean (2)	N (3)
Furniture Value	25.56	163.15	1,325
	(13.61)*	(214.24)	
Agricultural Tools Value	2.64	21.90	1,282
	(1.90)	(27.46)	
Bicycle Value	6.15	22.75	1,224
	(2.70)**	(40.77)	
Motorbike/Car Value	63.85	84.16	1,325
	(35.30)*	(471.52)	
Appliances Value	15.48	46.99	1,325
	(7.91)*	(113.95)	
Mobile Phone Value	7.05	45.77	1,292
	(3.28)**	(45.94)	

Notes: The table reports instrumental variable estimates based on specification (1). *Won Scholarship* is instrumented with the randomized treatment assignment T. *Won Scholarship* is a dummy equal to 1 if the applicant won the scholarship. T is a dummy equal to 1 if the applicant belongs to the treatment group. Column 1 shows estimation results. Column 2 reports outcome mean in the control group. Column 3 reports the number of observations. All specifications control for applicant gender, cohort and district fixed effects. Standard errors are given in round brackets; *** (**) (*) indicates significance for the test of the null hypothesis of no effect at the 1% (5%) (10%) level.

Table B.XII: EFFECTS ON ANIMALS - IV ESTIMATES

Outcome	Treatment (1)	Control Mean (2)	N (3)
Cows Value	-4.50	281.28	1,213
	(54.45)	(961.56)	
Sheep/Goat/Pig Value	21.89	74.79	1,325
	(10.29)**	(156.99)	
Chickens Value	9.54	25.94	1,250
	(3.48)***	(46.76)	

Notes: The table reports instrumental variable estimates based on specification (1). *Won Scholarship* is instrumented with the randomized treatment assignment *T*. *Won Scholarship* is a dummy equal to 1 if the applicant won the scholarship. *T* is a dummy equal to 1 if the applicant belongs to the treatment group. Column 1 shows estimation results. Column 2 reports outcome mean in the control group. Column 3 reports the number of observations. All specifications control for applicant gender, cohort and district fixed effects. Standard errors are given in round brackets; *** (**) (*) indicates significance for the test of the null hypothesis of no effect at the 1% (5%) (10%) level.

Table B.XIII: EFFECTS ON EXPENDITURES - IV ESTIMATES

Outcome	Treatment (1)	Control Mean (2)	N (3)
Food Expenditures	10.46	87.27	1,296
	(6.23)*	(99.66)	
Personal Expenditures	4.08	13.33	1,325
	(1.58)***	(23.04)	
Fuel Expenditures	0.69	6.85	1,269
	(0.82)	(12.11)	

Notes: The table reports instrumental variable estimates based on specification (1). *Won Scholarship* is instrumented with the randomized treatment assignment *T*. *Won Scholarship* is a dummy equal to 1 if the applicant won the scholarship. *T* is a dummy equal to 1 if the applicant belongs to the treatment group. Column 1 shows estimation results. Column 2 reports outcome mean in the control group. Column 3 reports the number of observations. All specifications control for applicant gender, cohort and district fixed effects. Standard errors are given in round brackets; *** (**) (*) indicates significance for the test of the null hypothesis of no effect at the 1% (5%) (10%) level.

Table B.XIV: EFFECTS ON INCOME-GENERATING ACTIVITIES - IV ESTIMATES

Outcome	Treatment (1)	Control Mean (2)	N (3)
Household Owns Business	0.02	0.25	1,325
	(0.03)	(0.43)	
Number Of Businesses	0.02	0.29	1,116
	(0.04)	(0.57)	
Hours Worked In Business	-64.33	409.65	1,304
	(63.51)	(1, 101.71)	
Business Revenues	56.38	6,016.59	1,278
	(1, 358.60)	(21515.42)	

Notes: The table reports instrumental variable estimates based on specification (1). *Won Scholarship* is instrumented with the randomized treatment assignment *T*. *Won Scholarship* is a dummy equal to 1 if the applicant won the scholarship. *T* is a dummy equal to 1 if the applicant belongs to the treatment group. Column 1 shows estimation results. Column 2 reports outcome mean in the control group. Column 3 reports the number of observations. All specifications control for applicant gender, cohort and district fixed effects. Standard errors are given in round brackets; *** (**) (*) indicates significance for the test of the null hypothesis of no effect at the 1% (5%) (10%) level.

Table B.XV: EFFECTS ON NUTRITION - IV ESTIMATES

Outcome	Treatment (1)	Control Mean (2)	N (3)
Number of Meals Score	0.11	0.00	1,272
	(0.06)*	(1.00)	
Dietary Diversity Score	0.19	-0.00	1,325
	(0.05)***	(1.00)	
HH has Fish/Meat	0.07	0.24	1,325
	(0.03)***	(0.42)	
HH has Vitamin A-rich foods	-0.01	0.67	1,325
	(0.03)	(0.47)	

Notes: The table reports instrumental variable estimates based on specification (1). *Won Scholarship* is instrumented with the randomized treatment assignment *T*. *Won Scholarship* is a dummy equal to 1 if the applicant won the scholarship. *T* is a dummy equal to 1 if the applicant belongs to the treatment group. Column 1 shows estimation results. Column 2 reports outcome mean in the control group. Column 3 reports the number of observations. All specifications control for applicant gender, cohort and district fixed effects. Standard errors are given in round brackets; *** (**) (*) indicates significance for the test of the null hypothesis of no effect at the 1% (5%) (10%) level.

Table B.XVI: EFFECTS ON PSYCHOLOGICAL WELLBEING - IV ESTIMATES

Outcome	Treatment (1)	Control Mean (2)	N (3)
Depression Score+	-0.30	-0.00	1,325
	(0.06)***	(1.00)	
Happiness Score+	0.40	0.00	1,325
	(0.06)***	(1.00)	
Life Satisfaction Score+	0.35	0.00	757
	(0.08)***	(1.00)	

Notes: The table reports instrumental variable estimates based on specification (2); *Won Scholarship* is instrumented with the randomized treatment assignment *T*. *Won Scholarship* is a dummy equal to 1 if the applicant won the scholarship. *T* is a dummy equal to 1 if the applicant belongs to the treatment group. Column 1 shows estimation results. Column 2 reports outcome mean in the control group. Column 3 reports the number of observations. All specifications control for applicant gender, cohort and district fixed effects. Standard errors are given in round brackets; *** (**) (*) indicates significance for the test of the null hypothesis of no effect at the 1% (5%) (10%) level.

Table B.XVII: EFFECTS ON SCHOOL OUTCOMES - IV ESTIMATES

Outcome	Treatment (1)	Control Mean (2)	N (3)
Enrolled+	0.12	0.87	1,325
	(0.02)***	(0.34)	
UCE Score+	0.39	-0.00	372
	(0.10)***	(1.00)	
Private School	0.03	0.61	1,063
	(0.03)	(0.49)	
Boarding School+	0.30	0.70	1,144
	(0.02)***	(0.46)	

Notes: The table reports instrumental variable estimates based on specification (1) (or based on specification (2) if marked by +). *Won Scholarship* is instrumented with the randomized treatment assignment *T*. *Won Scholarship* is a dummy equal to 1 if the applicant won the scholarship. *T* is a dummy equal to 1 if the applicant belongs to the treatment group. Column 1 shows estimation results. Column 2 reports outcome mean in the control group. Column 3 reports the number of observations. All specifications control for applicant gender, cohort and district fixed effects. Standard errors are given in round brackets; *** (**) (*) indicates significance for the test of the null hypothesis of no effect at the 1% (5%) (10%) level.

C. Variable List

Variables, In Order of Appearance	
Household size	The number of people living in the respondent's household.
Mother's Age	The age of the applicant's mother.
Mother's Years of Schooling	The number of completed years of education of the applicant's mother.
Father's Age	The age of the applicant's father.
Father's Years of Schooling	The number of completed years of education of the applicant's father.
Number of Dependents	The number of people living in the respondent's household who are younger than 15 years of age and older than 64 years of age.
Number of Children of Schooling Age	The number of children living in the respondent's household who are aged between 6 and 18.
Labor Income	Average monthly labor income (in PPP USD) of the household during the month preceding the survey.
Cash Savings	The value (in PPP USD) of cash savings that the household had at the time of the survey.
Consumption	The monthly consumption expenditure (in PPP USD) of the household. It is the sum of the respondents monthly personal consumption on non-food items and services with her households per-capita food, health, water, fuel, education consumption. Household per capita monthly food consumption is imputed from previous 7 days recall. The respondents non-food personal expenditure includes the following items: clothes, shoes, phone airtime, transportation, jewelry/ornaments, hairdressing, soda, sweets, cosmetics/toiletries, alcohol.
Household Assets	The value (in PPP USD) of durable assets owned by the respondents household at the time of the survey.
Household Owns Business	A dummy variable taking the value of one if any of the household members owns a non-agricultural business.
Poverty Score	A country-specific indicator developed by Mark Schreiner to measure the likelihood that a household lives below a certain poverty line (see `www.progressoutofpoverty.org`).
Home Has Thatch Roof	A dummy variable taking the value of one if the household's main dwelling has a thatch roof.
Applicant Works	A dummy variable taking the value of one if the applicant works for income.

Brac Beneficiary	A dummy variable taking the value of one if any household member is participating in a Brac program at the time of the survey.
T	A dummy variable taking the value of one if the applicant has been assigned to the treatment group.
Land Value	The value (in PPP USD) of the residential and cultivable land owned by the respondents household at the time of the survey.
House Value	The value (in PPP USD) of the houses owned by the respondents household at the time of the survey.
Livestock Value	The value (in PPP USD) of the animals owned by the respondents household at the time of the survey. Animals include cows/bulls, goats, sheep, pigs and poultry.
Furniture Value	The value (in PPP USD) of the furniture owned by the respondents household at the time of the survey.
Agricultural Tools Value	The value (in PPP USD) of the agricultural tools owned by the respondents household at the time of the survey.
Bicycle Value	The value (in PPP USD) of the bicycles owned by the respondents household at the time of the survey.
Motorbike/Car Value	The value (in PPP USD) of the cars and motorbikes owned by the respondents household at the time of the survey.
Appliances Value	The value (in PPP USD) of the appliances owned by the respondents household at the time of the survey. Appliances include radios, televisions, DVD players, computers and refrigerators.
Mobile Phone Value	The value (in PPP USD) of the mobile phones owned by the respondents household at the time of the survey. Appliances include radios, televisions, DVD players, computers and refrigerators.
Food Expenditures	The monthly food consumption expenditure (in PPP USD) of the household. Household per capita monthly food consumption is imputed from previous 7 days recall.
Personal Expenditures	The monthly consumption expenditure (in PPP USD) of the respondent in the following items: clothes, shoes, phone airtime, transportation, jewelry/ornaments, hairdressing, soda, sweets, cosmetics/toiletries, alcohol.
Fuel Expenditures	The monthly fuel consumption expenditure (in PPP USD) of the household. It includes money spent on electricity, charcoal, paraffin, kerosene, gasoline and firewood.
Number of Businesses	The number of businesses owned by household members.

Hours Worked in Business	The number of hours worked by the respondent in any of the household businesses.
Business Revenues	The monetary value of the yearly revenues of household businesses in PPP USD terms.
Number of Meals Score	The standardized value of the number of meals eaten by the household in the past 2 days.
Dietary Diversity Score	The standardized value of the dietary diversity indicator developed by the Fanta project at USAID (fantaproject.org) to measure food access. It consists in the number of unique food categories consumed by household members over a given period out of 12 possible categories. The categories are: grains and cereals, roots and tubers, vegetables, fruits, meat, eggs, fish and seafood, pulses/legumes and nuts, milk and milk products, oils and fats, sugar and honey, condiments and coffee/tea. The original score ranges from 0 to 12.
Household has Fish/Meat	A dummy variable taking the value of one if the household had any fish or meat in the past 2 days.
Household has Vitamin A-rich foods	A dummy variable taking the value of one if the household had pumpkin, carrots, squash or orange-fleshed sweet potatoes.
Depression Score	The standardized value of the respondent's Center for Epidemiologic Studies Depression Scale (CES-D) score, a 20-item measure to identify individuals at risk for clinical depression. Official website: https://cesd-r.com.
Happiness Score	The standardized value of the respondent's own happiness assessment, which takes value 1 (not at all happy), 2 (not very happy), 3 (quite happy), 4 (very happy) .
Life Satisfaction Score	The standardized value of the respondent's own life satisfaction assessment, which takes value 1 (very dissatisfied) to 10 (very satisfied) . This variable is only available for the second student cohort.
Enrolled	A dummy variable taking the value of one if the participant (applicant or sibling, depending on the table) is enrolled in school at the time of the survey.
UCE Score	The standardized value of the applicant's O-level exam score. The official name of the exam is Uganda Certificate of Education (UCE). Only students who have completed the first four years of secondary education can sit the exam. UCE scores are only available for students in the second cohort (those who entered the program in 2015).

Private School	A dummy variable taking the value of one if the participant (applicant or sibling, depending on the table) is enrolled in a private school at the time of the survey.
Boarding School	A dummy variable taking the value of one if the applicant is enrolled in a boarding school at the time of the survey.
Won Scholarship	A dummy variable taking the value of one if the applicant was selected to receive the MasterCard Foundation Scholars Program.
School Fees	The amount (in PPP USD) of total school fees paid for the student during the previous school year.

Table C.XVIII: LIST OF VARIABLES

D. Application Form

THE MASTERCARD FOUNDATION
Scholars Program

The MasterCard Foundation | brac

Application Form UCE (S.1) -Second Cohort 2014.

BRAC Uganda in partnership with MasterCard Foundation is implementing a scholars program that was launched in 2013 to provide talented yet economically marginalised Ugandans, next-generation leaders who are committed to giving back, with access to quality education, and deliver holistic financial, emotional and academic support throughout their secondary education.

To be filled by applicant (Incomplete applications will not be considered, please write clearly)

Application category: (tick one) ☐

A. Personal Information (As per PLE registration):

A1. Surname of the Applicant……………………………………………………………….……

A2. Other Name(s) ……………………………………………………………………..……..

A3.Date of Birth (dd/mm/yyyy): ___ ___/___ ___/___ ___ ___ ___ A4.Sex: Male☐ Female☐

A5. Current Contact Address:

Village……………………………….......... Parish……………………………….…….

Sub-County……………………………… District……………………..………………

A6. Home District……………………………………………………….…………………...

A7. Contact numbers (please fill this in as we will contact you through this number if you are shortlisted)											Name of contact person/owner of this number	Relationship with contact person (Write 'own' if your number)
Number 1												
Number 2												

A8. Who currently supports your education costs? (Tick as many is applicable for you)

Myself☐ Father☐ Mother☐ Foster Parents☐ Scholarship☐

Others, specify ....

B. Information of parents:

B1. Father's name:	B5.Mother'sname: ....
B2. Is he alive? Yes☐ No☐	B6. Is she alive? Yes☐ No☐
B3. Father's education:	B7. Mother's education: ....
B4. Father's occupation:	B8. Mother's occupation: ....

B9. Are you the head of your household? Yes☐ No☐

B10. Who do you live with? Both Parents☐ Mother only☐ Father only☐

Foster parents/guardian☐

B11. (If living with foster parents/guardian) Name: ....

B12. (If living with foster parents/guardian) Occupation: ....

B13. Is any member of your family a BRAC member? Yes☐ No☐

B14. If BRAC member, which programme? ....

B15. Have you benefited from any sponsorship before? Yes☐ No☐

B16. (If yes) Name the Sponsor…………………………………………………………………

B17. Are you still receiving support from this sponsor? Yes☐ No☐

B18. (if no) Why not? ....

C. Status of applicant:

C1. Do you have any form of disability? Yes ☐ No ☐

C2. If yes, what form of disability? ....

C3. Have you been formerly abducted? Yes ☐ No ☐

C4. How many brothers and sisters do you have? .... C5. How many of them are studying? ....

C6. What is your birth order among your brothers and sisters?

1st born/eldest ☐ 2nd ☐ 3rd ☐ 4th ☐ 5th ☐ 6th or younger ☐

C7. Who is the main income earner in your household?

Father ☐ Mother ☐ Brother ☐ Sister ☐ Other, specify……………

D. Academic Information (This section is mandatory, please fill it for your application to be considered. Remember to write everything clearly)

Examination	Year of taking this exam	School/Institution and address	Aggregate obtained
D1. Mock test			
D2. 1st term			
D3. PLE			

D4. Index Number for **PLE** ☐☐☐ ☐☐☐ ☐☐☐ Year: ☐☐☐☐

D6. Name and district of the schools that you have selected for UCE/UACE

1…………………………………………………………………………… District: ....

2…………………………………………………………………………… District: ....

3…………………………………………………………………………… District: ....

4…………………………………………………………………………… District: ....

D7. How did you hear about this scholarship programme? Radio ☐ Newspaper ☐

Poster ☐ School ☐ BRAC staff ☐ Friend/word of mouth ☐ Other, (specify)…………………

I declare that all the information provided here is true and accurate to the best of my knowledge, and I have read and understood the notes to applicants below.

Applicant:

Signature and date: ……………………………..

Name: …………………………………...........…

Endorsed by parent/guardian/Head Teacher

Signature and date: ……………………………..

Name: ....

To be filled out by the Head Teacher of your current school/institute

Please provide your assessment (to the best of your knowledge) about the applicant on:

Academic ability:

Excellent (top 10%) ☐ Very good (top 20%) ☐ Good (top 35%) ☐ Fair (top 50%) ☐

Financial ability:

Rich ☐ Middle-class ☐ Needy ☐ Very needy ☐

Name and Signature of Head Teacher (with date and stamp): ....
....

Note to applicants: *This application form is free of charge. No fee should be charged at any stage. After completing the form, submit at the school from where you collected this form OR at the nearest BRAC branch office. Only short listed candidates will be contacted to appear for interview at district head office. Inaccurate information, multiple applications and any effort to influence the process will lead to disqualification. Decisions of the selection committees are final. Finally selected scholars' names will appear at national newspaper. Ensure that contact numbers filled are accurate and reliable. This scholarship is for Ugandan nationals only.*

Moral Hazard:

Experimental Evidence from Tenancy Contracts

with Konrad Burchardi, Selim Gulesci and Munshi Sulaiman

"For, when the cultivator has to give to his landlord half of the returns to each dose of capital and labor that he applies to the land, it will not be to his interest to apply any doses the total return to which is less than twice enough to reward him." (Marshall, 1890, Book VI, Chapter X.14)

I. Introduction

Agriculture is the main source of income for a majority of the rural poor in developing countries; yet agricultural productivity remains notoriously low (Gollin, Lagakos, and Waughn, 2014). A commonly cited explanation for low agricultural output in developing countries is the prevalence of output sharing rules that make farmers less-than-full residual claimants.[1] Such output sharing rules may take the form of sharecropping contracts, whereby a tenant farmer pays a share of her output to the landowner (Banerjee, Gertler, and Ghatak, 2002), formal taxes or infor-

[1] According to a household panel survey by Uganda Bureau of Statistics, 38% of the rural households producing crops were engaged in sharecropping arrangement in 2009-10 (Khandker and Koolwal, 2014). According to a nationally representative survey of rural areas in Bangladesh in 2007, 26% of the cultivable land were under sharecropping compared to 9% with rental arrangement (Hossain and Bayes, 2015).

mal taxes such as kinship taxation (Lewis, 1955; Jakiela and Ozier, 2016), or imperfectly defined and secured property rights (Besley, 1995; Shleifer and Vishny, 1998; Acemoglu, Johnson, and Robinson, 2001). It is a central idea of modern microeconomics that such output sharing rules induce inefficient behavior by the agent as long as she is not the full residual claimant. This powerful idea dates back to the classical authors Adam Smith and, in particular, Alfred Marshall, who stated it succinctly, precisely to highlight sharecropping contracts as a potential source of low agricultural output.

How important is the degree of output sharing in explaining low agricultural output? How would tenant farmers adjust their behavior in response to a higher share? How much of that effect is due to the incentive effect conjectured by Alfred Marshall? These questions are empirical in nature. Over the past couple of decades research has attempted to answer these questions using observational data (Rao, 1971; Bell, 1977; Shaban, 1987). This paper reports novel results from the first field experiment designed to estimate and understand the effects of sharecropping contracts on tenant farmers' input choices, risk-taking behavior and output. These estimates provide answers to the three questions set out above.

Quantifying the incentive effects of contracts on production decisions generally poses at least two challenges. First, the outcomes of interest as well as the contractual terms are likely to be determined jointly by unobservable factors. In tenancy contracts, technology adoption and investment choices are likely to be a function of factors such as unobserved productivity, farmer ability or outside options, and contractual terms are chosen endogenously as a function of the same factors. In fact, an extensive theoretical literature discusses the potential determinants of agricultural tenancy contracts.[2] This body of work implies that a positive correlation between the tenant's share in output and the level of total output might be the con-

[2]Sharecropping contracts can be understood as trading off incentive and risk-sharing motives (Stiglitz, 1974), as incentivizing the landlord's inputs, some of which may be unobservable and therefore non-contractible (Eswaran and Kotwal, 1985), as trading off moral hazard in effort and risk-taking (Ghatak and Pandey, 2000), as screening tenants of different abilities (Hallagan, 1978; Newbery and Stiglitz, 1979) and as the optimal contract under financial constraints (Shetty, 1988; Laffont and Matoussi, 1995; Banerjee et al., 2002). See Binswanger and Rosenzweig (1982) and Otsuka and Hayami (1988) for reviews of the literature on contract choice and the co-existence of different types of tenancy contracts.

sequence of unobservable factors driving both the adoption of certain contractual terms and agricultural output, rather than evidence of incentive effects. Secondly, even when plausibly exogenous variation in a tenant's share of the output exists, it cannot solely be interpreted as an incentive effect: a higher output share has an incentive effect, but additionally induces higher income and higher exposure to risk, both of which might influence farmers' choices independently.

To overcome these challenges, we collaborated with the NGO BRAC in Uganda to implement a randomized controlled trial that induces variation in real-life tenancy contracts. As part of their operations, BRAC leased plots of land to women from low socio-economic backgrounds who were interested in becoming farmers (henceforth 'tenants') and provided them with agricultural training and a package of seeds for cultivation – effectively acting as the 'landlord'. The experiment was conducted with 304 tenants located in 237 villages, and at most two tenants per village. In all villages, tenants were contracted for one season under a sharecropping contract that gave them a 50% stake in the output. After signing the contract, villages were randomized into three groups.[3] In the first group (C), the contract was maintained – i.e. tenants received 50% of output. In the second group (T1), tenants were offered to keep 75% of the output. Tenants in a third group (T2) kept the same output share as in control (50%) but received an additional fixed payment which was independent of their output level, paid at harvest and announced at the same time as T1 received news of the higher share.[4] Within this third group, half of the tenants (T2A) received it as a risk-free cash transfer while the other half received part of their additional payment as a lottery (T2B), the expected payment in T2A and T2B being the same. The plots were visited pre-harvest to measure output levels and crop choice; and all tenants were surveyed shortly after the harvest to record their input use, such as labor, fertilizer, and tools.

The experimental design entails six key elements that allow us to estimate and understand the effects of output sharing rules on farmers' decisions. First, by ran-

[3] The village level randomization guarantees that if there were two tenants in a village, both were exposed to the same treatment condition.

[4] Note that the treatment is designed to emulate the nature of the income effect of treatment T1; it should not be thought of as realized unconditional cash transfer (Haushofer and Shapiro, 2016).

domly assigning tenants to contracts with varying terms, we ensure that tenants in different groups are not systematically different in their (unobservable) characteristics, such as their abilities, time preferences or risk attitudes. Second, the same contract was advertised in all groups to rule out ex-ante selection effects.[5] Further, tenants in the treatment groups were offered a change in contract that was unambiguously beneficial to avoid design-induced attrition. Third, we changed the terms of the tenancy agreements in T1 to generate exogenous variation in the tenant's share of output. This variation is key for estimating the incentive effect of the sharecropping contract. However, tenants entitled to 75% of their output are not only exposed to stronger incentives relative to those who receive 50%; they also have higher expected income, and they are exposed to additional risk. Fourth, the additional income may influence tenants' input choice and risk-taking through various mechanisms, rendering the direction and the magnitude of the effect unclear.[6] For that reason we implemented T2. The comparison of T2 with C allows us to test for the presence of an income effect on agricultural productivity. Fifth, to test whether tenants' exposure to risk alters their agricultural choices, some tenants within T2 received a risky income transfer while others received a safe one. The comparison of T2B with T2A allows us to test for the presence of a risk exposure effect. Sixth, tenants might have an incentive to misreport the agricultural output when a share of the output has to be given to the landlord. We therefore conducted pre-harvest plot-surveys to obtain an objective measure of output.

We present a model that specifies how incentives, income and risk exposure will impact tenants' input choices and risk-taking behavior, and consequently output. We model tenants as expected-utility-maximizing risk-averse agents who must decide the level and risk profile of inputs to be used on a plot. In particular, a tenant

[5] Ackerberg and Botticini (2002) show that tenants are matched endogenously to contracts (and plots/crops). Randomization also ensures that there are no systematic differences in terms of plot or crop characteristics ex-ante across the different treatment groups. There may still be ex-post differences in tenant characteristics due to differential attrition, which we test for.

[6] Higher expected income may lower an individual's labor supply through a standard income effect. It may also affect incentives for risk-taking, as we demonstrate in Section II. Moreover, since tenants in T1 receive a better contract than what they had initially agreed on, they may increase their effort due to the presence of an efficiency wage. Finally, higher expected income may increase a tenant's access to credit which may enable him to increase the supply of inputs.

can choose between a risk-free cultivation technique or a risky but, in expectation, more productive one. Her compensation is in the form of a share s of the realized output and a fixed payment w, which could be positive (a wage), negative (a rent) or zero. The model predicts that an increase in s leads to an increase in the level of inputs the tenant chooses to employ in cultivation (the 'Marshallian inefficiency' effect); but has an ambiguous effect on her risk-taking, the direction of which depends on the shape of her utility function.[7] On the other hand, an increase in w should have no effect on the level of her investment in inputs, independent of the risk profile of w. A safe increase in w will have a non-negative effect on her risk-taking (positive if the tenant's absolute risk aversion is decreasing with income). Additional exposure to uncorrelated risk will lead to less risk-taking. In terms of output, the effect of increasing s is positive, as long as the effect on risk-taking does not offset the effect on increasing the level of inputs. The effect on output of increasing w depends on how w affects tenants' risk-taking: if higher w leads to greater risk-taking by the tenant, it will lead to greater expected output as well. The experiment allows us to test these predictions.

We find that the fields of tenants with 75% output share generated on average 60% higher agricultural output compared to tenants who were allowed to keep 50% of output (T1 vs. C). We do not find that tenants who received a higher income produced significantly more (or less) output than tenants in the control group (T2 vs. C). We do observe a small, negative and imprecisely-estimated effect of risk exposure on the output level of tenants (T2B vs. T2A).

Next we show how tenants respond in terms of input levels and risk-taking. For input levels, we find that the tenants who retained a higher share of their output (T1) invested more in capital inputs to cultivate their plots. In particular, they used more fertilizer (120% more than the control group) and they acquired more agricultural tools (29% more relative to the control). We also find an increase in their use of unpaid labor (by 64% relative to control), but the effect on total labor hours is imprecisely estimated. In contrast, tenants who received higher income

[7] The latter is a standard result in public finance literature that studies the effect of taxation on entrepreneurial risk-taking (Domar and Musgrave, 1944; Mossin, 1968; Stiglitz, 1969; Feldstein, 1969).

(T2) did not invest more in capital or labor inputs relative to the control group.

We assess changes in the tenants' risk-taking in three ways. The most direct approach is to study the crop-mix the tenants chose to cultivate on their plots. In effect, crops are differently risky assets between which the tenant chooses, conditional on a level of investment. In order to determine the relative riskiness of the different crops, we assess their sensitivity to rainfall and the volatility of their yield. We then study the differential crop choice of tenants across treatment groups.[8] Secondly, we analyze the dispersion of output across treatment groups. Third, we estimate the responsiveness of output to rainfall variation across treatment groups. Across these approaches we consistently find evidence of significantly higher risk-taking amongst tenants with a higher output share (T1), mildly higher risk-taking amongst tenants who receive a risk-free income (T2A), and mildly lower risk-taking amongst tenants who received the risky income transfer (T2B), all relative to control (C). It should be noted that our approach does not allow to measure the returns to risk taking. Standard asset pricing theory and empirical work suggests that they are positive.

We do not find that the increase in output for tenants with higher output share had other adverse effects. In theory, tenants in T1 may have diverted their investments from other plots or reduced their involvement in other income-generating activities to generate the high output we observe on the experimental plots. We find no evidence of such adverse effects: total household income is significantly higher among the tenants in T1 and we find no crowding out of other income generating activities at the household level. Another concern with high-powered incentives is they may lead to over-investment in technologies that maximize short-term output at the expense of long-term soil quality. We collected soil samples from the experimental plots and tested for any impact on indicators of soil qual-

[8]In particular, we study the sensitivity of each crop's yield to rainfall in two ways: first, by exploiting rainfall variation across plots cultivated by the tenants in the control group; second, in a panel data of crop yields in Sub-Saharan African countries from FAOStat. Both methods show that beans are less sensitive to rainfall compared to maize, tomatoes or peanuts. Moreover, the yield of beans has a lower coefficient of variation in the country level panel data. We find that tenants in T1 cultivated more of the riskier crops (maize, tomatoes and peanuts) while there was no significant effect on their cultivation of the safer crop (beans) relative to the control group.

ity.[9] We do not find any evidence that the high-incentive tenancy contracts had led to soil degradation by the end of the experiment.

In Section V we discuss the interpretation of the results. First, we explain that the empirical findings are in line with the predictions of our theoretical framework. Second, we demonstrate that the output increase of tenants with a high share can also be quantitatively accounted for by observed changes in the input levels and risk-taking behavior of tenants, with each contributing about half of the full effect. Third, we simulate the welfare consequences of a higher crop share and find that these are large for reasonable levels of risk-aversion. This is unsurprising given that the gross income of tenants with high output share increases by 140% relative to control. Last but not least we discuss a number of important limitations of our approach: we work with an implicitly selected sample of farmers; we estimate output responses using data from two agricultural seasons which necessarily constitute a particular realization of weather and other risks; the experiment was conducted in a setting where formal sharecropping contracts are uncommon; and the experimental design does not allow to capture potential externalities. We discuss at length whether and how these features of the experiment limit the ability to extrapolate from our findings.

Our paper contributes foremost to the empirical literature on the incentive effects of agricultural tenancy contracts. Rao (1971) shows that output is higher in owner-operated relative to sharecropped farms in India, but a large share of the difference can be attributed to differences in land size. Controlling for farm size changes the sign of the correlation between ownership and output. An important methodological contribution is made by Bell (1977) and Shaban (1987) who use plot level data, and compare output and input levels across plots with different tenancy statuses within the same household, thus controlling for many unobservable household level characteristics. Nevertheless, the endogeneity of contract choice and the presence of unobserved plot level characteristics are potential sources of bias in their findings (Arcand et al. (2007); Braido (2008); Jacoby and Mansuri

[9] In particular, we test for the levels of nitrogen, potassium, phosphorous, organic matter and the Ph-level.

(2009)). Banerjee et al. (2002) show that a tenancy reform which simultaneously changed legal output share of registered tenants and reduced their likelihood to be evicted by the landlord increased agricultural output in West Bengal. However, it is not clear to what extent this effect was driven by the change in tenants' legal crop share or their security of tenure.[10] As far as we are aware, the current paper is the first to provide experimental evidence on the incentive effects of tenancy contracts.

More broadly this paper contributes to the growing literature that seeks to understand the agricultural productivity shortfall in developing countries and identifies policies that increase agricultural productivity and output. Notable contributions are the work by Karlan, Osei, Osei-Akoto, and Udry (2014), who find that farmers in Ghana make riskier production choices when provided with insurance; Duflo, Kremer, and Robinson (2008), who show that subsidies can be carefully designed to increase the adoption of profitable technologies in the presence of hyperbolic discounting; and research by Adamopoulos and Restuccia (2014) and Restuccia and Santaeulalia-Llopis (2017), who show that the reallocation of agricultural land across heterogenous farmers might have large output and welfare gains. We show that policies that effectively strengthen the cultivators' position as residual claimant also have the potential to substantially increase agricultural output.

The paper is also related to recent empirical studies that have demonstrated the role of agents' incentives in other contexts; see, for example, Prendergast (1999) and Bandiera et al. (2011) for the role of contracts in incentivizing workers within a firm. Tenant farmers, compared to typical wage workers, have a wider set of decisions to make, often trading off expected returns with the riskiness of production (Ghatak and Pandey, 2000). In this respect, the decisions of tenant farmers are conceptually closer to those of entrepreneurs or corporate executives, analyzed in public economics (Domar and Musgrave, 1944; Mossin, 1968) and corporate finance (Jensen and Meckling, 1976). This literature highlights the role of output sharing rules for risk-taking and shows that the effect of taxation on risk-taking is ambiguous in a general setup. The sign of the effect depends on the exact shape

[10]Related to the tenure security effect of the reform, an eminent literature demonstrates the role of property rights in driving agricultural decisions and productivity (Besley, 1995; Braselle et al., 2002; Jacoby et al., 2002; Goldstein and Udry, 2008; Hornbeck, 2010; Montero, 2018; Iwanowsky, 2018).

of the tax schedule as well as the agent's utility function (Domar and Musgrave, 1944; Mossin, 1968; Stiglitz, 1969; Feldstein, 1969). Empirical tests of the theory have been limited due to the endogeneity of taxes to income and wealth (Feldstein, 1976). While some papers (see e.g. Poterba and Samwick, 2003) have exploited changes in tax regimes to study household portfolio choice, the evidence on the effect of taxation on entrepreneurial risk-taking is limited. We contribute to this literature by providing evidence that a lower tax (higher output share) increases risk-taking among farmers.

II. Theory

Set-Up Suppose that a tenant's preferences can be represented by expected utility maximization and a Bernoulli utility function $u(c)$, defined over a consumption good c, with $u : \mathbb{R}^+ \to \mathbb{R}$ being increasing, concave and twice differentiable. When assessing welfare consequences, we assume specifically $u(c) = \frac{c^{1-\eta}-1}{1-\eta}$, where η is the (constant) coefficient of relative risk aversion.

The tenant faces two choices: she purchases a bundle of inputs x at unit price p; and she determines the risk profile of returns to her investments. The latter choice represents both which input mix the tenant purchases, and how she chooses to use these inputs. We parametrize this notion by assuming that a tenant's output can be written as

$$y = a\theta f(x) + (1-a)f(x),$$

where $f : \mathbb{R}^+ \to \mathbb{R}^+$ is an increasing, concave and twice differentiable production function, θ is a random variable with positive support, and $a \in [0,1]$ captures the extent to which tenants take on risk. For $a = 0$ the tenant chooses not to be exposed to risk; for $a = 1$ she chooses the maximal level of risk; intermediate choices of a represent a convex combination of the return profiles of these polar cases. We implicitly normalize the return of the risk-free investment to 1. Let the c.d.f. of the distribution of θ be denoted by $G(\theta)$, with support $[\underline{\theta}, \overline{\theta}]$. We assume $\underline{\theta} \in [0,1]$ and $\mathbb{E}_\theta[\theta] > 1$; those are necessary conditions for an interior solution for a. The formulation also implicitly assumes that tenants take output prices as given.

A linear sharecropping contract specifies that the tenant pays a share $(1-s)$ of gross output to the landlord, in addition to a fixed payment. The fixed payment to the tenant can be positive (a wage) or negative (a fixed rent). The tenant may also have additional income. We denote with w the sum of additional income and any payment to the tenant agreed with the landlord.

The tenant's consumption is then $c = s[a\theta f(x) + (1-a)f(x)] - px + w$. She will choose the input bundle x and the risk-profile of investment a to maximize

$$\mathbb{E}_\theta[u(c)] = \int u\left(s[a\theta f(x) + (1-a)f(x)] - px + w\right)\, dG(\theta). \tag{1}$$

This framework captures a number of aspects of a tenant's choice that we consider realistic and potentially important. Firstly, agricultural output is typically subject to aggregate risks that are difficult to insure locally, such as output risks resulting from rainfall and temperature variation or pest outbreaks. Secondly, we model tenants' risk aversion. There is both empirical evidence suggesting that tenants are risk averse, and theoretical reasons to believe that an agent's risk aversion might be important for her productive choices.[11] Third, we restrict attention to linear incentive contracts. This aspect of the model lacks theoretical generality, but not realism: surveys of tenancy contracts show that a large majority of observed sharecropping contracts take a linear form.[12] Fourth, and most importantly, we think of the tenant's problem as choosing both the level of investment and the risk profile of investments. We believe this to be a realistic representation of a tenant's choice. Agricultural tenants typically choose the level of inputs such as their own or hired labor, the intensity of their own labor (often referred to as "effort"), total expenditures on seeds, fertilizer, pesticides and irrigation, amongst others. However, in choosing the specific mix of these inputs, such as the composition of seeds,

[11] Smallholder farmers have been shown to exhibit substantial risk aversion in both survey and lottery based measures of risk aversion (Binswanger, 1980) and farmers' behavior (Karlan et al., 2014). Risk aversion is central to standard explanations for the existence of partial incentive contracts, pioneered by Stiglitz (1974).

[12] Holmstrom and Milgrom (1987) present sufficient conditions for linear contracts to be optimal theoretically.

or how to apply them, they also effectively choose between investments with different risk profiles. Our set-up allows us to study both choices jointly: A change in the terms of the sharecropping arrangement – or, under an alternative interpretation, the effective tax schedule – will potentially lead to a change in the tenant's level of input purchases. A change in the sharecropping arrangement might also change the incentives for risk-taking. Importantly, both of these decisions might interact, and understanding them in isolation might not be possible. The framework outlined here allows us to study the joint determination of the level of investment and its risk profile. It will guide how we interpret the reduced form effects of variation in sharecropping arrangements on outcomes of interest.

This formulation is special in at least two ways. First, a set-up where $f(x)$ is linear in x would be closer to the problem analyzed in the theory of portfolio choice, where typically the level of asset holdings does not alter the distribution of marginal returns of each asset. Second, we assume, given a level of investment x, a particular relationship between the mean gross return of an investment and the associated dispersion around the mean. In a general framework the tenant would choose between a set of investments with unrestricted distributions of returns.[13]

Understanding Tenants' Choices Assuming an interior solution, a tenant's optimal choice of (x, a) is characterized by the following first order conditions:

$$\int u_c \cdot [s[a\theta f_x(x) + (1-a)f_x(x)] - p] \; dG(\theta) \quad = \quad 0 \tag{2}$$

$$\int u_c \cdot [s\theta f(x) - sf(x)] \; dG(\theta) \quad = \quad 0, \tag{3}$$

where $u_c \equiv \frac{\partial u(c)}{\partial c}$. We will denote the elements of the associated Hessian as $D_{ij} = \frac{\partial^2 \mathbb{E}_\theta[u(c)]}{\partial i \partial j}$.

To understand the tenant's input and risk-taking choices it is instructive to first consider (3), the first order condition with respect to a. It captures the trade-off

[13]Conditional on any mean return a preferred investment portfolio will always exist. However, the dispersion of returns around the mean of that portfolio might have a general form. In contrast, our formulation implies a particular relationship: at the mean return $[a\mathbb{E}_\theta[\theta] + (1-a)]f(x)$ gross returns have one specific distribution, with variance $a^2\mathbb{E}_\theta[\theta - \mathbb{E}_\theta[\theta]]^2 (f(x))^2$. A feature of this relationship is that higher mean returns require a tenant to take on additional dispersion of returns.

between higher mean returns and additional risk. It states that the tenant will take on risk until the marginal expected utility from additional risk is equal to 0. Now consider (2) and note that it can be rearranged in two parts as $\int u_c \cdot [sf_x(x) - p]\, dG(\theta) + \int u_c \cdot [saf_x(x)(\theta - 1)]\, dG(\theta) = 0$. The first part captures the increase in the expected marginal utility from increasing the level of returns across investments. The second part captures that a higher x also increases the absolute dispersion of returns, just like risk-taking does. We can derive the following prediction. (All proofs are in Online Appendix I[14].)

Prediction 1. *(Input Effects)*

i. An increase of the tenant's share in output, s, increases level of investment, $\frac{dx}{ds} = -\frac{f_x(x)}{sf_{xx}(x)} > 0$.

ii. An increase of the tenant's income level, w, leaves the level of investment unchanged, $\frac{dx}{dw} = 0$. (This result is independent of the stochastic profile of w.)

The first part of the result captures the intuition that Alfred Marshall had in mind: a higher share of the agent increases the marginal return to investments keeping the costs constant, which increases the level of investments. This result would be straight-forward to demonstrate in a framework where the agent is risk neutral. Prediction 1 demonstrates that it also holds for risk averse tenants, as long as the tenant can adjust the level of risk-taking: the endogenous adjustment of risk-taking allows to offset the risk exposure effect of higher investment levels.[15] Therefore the only effect determining the level of investment is the standard trade-off between expected marginal utility gains and costs. If a risk-averse agent cannot adjust the level of risk-taking, an increase in s would, in addition to the standard incentive effect, also have a risk exposure and wealth effect. These effects might work in opposite direction, which is a well-known result since Pratt (1964) and Arrow (1971), and the sign of the sum of them would be ambiguous. (See Online Appendix I.A.)

It is worth noting that the effect of s on x will be larger when a adjusts endoge-

[14]The Online Appendix is available at the link https://doi.org/10.1093/qje/qjy023.

[15]Note that this also implies that the second order conditions are satisfied.

nously than when a is kept fixed.[16] The intuition for this result is that the tenant does not take into account any effect of x on risk exposure when choosing its optimal level, since risk exposure can be undone by adjusting the level of risk-taking conditional on x – an instance of Le Chatelier's principle. The result is important for the interpretation of our results. As we will show, tenants do adjust the risk level in our setting. If however in some other setting tenants cannot adjust a – for technological, institutional or behavioral reasons – we would expect to see smaller effects of changes in the tenants' share on investment levels.

A useful corollary of Prediction 1 is that $\frac{-f_x(x)}{xf_{xx}(x)}$ is a sufficient statistic for the elasticity of investments with respect to the tenant's share s. In particular, no knowledge of the specific utility function is required to predict changes in the investment level when changing s. This implies that estimates of $\frac{dx}{ds}$ have external validity as long as production choices are common – even though tenants might have heterogeneous utility functions.

Lastly, an increase in w is predicted to leave the choice of x unchanged. This result also holds when the increase in w is stochastic, independent of the type of correlation structure between θ and w. Both additional income and risk exposure do not affect the choice of x.

Next, we turn to the effects of the contractual terms on the tenant's risk-taking behavior.

Prediction 2. *(Risk-Taking)*

i. *An increase in s has an ambiguous effect on risk-taking, a when $u(\cdot)$ exhibits DARA.*

ii. *Consider an increase in safe w. Then the level of risk-taking, a, increases when $u(\cdot)$ exhibits DARA, $\frac{da}{dw} > 0$.*

iii. *Consider an increase in stochastic w, with w independent of θ. An increase in w*

[16] If the level of risk-taking adjusts endogenously, we can write $\frac{dx}{ds}$ as

$$\Psi \times \frac{-1}{D_{xx}} \int u_c \cdot [a\theta f_x(x) + (1-a)f_x(x)] \, dG(\theta),$$

with $\Psi := \frac{D_{xx}}{\int u_c[sa\theta f_{xx}(x)+s(1-a)f_{xx}(x)]} > 1$. Compare this to the incentive effect in Online Appendix I.A.

has an ambiguous effect on the level of risk-taking, a, when $u(\cdot)$ exhibits DARA.

A large literature in public finance studies the theoretical effect of taxation on risk-taking, especially entrepreneurial risk-taking. It analyzes the risk-taking effects of taxation in isolation of effects on investment levels. It finds the sign of the effect of taxation on risk-taking to be indeterminate in a general setup; predictions depend on the shape of the tax schedule and the utility function (Domar and Musgrave, 1944; Mossin, 1968; Stiglitz, 1969; Feldstein, 1969).

The first part of Prediction 2 shows that this conclusion carries over to our framework. An increase in s implies a higher exposure to risk – both mechanically and because x increases – as well as higher wealth. The income effect is described by part (ii): absolute risk-taking increases for an agent characterized by a DARA utility function. This is nothing more than the name-giving property of such utility functions. Part (iii.) in combination with part (ii) highlights that additional exposure to income risk dampens the tenants willingness to take on additional risk through a. This explains part (i): Under DARA further assumptions are needed to sign the effect of s on risk-taking.[17] Note that DARA is likely a plausible assumption. Therefore this result also highlights how understanding the effect of the tenant's share on risk-taking is an inherently empirical question.

In summary, the theory predicts that an increase in the tenant's share s increases the level of inputs purchased. This is an incentive effect, both income and risk-exposure have no effect on the level of inputs. And an increase in s has an ambiguous total effect on risk-taking. The income effect of the increase in s has a positive effect on risk-taking and the risk-exposure effect of the increase in s has a negative effect on risk-taking.

Lastly, much of the interest in sharecropping contracts is concerned with designing contracts and regulation to increase agricultural output. Predictions 1 and 2 do translate into implications for expected output.

Prediction 3. *(Output Effects)*

i. The tenant's expected output increases with s, as long as $\frac{da}{ds}$ exceeds some negative

[17] Under CARA an increase in safe w leaves risk-taking unchanged, which is again the name-given property of such utility functions, and an increase in stochastic w decreases risk-taking.

bound.

ii. The tenant's expected output increases with w if and only if $\frac{da}{dw} > 0$.

This result highlights how an increase in the tenant's share does not necessarily need to translate into higher expected output. The reason is that the increase in output implied by the Marshallian incentive effect might be offset by the tenant taking on less risk. However, moderate levels of risk reduction still imply increases in expected output, and increases in the level of risk-taking amplify the effect of the tenant's share on output. Increases in the tenant's income w do not effect the input choice, therefore any effect on expected output from changes in w is coming from changes in the level of risk-taking.

The predictions are summarized in Supplementary Table I in the Online Appendix.

III. Methods

III.A. Setting

In order to test the theoretical predictions above, we implemented a field experiment in collaboration with BRAC. Uganda has one of the youngest populations in the world. In 2014, 48% of Uganda's population of 35 million was aged 15 or below, while – as a point of comparison – the figure is 21.2% in the US. Among the youth, young women are particularly at risk as they are more likely to drop out of school at an early age and face social and economic constraints in entering the labor market. BRAC operates a program called Empowerment and Livelihood for Adolescents (ELA) with the objective to empower young women in Uganda. At the core of this program is to open, finance and operate youth "clubs" for girls. In rural areas, each club is assigned to a village. BRAC provides vocational and life skills training, as well as various social activities through these clubs.[18] As part of these efforts, BRAC decided to lease plots of land to women who were interested

[18]See Bandiera et al. (2018) for further details of the ELA program.

in becoming farmers. Women in Uganda head 26% of rural households and grow 70% – 80% of food crops, yet own less than 8% of the land (Nafula, 2008). Moreover, even on plots of land controlled by women, productivity is likely to be lower due to differential access to factors of production (Udry, 1996). In order to assist young women who wanted to become farmers but faced difficulties in setting up their farm activities, BRAC started implementing the intervention that forms the setting of our experiment. During the design phase, focus group discussions with club members revealed that due to credit constraints and concerns about the riskiness of cultivation, most potential tenants would not find a fixed-rent contract suitable. As such, BRAC decided to implement the intervention under a sharecropping arrangement.

III.B. Timeline

Season 0. In July 2013, BRAC selected 300 clubs in Eastern, Western and Central regions of Uganda to implement the intervention.[19] At most one club was selected per village; for the purpose of our experiment the club and village level are hence the same. BRAC then attempted to rent a plot of agricultural land of roughly 0.5 acre close to the club, and searched for up to three club members willing to rent the plot under a $s = 0.5$ sharecropping contract, with no fixed payment component, for one season. In 285 clubs both land and potentially interested tenants were found. Figure I shows the location of these clubs. The interested girls were then offered the land, in an order randomized by the authors, until one of them decided to take up the offer and become a tenant. Both a plot and a farmer who actually signed up as tenant of the plot were found in 259 clubs. The tenants cultivated the plot for the following agricultural season, from September 2013 to January 2014 (henceforth 'Season 0'), which served as a pilot season.

[19]Uganda has four main regions: Eastern, Western, Southern and Northern. The Northern region differs significantly from the other three in terms of geography, climate and socio-economic organization.

Seasons 1 and 2. We collaborated with BRAC to implement the experiment in two agricultural seasons of 2014, spanning from March to July ('Season 1') and September 2014 to January 2015 ('Season 2'). In Season 1, the plots were advertised to be available for tenants under a 50% sharecropping contract with no fixed component. Tenants who had cultivated the plots in Season 0 were given priority. Roughly half of the Season 0 tenants decided to continue in Season 1. In the remaining cases new tenants signed up. Additionally BRAC decided to scale up the program for Season 1, both by renting an additional plot in clubs where a plot was rented in Season 0, and also by re-attempting to rent plots close to clubs for which no plots were found in Season 0. As a result of these changes 304 tenants signed a 50% sharecropping contract at the beginning of Season 1. In preparation of Season 2, the plots were again offered under a 50% sharecropping contract with no fixed component, with priority given to Season 1 tenants.

Within-Season Procedures In each agricultural season BRAC provided the tenants with agricultural training. The training taught best-practice recommendations on (a) how to prepare the land and plant, (b) grow, and (c) harvest crops. The first training session took place before planting, the last training session took place before harvesting.[20] During the first of these training sessions, BRAC also provided the tenants with a bundle of high yield variety seeds. In Season 0 tenants were given maize, beans, cabbages and tomato seeds, for a total seed bundle value of 12 PPP USD; in Seasons 1 and 2 tenants were given maize, beans, and peanut seeds for a total seed bundle value of 32 PPP USD.[21] The training focused on techniques related to these crops, respectively. During the first training session the tenants signed the 50% sharecropping contract, valid for one season, in the presence of the BRAC program assistant as well as another witness.

[20] In Seasons 1 and 2 there were only two training sessions, and topics (a) and (b) were both taught during the first training session. In Season 0, topic (b) was taught in a separate mid-season training session.

[21] In two areas potato seedlings were provided instead of peanuts. In that case the seed bundle value was 28 PPP USD. BRAC decided to change the seed mix provided to farmers between Season 0 and the following seasons after program assistants reported that farmers preferred peanuts or potatoes to tomatoes and cabbages.

III.C. Experiment

Treatments. The experiment was implemented in Seasons 1 and 2.[22] At the start of both Season 1 and Season 2 the plots were advertised with a 50% sharecropping contract. Tenants who agreed to rent the plot under that contract signed it during the first training session. The contracts explicitly stated and the tenants were clearly told that the arrangement would last for one season, and there was no guarantee that their tenancy would be renewed in the future.

Tenants were assigned to one of four treatment conditions (see Figure II):

Control (C): Tenants keep the $s = 0.5$ contract.

High s (T1): Tenants are offered a contract with $s = 0.75$.

High w, safe (T2A): Tenants keep $s = 0.5$ and are offered a fixed payment w, with w being set to 25% of Season 0's median harvest value, to be paid at the time of the next harvest.[23]

High w, risky (T2B): Tenants keep $s = 0.5$ and are offered a payment w, with w being 20% of Season 0's median harvest value with probability 0.5, and 30% of Season 0's median harvest value with probability 0.5, to be determined and paid out at harvest time.

We refer to the union of T2A and T2B as T2.

While tenants were assigned to the same treatment condition across seasons, in both Season 1 and 2 the tenants initially signed a 50% sharecropping contract, and only heard about the update to their contract when they were contacted by BRAC program staff after the training of the respective season. During these calls tenants were reminded that they have signed a $s = 0.5$ sharecropping agreement, and comprehension checks were performed and repeated until passed satisfactorily. Tenants in treatment groups T1 and T2 were informed about the change in the

[22] In the study area there are two agricultural seasons per year. The first one extends from March to August, the second from September to February. Rains in the first season are usually heavier, and the chance of crop failure is lower.

[23] The level of the transfer was calculated at the BRAC branch office level, using data on the harvest value of all Season 0 farmers. Note that Season 0 is the baseline season; no experimental variation in contracts had been induced or announced in Season 0.

terms of their contract, and comprehension checks were performed. The tenants in group T1 and T2 were told that they had been selected for the more favorable contract by a lottery. The terms of the new contract were explained to them in detail and were stated as applying to the upcoming season. Tenants in T2 were informed of the amount of cash transfer they would receive at the end of the season, those in T2B were explained the details of the lottery (i.e. the risky cash transfer) they would participate in. After the phone calls the BRAC program assistant delivered a letter to the tenant specifying the updated contract. Additionally all tenants received this information in a text message.

Rationale. The objective of the research project was to understand the nature and magnitude of a number of specific effects of agricultural land tenure systems on the behavior of the tenants on input choices, risk-taking and agricultural output. The experimental design allows us to test the Marshallian hypothesis and identify the mechanisms behind it.

Firstly, BRAC advertised the same contract (with s=50%) in all treatment groups. This is a version of the seminal experimental design in Karlan and Zinman (2009) and controls for selection effects. As such, there is no reason to believe that tenants who sign up are systematically different on any unobservable characteristics across the different treatment groups.

Secondly, after the tenancy contracts were signed, tenants in T1 were offered s=75%, in order to generate variation in the tenant's share in output. We chose to implement a change to the tenancy contracts in T1 which we surely knew was dominating the original contract from the perspective of the tenant, in order to avoid design-induced attrition. The exogenous variation in output share induced in T1 is key to test the incentive effects of sharecropping contracts.

Third, the comparison of input intensities and output levels between C and T1 does not necessarily allow us to estimate the incentive effect of a higher share in the output. Increasing a tenant's share of the output does not only have an effect on the marginal revenue of the tenant, but might also have an income effect. A classic income effect driven by the tenant's labor-leisure choice would suggest that individuals at higher expected income levels may choose to work less. Higher expected income may also increase the tenant's access to credit which may enable her to increase the supply of inputs. In order to test for the collection of these effects, we introduce T2. In this group, tenants are offered the same output share (s=50%) as in C, but receive a fixed payment. This allows us to estimate the size of the income effect. If this estimate is 0, the comparison of C and T1 estimates the incentive effect.[24]

Finally, within T2, half of the tenants were offered a risk-free cash transfer (T2A) while for half of them, part of the payment was based on a lottery (T2B). The expected transfer amount is the same across the two groups. To the extent that any income effect exists in T1, this is the effect of a *risky income*, since agricultural output is necessarily stochastic from the point of view of the tenant. Any income effect likely varies with the risk profile of the additional income, either because the tenant is not risk neutral or because credit access is affected by the stochastic nature of the additional income. The treatment T2B allows us, by comparison with T2A, to test whether indeed the risk profile of income is important to understand tenants' behavior. We designed T2B such that the first and second moments of the distribution of additional income in T2B roughly match the first and second moments of the distribution of additional income induced by treatment T1. However, higher moments of these two distributions are likely different; further the additional risk in T2B is perfectly uncorrelated with agricultural yields, whereas the additional income risk induced by T1 is perfectly correlated with agricultural yields. This should be kept in mind when interpreting the results.

Implementation Challenges. In implementing the experimental design we faced two challenges. First, the amount of additional income provided in T2 was determined as 25% of the BRAC branch level median output of Season 0. This might incorrectly reflect the (expected) income effect of treatment condition T1, which it would ideally match. We will address this when discussing the main effect of

[24] If the estimate of the income effect is significantly different from 0, we can estimate a structural model of labor supply which features two structural parameters, one governing the income effect, and one governing the incentive effect.

treatment condition T2 relative to treatment condition T1 in Section IV.B. Second, the information about the updated contract was to be provided shortly after the first training session, prior to the start of the agricultural season. This feature was implemented as such in Season 1. However, in Season 2, due to administrative constraints on the ground, the information about the updated contracts was provided to the tenants only in January 2015, three months late into the agricultural season. This needs to be kept in mind when interpreting the findings.

Randomization. We grouped the 300 clubs originally designated as potential study sites into clusters of three clubs (henceforth referred to as 'blocks'), with the heuristic objective to minimize within-block geographic distance. The study clubs were typically geographically bunched – see Figure I for a visualization of this. We grouped clubs into these large clusters (henceforth referred to as 'strata'). Assignment to treatment was randomized at the club and hence village level at the beginning of Season 0. We assigned equal fractions of the 300 potential study clubs to C, T1 and T2, stratified by blocks. Within T2 clubs we assigned 50 clubs to T2A and T2B, stratified by strata.

III.D. Measurement

We collected data through two types of survey instruments, a tenant level survey ('Tenant Survey') and a plot level survey designed to estimate outputs on the field ('Crop Assessment').

The Tenant Survey collected information on the tenants' and their households' demographic and socioeconomic characteristics. We recorded their educational history, health status, labor supply and employment characteristics, household structure, detailed agricultural practices and output on each of the household's cultivated plots, including the plot rented from BRAC, ownerships status of plots, the household's asset holdings, and consumption expenditures, the tenant's savings and loans. The survey was administered by enumerators who were hired by BRAC and managed by the research team. The survey was administered to all potential tenants before each season of cultivation. It was also administered to all tenants about one month after the end of the season. It provides baseline information on the tenants in our sample (collected at the end of Season 0), as well as followup information at the end of Seasons 1 and 2.

A central challenge was to measure agricultural output in a way that is immune to manipulation by the tenant. Neither self-reported yields, nor crop-cutting and whole-plot harvesting techniques – commonly used to measure agricultural output[25] – satisfy this criterion.[26] Instead from Season 1 onwards we conducted a plot level survey of yields shortly before maturity of the crops ('Crop Assessment').[27] For this survey we hired students of agriculture as enumerators. They measured the size of plots and its parcels using GPS trackers; collected exhaustive data on the plot, including agricultural practices applied; took soil samples and tested levels of nitrogen, phosphorus, potassium, organic matter and soil pH. Importantly, to assess the output, they placed 1.5m $\times$ 1.5m quadrants on representative sections of the plot's parcels (8 quadrants per acre), and recorded detailed plant characteristics within each quadrant. Further they were trained to assess the expected output at harvest time for every plant in every quadrant. In order to value the output of a given crop, we conducted a survey of crop prices at the nearest local markets at harvest time. While in theory it is possible that local prices may be affected by the treatment assignment, in practice it is unlikely as the plots are small (0.5 acre on average) and therefore the crops harvested from the experimental plots make up only a very small fraction of the total output in each village. Hence, any general equilibrium effects on local prices are unlikely. Starting from Season 1 these estimates were used to determine the tenants' due payment, which was collected by

[25] For a comprehensive list of available techniques, see Fermont and Benson (2011).

[26] Notice that tenants across treatment groups have a differential incentive to misreport their yields. Further, farmers might harvest mature crops at any time before the arrival of the surveying team and again the incentives to do so are differential across treatment groups.

[27] In Season 0 the crop assessment was conducted by BRAC: Two BRAC program assistants, the tenant, and an enumerator visited the plot at harvest time and surveyed plant density, quality and other characteristics for maize, beans, tomatoes and cabbage, and estimated the plot size. In addition the tenants were asked to report the recalled amount and value of crops that had already been harvested, both for sale or own consumption. This procedure turned out to have a number of drawbacks. One drawback is it was conducted shortly before the harvest time of maize. The harvest time of other crops, such as beans and tomatoes for example, would likely have been earlier.

BRAC field officers.

III.E. Sample, Attrition and Seasons

Sample. Subsequently we report results using data from the Tenant and Crop Assessment surveys in Seasons 1 and 2. All analysis is based on the sample of tenants who signed the tenancy contract in the beginning of Season 1 and the plots of those tenants. We do not report results for tenants who only started renting a plot in Season 2. Figure II provides a visual summary of the experiment's setup, timeline and sample sizes in each treatment group.

Attrition. Of the 304 tenants who signed a tenancy contract in the beginning of Season 1, we successfully surveyed 253 tenants during Tenant Survey 1, and we surveyed the plots of 228 tenants in Crop Assessment 1.[28] Supplementary Table XI tests for differential attrition during Season 1. In the control group, 24% of tenants did not have a Crop Assessment in Season 1 and 20% of tenants could not be surveyed in the Tenant survey. The attrition rates in the treatment groups were similar to the control and to each other. The table shows that any differences in attrition rates across the different groups are not statistically significant.

As described in Section III.B, tenants who participated in the first season of the experiment were invited to sign a new contract in the second season.[29] In Season 2 we surveyed 179 of the Season 1 tenants in Tenant Survey 2, and we surveyed the plots of 192 of the Season 1 tenants in Crop Assessment 2.[30] In Supplementary Table XII, we test if the attrition rate in Season 2 – defined as a successful Crop Assessment or Tenant survey – was differential across the treatment groups. Dif-

[28] This excludes plots on which the measured output was above the 99th percentile of the distribution of measured outputs, which we trimmed. Of those 228 tenants, 195 had rented one plot, 16 had rented two plots and 1 tenant had received 3 plots. There are therefore 262 plots from Season 1 in our dataset.

[29] In most cases where the tenants from Season 1 did not want to carry on cultivating the plot in Season 2, BRAC found replacement tenants. However, since this round of recruitment was carried on after the random assignment into treatment and control groups, we exclude these replacement tenants from the analysis in order to control for any selection effects.

[30] This excludes plots on which the measured output was above the 99th percentile of the distribution of measured outputs, which we trimmed. Of those 192 tenants, 173 had rented one plot, and 19 had rented two plots. There are therefore 211 plots from Season 2 in our dataset.

ferences in the rate of attrition are not significant throughout. They are also small in quantitative terms for the Crop Assessment 2 survey; however, the attrition rate in Tenant Survey 2 is around 11 percentage points higher amongst treatment tenants.

While it is comforting that we do not observe differential attrition across treatment groups, the average attrition rate in our experiment is high. This is likely because the tenants were young women who at the start of the experiment were living in their parents' household and were not married, and geographic mobility amongst this group is relatively high. Throughout we will probe the robustness of our findings to different bounding exercises (Lee, 2009; Fairlie, Karlan, and Zinman, 2015) where the bounds assume the tracked sample is either negatively or positively selected. These are described in detail in Section IV.A below. The key results of the paper are robust to making extreme assumptions about the selection of attritors.

Balance. Table I provides balance tests for the baseline characteristics of the tenants and plots. Panel A present tenant characteristics such as their age, schooling, marital status, household demographics and socioeconomic status. The data was collected at the end of Season 0, prior to the start of Season 1. The average tenant in the sample is 21 years old, has 8 years of schooling, has 2 children and lives in a household with 5.4 people; 51% of the tenants are married. These observable characteristics are balanced across treatment groups. Out of 45 pairwise tests comparing C, T1 and T2 for each characteristic, we find that only one is significantly different at the 10% level based on randomization inference p-values: tenants in T1 had higher consumption expenditure than those in T2. These differences are unlikely to be important for the interpretation of our results.

Panel B presents plot level characteristics collected in Crop Assessment 1. That survey was conducted towards the end of Season 1. Therefore we consider immutable plot characteristics only for the balance checks. In addition summary statistics for total rainfall (pooled sample of both seasons) are reported. Across characteristics we find no economically meaningful and statistically significant differences between plots characteristics across treatment arms.[31]

Experimental Seasons. In Supplementary Figure I we describe the weather conditions on experimental plots during the experimental seasons relative to the typical weather conditions. We calculate the ratio of the estimated rainfall on experimental plots during each month of Season 0, 1 and 2, relative to the historic average rainfall in the same calendar month in the same area. We depict the distribution of that ratio across experimental plots, for each month separately. The figure shows that, on average, the weather conditions during the experimental seasons were similar to typical weather conditions. This suggests that none of our results will likely be driven by unusual weather events. The figure also shows that across plots there is substantial heterogeneity: some experimental plots experienced much lower or higher rainfall than is typical in the area during a calendar month. We will exploit that cross-sectional variation when estimating the responsiveness of yields to weather conditions in Section V.B.

IV. Results

IV.A. Estimation

To identify the treatment effects of different contractual variations, we estimate:

$$y_{ict} = \textstyle\sum_{k=1}^{2} \lambda_k T_{ik} + \delta_s + \epsilon_{ict}, \tag{4}$$

where y_{ict} is the outcome of interest for tenant i from club c in season t; T_{ik} is an indicator variable equal to 1 if tenant i belonged to a club of treatment group k and 0 otherwise, and δ_s are strata fixed effects. The sample includes tenants who were contracted at the beginning of season 1, prior to randomization. We use observations from both seasons 1 and 2 in order to improve statistical power.

[31] The only statistically significant differences are in terms of longitude and latitude: plots in treatment groups T1 and T2 tend to be located slightly to the North and plots in treatment group T2 tend to be located slightly more westernly. However, the magnitude of these differences is small: a difference of 0.013 degrees in latitude or longitude corresponds to roughly 1.5km around the equator. This difference is highly unlikely to explain differences in agricultural yields.

The key parameters of interest are λ_k, the difference between outcomes of tenants who were assigned to treatment k and the control group. Under the identifying assumption that the control group represents a valid counterfactual, λ_k identifies the causal effect of the change in tenant i's contract on y_{ict}. In all regressions we report standard errors, clustered at the village level (the unit of randomization).

Throughout the paper, the p-values associated with hypothesis tests are calculated using randomization inference (Fisher's exact test). We estimate the coefficient of interest in 1000 alternative random assignments.[32] In each iteration we cluster standard errors at the village level, and record the distribution of the F-statistic associated with the hypothesis of interest. The randomization inference p-values report the percentile of the F-statistic found under the actual treatment assignment in the distribution of F-statistics found under alternative treatment assignments.

In order to assess the sensitivity of our findings to differential attrition (see Section III.E), we calculate bounds that adjust for differential attrition across the treatment and control groups under different assumptions regarding the positioning of the attritors within the distribution. 'Lee bounds' (Lee, 2009) trim observations from above (below) in the group(s) with lower attrition, to equalize the response rates across the treatment and control groups.[33] We then re-estimate the treatment effects in the trimmed sample to deliver the lower (upper) bounds for the true treatment effects. We also calculate alternative bounds, following Fairlie et al. (2015). For non-responders we impute – within treatment groups – the mean minus (plus) a specified standard deviation multiple of the observed distribution of outcomes in that treatment group. We then re-estimate the treatment effects in the sample including imputed data to find their lower (upper) bounds.

[32] A treatment assignment is an N-component column vector, denoted $\mathbf{A}$, with i^{th} element $A_i \in \{C, T1, T2A, T2B\}$. Further denote the set of all potential treatment assignments, given our randomization procedure, as $\mathcal{A}$. We sample the 1000 alternative treatment assignments from $\mathcal{A}$ by running the same code which generates the actual treatment assignment another 1000 times to generate alternative treatment assignments.

[33] In particular, we find – by season – the group with highest attrition, and then delete – by season – observations with the highest (or lowest for the upper bounds) values in the other treatment groups until we have the same attrition rate as in the group with the highest attrition.

IV.B. Effects on Output

We start by discussing the effects of a higher output share, income and risk exposure on output levels and yields.

Output Level. Table II presents the treatment effects on the total output (of all crops) that was observed on the plots during the pre-harvest crop assessment surveys. Column 1 shows that the average tenant in the control group had an output of USD 95 (at PPP). Relative to that, tenants in T1 had USD 56 more output on their plots. This implies that the 50% increase in their output share (from 50% to 75%) increased their output by 60%. On the other hand, tenants in group T2 had USD 5 more output relative to C, but this is imprecisely estimated. Moreover, the difference between T1 and T2 is significant (p-value=0.023). Overall, these findings imply that the tenants who were given a higher output share were more productive, and this was driven by the incentive effect rather than an income effect.[34]

Column 2 shows the effects for groups T2A and T2B separately. There is no significant difference between the coefficients of T2A and T2B. This implies that the risk profile of additional income does not play a significant role as T2A and T2B had similar effects on output. This reinforces the idea that the effect of treatment status T2 does capture any income effect induced by treatment condition T1. Nevertheless, it is important to note that the point estimates of T2A and T2B have different signs. Moreover, the difference between T1 and T2A is large (the magnitude of the point estimate for T1 is more than thrice as large as that of T2A), but not statistically significant. This suggests that some tenants in T2A, who were promised a safe income transfer at the end of the season, may have generated higher output than the control tenants while for tenants with the risky income transfer (T2B) this was not the case.[35]

[34]The finding that T2 tenants did not generate more output while T1 tenants did suggests that an efficiency wage story or a behavioral mechanism based on reciprocity are unlikely to be driving the effect of T1. If tenants in T1 were more productive because they received a better deal than they expected and wanted to work hard to reciprocate this favor (or to maintain it in the future), then we should see a similar effect on tenants who were given a cash transfer.

[35]Supplementary Table XIII in the Online Appendix shows the effects on self-reported output. The level of output is lower in all groups and while the signs of the point estimates are similar, the magnitudes are much smaller. This highlights the importance of using observed as opposed to

Figure III shows the cumulative distribution function (CDF) for output in each treatment group. One can see that the CDF of output for tenants who were assigned the high-incentive contract (T1) lies to the right of the CDF for tenants with the standard contract (control group). This implies that the differences in average output levels reported above are not driven by a particular group of tenants responding to the high-incentive contract, but rather by an effect throughout the distribution, in particular from the median upwards. The figure also shows that tenants in T1 performed better than the tenants who were given a cash transfer (T2), which demonstrates that the effects are not driven by the increase in expected earnings. A summarized version of these findings is presented as a box plot in Supplementary Figure II.

Yield. The rest of Table II presents the effects on yield, measured as output per square meter. We find that an increase in the tenant's share of output from 50% to 75% increases her yield by 0.074 USD per m^2 (p-value=0.024). We find no income effect in the specification of column 3 where we do not differentiate between T2A and T2B tenants (p-value=0.993). When estimating the effect of T2A and T2B separately in column 4 we find a small positive effect of treatment condition T2A and a negative effect of T2B. None of the effects are significant at conventional levels. These effects are qualitatively similar to those on total output.

Robustness. Supplementary Table XXIV provides attrition bounds for the effects on output. Overall, the estimates are robust to different adjustments for differential attrition.

In Table II, output value is trimmed at the top so that the top 99% of each treatment group is coded to missing. Effects without trimming are reported in Supplementary Table XIV where we find an even larger effect for being assigned to T1, and no significant effect of being assigned to T2. The larger effect of T1 in the non-trimmed results are driven by a handful of highly productive tenants in T1. Therefore, we rely on the trimmed observations as the main results.

self-reported information on output for our methodology.

In Section III.C we discussed that the income transfer in T2 might have been different from the (pre-season expected) income effect of treatment condition T1.[36] Since the income transfer in T2 was determined at the branch level, there is branch level variation in the ratio of the income transfer we implemented over the income transfer we should have implemented. In Supplementary Table XV we exploit this variation to assess whether a mis-calibration of T2 could explain why we do not find any significant income effect. In particular, this table presents results of regressions analogous to those in Table II, with the only exception that T2 is a continuous variable measuring the aforementioned ratio. We proxy the pre-season expected income effect of T1 by half the realized output of tenants in the control group in the respective season, calculated at the branch level. To the extent that this is a suitable proxy, the ratio will be 1 in branches and seasons where the actual income transfer in treatment group T2 matches what we should have implemented. And it is proportionally higher (lower) in branches where the income transfer in treatment group T2 is higher (lower) than what we should have implemented. Under the assumption that the marginal income effect is constant, the coefficient on T2 will then estimate the true income effect of T1. The analogous statement holds for T2A and T2B. The results in Supplementary Table XV indicate that our previous conclusions in Table II do still hold with this alternative definition of the treatment variable. In particular, we continue to find very similar, quantitatively large, significant effects of treatment condition T1 on output, even though the level of significance decreases somewhat. In contrast, we do not find any significant income effect on output levels.

A concern with measuring output in our setting is that farmers have differential incentives to hide output. For that reason we do not rely on farmer-reported output measures or a crop-cutting survey conducted at harvest, but instead the pre-harvest Crop Assessment as discussed in Section III.D. To address concerns whether these efforts were sufficient to avoid differential output hiding across

[36] An alternative experimental design would have been to link the income transfer in T2 to the season's realized output in geographically close control clubs. This would have circumvented the challenges we faced in calibrating T2. That design requires to inform participants of the existence of other treatment conditions, which our design does not require. Whether this is an important advantage will depend on the specific setting.

treatment groups, we examine the heterogeneity of the treatment effects on output, with respect to the timing of the crop assessment survey in each season.[37] Supplementary Table XVI repeats the analysis of Table II, with the exception that we include the interactions of the treatment indicators with *Survey Day* measuring the number of days between the crop assessment conducted on a tenant's plot and the first day of the crop assessment survey. There is no sizable or statistically significant evidence of differential harvesting between T1 and T2/C, both when considering the interaction terms individually and when testing the null of no differential treatment effect by survey days across all treatment arms jointly.[38] Online Appendix V.C. presents additional robustness checks.[39] Taken together, these robustness tests build confidence that differential incentives to hide output are unlikely to be driving the effects on output.

A separate concern is whether the pre-harvest Crop Assessment is closely related to realized harvests. Burchardi et al. (2018) validate the pre-harvest Crop Assessment methodology in a setting where farmers have no incentive to hide output. They conduct, also amongst Ugandan farmers, a crop assessment prior to harvest and maturity of maize and conduct a crop-cutting survey at harvest time. They find that output measured through the pre-harvest Crop Assessment is strongly proportionately related to output measured through a crop-cutting survey.[40]

[37] The intuition is that on early days of the crop assessment differential harvesting is less plausible; if we observe larger output differences between T1 and T2/C on later harvesting days, this would be worrisome, since it might indicate that farmers in groups T2 and C try to harvest crops before the arrival of the crop assessment survey team.

[38] If anything we observe larger differences between groups T1 and T2/C on plots that were surveyed early, though none of these differences are significant.

[39] During the crop assessment farmers are asked to self-report whether and what quantity of crops they harvested earlier in the season. Supplementary Table XVIII analyzes whether treatment status had any effect on early harvesting. We do not find any significant or sizable effect of treatment status on early harvest behavior at the extensive or on the intensive margin. Further the level of crops reported as being harvested prior to the crop assessment is low. However, in interpreting this robustness check it should be kept in mind that the early harvest data is self-reported. Another robustness check is to assess the heterogeneity of the treatment effects with respect to distance between the plots and the nearest market. The intuition is it may be easier for the tenants to sell their crops the closer their plots are to the market, and tenants in the control group may be particularly prone to do so, since they have a greater incentive to sell their produce before the arrival of the survey teams. Supplementary Table XVII reports the results. We do not find significant heterogeneity in the output effect with respect to distance to nearest market. We thank an anonymous referee for suggesting these tests and the heterogeneity analysis by survey day.

[40] Crop-cutting surveys are considered the gold standard of agricultural output measurement, but infeasible for the purpose of this project given potential output hiding prior to harvest.

IV.C. Effects on Input Use

Next we seek to understand how tenants adjust their behavior to generate the output effects found above. First we present the results on changes in input choices. Prediction 1 in Section II says that the increase in output share (s) for tenants in T1 should induce them to use more inputs while the increase in the output-independent income (w) of tenants in T2 should have no impact on their input use. We test these predictions using data from the tenant surveys conducted at the end of each season and recorded tenants' use of labor and capital inputs.

Capital Inputs. The tenants were asked to report the amount (if any) of any type of fertilizer and insecticide they used; and whether they acquired any agricultural tools during the past season.[41] Table III presents the effects of the treatment(s) on indicators of tenants' investments in capital inputs. Panel A of the table shows the effects on the extensive margin, while panel B presents the effect on the intensive margin (monetary value) of each input used.[42] In the first column, the outcome is any type of fertilizer used (either chemical or organic) by the tenants. Consistent with evidence from other East African settings (Duflo et al., 2011), fertilizer use was low among tenants in our sample. Only 28% of the tenants in the control group reported using any fertilizer on their plots. As a result of the higher output share, tenants in T1 were 9.4 percentage points (ppt) more likely to use any type of fertilizer. This corresponds to a 34% increase relative to the control group. While this effect is large, it is not precisely estimated at conventional levels (p-value=0.176). Panel B shows that the intensive margin effect on fertilizer use is even larger (in percentage terms) and precisely estimated. Tenants in T1 used on average USD 1.13 more fertilizer, which is 118% more compared to the average tenant in the control group. The corresponding effects for T2 are imprecisely estimated, although the point estimates are positive and not statistically different from

[41] All tenants were provided seeds by BRAC and, while they were free to use other seeds, only 13% of tenants reported using any seeds from another source, and this rate was not different across the treatment and control groups.

[42] For fertilizer and insecticide used, the monetary value corresponds to the amount spent on the relevant input used for the experimental plot; while for tools the monetary value corresponds to the total value of agricultural tools that the tenant owned at the time of the survey.

the effects of T1. The test of equality between the treatment effects of T1 and T2 results in a p-value of 0.310 (0.350) for the extensive (intensive) margin of fertilizer use – reported at the lower section of each panel.

The second column of Table III displays the effects on insecticide use. In the control group, 28% of tenants reported using insecticide and spent on average USD 1.8 on it. Relative to the control, insecticide use was not significantly different among tenants in T1 or T2, neither on the extensive nor on the intensive margin. However, tenants in T1 spent significantly more on insecticide relative to tenants in T2 (p-value=0.046). The third column of the table shows that tenants in T1 were 9 ppt more likely to have purchased or acquired tools, and at the end of the season, the value of agricultural tools owned by the respondent was higher by USD 11 in T1 (30% relative to C). This latter effect is precisely estimated. We find no such effect for tenants in T2 and the difference between the coefficients of T1 and T2 is also statistically significant (p-value=0.059).

We have discussed the results of the treatment effect on a number of sub-categories of capital inputs. Testing multiple hypotheses poses well-known challenges to the interpretation of p-values. We present results of two approaches to deal with these challenges. First, in the final column of Table III we use an aggregate index that combines the three indicators presented in the table. To construct this index, we first standardize each outcome into a z-score, by subtracting the control group mean at the corresponding survey round and dividing by the control group standard deviation. We then take the unweighted average of all the z-scores, and again standardize to the control group. The results show that while there were no significant differences on the extensive margin, the tenants in T1 spent on average 0.2 standard deviations more on capital inputs compared to tenants in the control group. The corresponding effect for T2 tenants is -0.05 standard deviations and imprecisely estimated (the difference between T1 and T2 is significant with a p-value of 0.080). Second, we estimate the equations in columns 1 through 4 jointly, and then test the null hypothesis that a specified restriction holds in all estimating equations across columns. The results of these tests are consistent with what we found before when constructing an index: There is no robust evidence for an extensive margin effect. On the other hand, there is robust evidence that tenants in T1 have more intensive use of capital inputs (p-value=0.039). The corresponding effect is insignificant for tenants in T2 (p-value=0.274). And the effect of treatment condition T1 on the intensive margin of capital use is significantly different from the effect of treatment condition T2 at the 5% level (p-value=0.044).

Supplementary Table XXV in the Online Appendix reports bounds that adjust for differential attrition across the treatment groups. The results show that the effects on the intensive margin (of fertilizer and tools) are robust if we impute – within treatment groups – the mean minus (plus) up to 10% of a standard deviation multiple of the observed distribution of outcomes in that treatment group. However, they are not robust if we conduct the imputation with 20% of a standard deviation, or if we trim observations at the top of the distribution to equalize the attrition rates across the groups (i.e. the lower Lee bound). They should be interpreted with this caveat in mind.

Labor Inputs. Tenants reported their own labor hours as well as any outside labor that they may have used on the plot, broken down into paid versus unpaid labor. Table IV reports the results of estimating specification 4 where the outcomes are variables pertaining to labor inputs used on the plot. Column 1 shows that tenants in T1 and T2 did not spend more hours working on their plots relative to tenants in the control group nor relative to each other. Similarly, in column 2, we do not find any significant differences in terms of paid labor across the treatment groups. On the other hand, column 3 shows that tenants in T1 had more "unpaid workers" working on their plots. In particular, they used 8 more days of unpaid labor during the season.[43] Relative to the mean in the control (12.5 days/season) this corresponds to a 64% greater use of unpaid labor on the plot. The difference between T1 and T2 in terms of unpaid labor is also large (approximately 6 days) but statistically not significant at conventional levels (p-value=0.173).

To address concerns related to multiple hypotheses testing, we again follow the

[43] A further breakdown of labor shows that the effect is driven by a combination of family and friends helping with cultivation, results available from the authors upon request.

two approaches discussed above. In the final column of the table we use an aggregate index that combines the three types of labor (own, paid and unpaid). The results show that the effect of T1 on this aggregate index is 0.2 standard deviation but imprecisely estimated at conventional levels (p-value=0.157) and the effect of T2 is 0.05 standard deviations, also imprecisely estimated (p-value=0.721). The difference between the two indices is insignificant (p-value=0.280). The same result is obtained when testing the corresponding cross-equation hypothesis.

Supplementary Table XXVI in the Online Appendix shows that these effects are not likely to be driven by differential attrition – the magnitudes of both the lower and upper bounds under alternative assumptions about the attritors are similar to the unadjusted estimates.

Summary. Figure IV provides a visual summary of the effects on input use. It plots the standardized effect size and the 90% confidence interval around the treatment effects for labor and capital inputs. The solid squares correspond to the effects of T1, while the hollow ones show the effect of T2 relative to control. Overall, the results show that the tenants in T1 have responded to the increase in their output share by increasing their use of inputs – in particular fertilizer, tools and unpaid laborers – while the increase in the income of tenants in T2 had no such impact. These effects are perfectly in line with Prediction 1 of the framework: higher s increases input use, while higher w does not.

IV.D. Effects on Risk-Taking

Prediction 2 says that the increase in s and w or risk exposure may also affect tenants' level of risk-taking. The direction of the effect is in general ambiguous, as it depends on the shape of the tenants' utility function. Only the prediction on background risk is unambiguous, it should decrease risk-taking. Typically it is difficult to test this prediction as often the researcher does not observe the risk associated with different input combinations. We provide three distinct pieces of evidence of changes in risk-taking. Note that we do not quantify the returns to risk-taking. Theoretically, standard asset pricing models suggest that the supply of riskier crops is such that their price and hence return compensates for risk-taking, i.e. is higher for riskier crops. Existing evidence on the risk-return trade-off in crop choice faced by farmers in developing countries is consistent with this (Cole et al., 2017).

Approach 1: Crop Choice. First, in our context the crops the tenant chooses to cultivate are a close proxy of risk-taking. The crops that BRAC offered seeds for and which were frequently cultivated by tenants imply different levels of risk exposure for the tenant. In particular, peanuts, tomatoes and maize are very sensitive to rainfall variation and exhibit high output volatility, while beans are relatively insensitive and exhibit less output volatility.[44] In Supplementary Table XX, we present two different approaches to demonstrate this. In Panel A, we exploit geographical variation among the plots cultivated by the control group of tenants to estimate the effect of rainfall throughout the season on the yield of each crop. In Panel B, we use data from FAOStat on crop yields of countries across time in Sub-Saharan Africa.[45] Both approaches show that maize and peanut yields are particularly sensitive to rainfall, while beans are less sensitive. We cannot use the first approach for tomatoes or potatoes, since no tenant in the control group chose to cultivate these two crops, but the results from the second approach demonstrate that tomatoes are as sensitive to rainfall as peanuts.[46] To the extent that rainfall is a good proxy for aggregate income shocks and that farmers can effectively not insure against it, this implies that the return to maize, peanuts and tomatoes has a

[44]This may not hold in other contexts. The FAO publication *Irrigation and Drainage Paper No. 33* relates yield to water intake using evapotranspiration as a main parameter, rather than rainfall. It reports maize and beans as sensitive to water deficit, while groundnuts are described as tolerant to water deficit. While these findings are different from ours with respect to beans and groundnuts, one should notice that they are not specific to East African cultivars and local crop management practices.

[45]Available from: `http://www.fao.org/faostat/en`.

[46]As an alternative way to quantify the riskiness of these crops, we used the FAOStat data to calculate the coefficients of variation in the yields (output per area cultivated) of maize, beans, peanuts, tomatoes and potatoes. We did so using cross-country variation, as well as time variation within countries, and finally using both cross-country and time variation in the panel data. Supplementary Table XXI shows that the coefficients of variation for maize, peanuts and tomatoes are greater than those for beans.

high risk component, while for beans this is not the case.[47]

In order to test Prediction 2, we show how an increase of s in T1 or of w in T2 affects the tenants' decision to grow certain crops more than others. Table V presents the results of estimating specification (4) for outcomes quantifying the extensive and intensive margin of tenants' crop choice. In Panel A the outcome variables are indicators for whether a given type of crop was on the plot at the time of the crop assessment survey (extensive margin); in Panel B the output variable is the number of plants of the respective crop, irrespective of the plants' yield (intensive margin); and in Panel C the outcome variable is the value of the output of the respective crop, taking into account both the number of plants and the number of crops observed on the plants. The first row of Panel A shows that the tenants in T1 were significantly more likely to have maize and tomatoes on their plots compared to tenants in C. While the coefficients for beans and peanuts are also positive, they are not precisely estimated. When we compare the effect of T1 with T2, we find that the only crop that is significantly more likely to be present on T1 plots compared to T2 plots was tomatoes. Panel B shows that on the intensive margin, tenants in T1 grow more tomatoes, maize and peanuts compared to tenants in C, although the former two effects are not statistically significant at conventional levels. They do not grow any more beans. These results are highly consistent with additional risk-taking amongst tenants in treatment group T1. No such pattern exists for tenants in treatment group T2. Panel C shows that a similar conclusion is drawn when measuring the intensive margin in terms of value of output. Tenants in T1 produced more peanuts as well as tomatoes compared to tenants in C and T2. In particular, their expected output was USD 33 more for peanuts and USD 8 more for tomatoes, and these effects are significantly different from the corresponding effects of T2 (p-values of 0.065 and 0.074 respectively). This set of results is highly

[47] Another dimension of risk that may affect crop choice is uncertainty of prices, as different crops are likely to have different levels of price variability. Tenants who choose to plant crops with greater price variability would be taking more risk. In the absence of time-series data on local prices in our study area, we use the cross-country panel data on crop prices provided by FAOStat to calculate the coefficient of variation of local average crop prices in Sub-Saharan African (SSA) countries. The lower panel of Supplementary Table XXI shows that the average (across SSA countries) coefficient of variation for price of beans is lower than those for maize, peanuts, tomatoes or potatoes. This suggests that beans are a safer alternative also in this respect.

consistent with the interpretation that the increase in s led to greater risk-taking by tenants in T1, by inducing them to increase their cultivation of riskier crops (maize, peanuts and tomatoes) compared the the safer option (beans).[48] [49]

Our theoretical framework predicts that the increase in w in T2 may also influence the tenants' risk-taking. In particular, a safe increase in w (as in T2A) can lead to more or less risk-taking (Prediction 2.ii) while a stochastic increase in w (as in T2B) is likely to reduce risk-taking (Prediction 2.iii). In order to test for these predictions, we estimate the effect of T2A and T2B separately on crop choice. Supplementary Table XXIII in the Online Appendix shows that the tenants in T2A produced more peanuts as opposed to the other crops. This suggests that some tenants in T2A may have increased their risk-taking, in line with Prediction 2. We do not find discernable effects of T2B on crop choice.

Approach 2: Distribution of Output. Second, risk-taking will affect the distribution of output across plots within treatment groups. One way to detect risk-taking from the distribution of output is to note that the coefficient of variation of output does not vary with choices that scale up production by a constant factor, but it does vary with changes in risk-taking. In particular, the theory discussed in Section II suggests a coefficient of variation of $\frac{a\sqrt{\int(\theta-\mathbb{E}_\theta[\theta])^2 d\theta}}{a\mathbb{E}_\theta[\theta]+(1-a)}$, which is independent of $f(x)$ and increasing in a. The coefficient of variation of output across plots takes values of 1.37, 1.57, 1.66, and 1.28 in treatment arms C, T1, T2A, and T2B, respectively. Consistent with the earlier results, this approach suggests additional risk-taking

[48] An alternative explanation could be that tenants in T2 diversify their crop portfolio in order to lower output variability. This would be the case if different crops had negatively correlated expected outputs, then the tenants could lower their risk exposure by intercropping them. Supplementary Table XXII shows that, among the control group, outputs of maize, beans and peanuts are not negatively correlated. If anything, the covariances are positive (imprecisely estimated). Moreover, as we show in the following section, tenants in T2 ended up having higher output variability relative to the control group. As such, a diversification strategy to insure against risks is unlikely to be driving the effects we observe on crop choice.

[49] Supplementary Table XXVII provides attrition bounds for the effects on crop choice. While most bounds are similar to the main estimates, there are a few notable differences. The Lee lower bound for the intensive margin of peanuts is close to zero and imprecisely estimated; while for tomatoes the Lee lower bound for both the intensive and the extensive margins are zero. This is because we have a small sample, and most tenants do not grow any tomatoes and few grow peanuts. Therefore when we trim the observations on top of the distribution for both of these crops, we lose all or almost all of the positive observations.

by farmers in the treatment arm that provides a high output share s (T1) relative to control (C). It also uncovers additional risk-taking when farmers experience additional safe income (T2A) relative to control (C); and less risk-taking when farmers experience additional risky income (T2B) both relative to control (C) and to additional safe income (T2A).

Another way to detect risk-taking from the distribution of output is to estimate quantile treatment effects (QTE). We do this using the following specification:

$$Quant_\tau\left(y_{ict}\right) = \sum_{k=1}^{2} \beta_\tau^k T_{ik} + \phi_\tau \delta_s, \tag{5}$$

where y_{ict} is the output level of tenant i from club c in season t; T_{ik} is an indicator variable equal to 1 if tenant i belonged to a club of treatment group k and 0 otherwise and δ_s are strata fixed effects. One caveat to bear in mind is that, due to the small sample size, we have low power in estimating the treatment effects across the distribution.

Figure V displays the results. The QTE estimates reveal that there is considerable heterogeneity in the effects of incentives on the realized output levels: the effect on the 90th centile of output is 4 times as large as the effect on the 50th centile. Moreover, while we observe no negative effect on output at any centile, the treatment effect at the lower centiles are indistinguishable from zero. These effects are again consistent with additional risk-taking by tenants in T1. On the other hand, the lower panel of Figure V reveals that tenants in the high-income group (T2) do not generate more output than the control group, at any decile.

Supplementary Figure III displays QTEs for the sub-group of tenants who received safe versus risky w (T2A vs. T2B) cash transfers. For the group of tenants with additional safe income (T2A) we observe positive point estimates of the treatment effect in the highest deciles. This is consistent with the idea that tenants in T2A take on more risk, as predicted in part (ii.) of Prediction 2. Receiving additional stochastic income (T2B) seems to have the opposite effect. Again this is consistent with the prediction of part (iii.) of Prediction 2: relative to safe income w, additional stochastic income will induce less risk-taking and might have a negative effect on risk-taking. Note that these quantile treatment effects are estimated imprecisely, given the small sample size.

Both approaches to detect risk-taking from the distribution of output should be interpreted with caution. While the results are consistent with risk-taking, they are also consistent with other explanations. Tenants might differ in their innate abilities, and more able tenants in T1 might respond more strongly to the high-incentive contract (Lazaer, 2000), for example by working harder.[50]

Approach 3: Responsiveness to θ. A third approach to uncover risk-taking is suggested by the theory: one can estimate the responsiveness of output to θ across treatment groups. The coefficient estimate will identify the treatment-group-specific $a \cdot f(x)$. The approach can be operationalized by using weather data to proxy for variation in θ. This allows us to draw inference on risk-taking, a, given information on treatment-group-specific changes in $f(x)$. We explain this approach and how information on treatment-group-specific $f(x)$ can be obtained in detail in Section V.B when discussing the quantitative contributions of input levels and risk-taking to the output effects. As an upshot, this approach also suggests significant additional risk-taking in the treatment arm that provides a high output share s (T1) relative to control (C).

Summary. The collection of evidence in this section shows that tenants with a higher share of output (T1 vs. C) made riskier input choices. Additional safe income w (T2A vs. C) leads to somewhat more risk taking, while additional exposure to uncorrelated background risk (T2B vs. T2A) induces less risk-taking.

IV.E. Effects on Other Outcomes

The results thus far showed that tenants in the high-incentive group (T1) invested more in cultivating their rented plots, took on additional risk and generated more revenue from them. A natural question is whether these are achieved at the ex-

[50] We did not find a significant difference in terms of hours worked by T1 tenants relative to the control group (Section IV.C), but they may have exerted more effort during those hours.

pense of other detrimental effects for them, their households or the plots. In particular, since we observe an increase in unpaid labor, in part driven by family labor, this raises the question of whether the increased labor activity on the plot crowded out other income-generating activities and reduced household earnings. To shed light on this, we estimate the impacts on respondent's and her household's economic wellbeing. Table VI presents the results. The table shows that tenants in T1 did not have lower labor income, consumption, cash savings, household income or assets at the end of the experiment. If anything, column 4 shows that they had higher household income and column 5 shows that they had more households assets (both marginally significant at 10% level) relative to C.[51] These findings imply that the high incentive contract did not crowd out any other productive activities. If anything, the evidence is in line with it increasing household income.[52]

While high tenant incentives may increase output and their households' economic well-being, they may have negative consequences for the environment. In particular, short-term, high-incentive contracts (such as those we study here) may lead the tenant to overwork the land (e.g. by overusing fertilizers) which may lead to environmental degradation. To test for such an effect, at the end of the experiment (i.e. at the end of the second experimental season) we collected soil samples from the plots that were part of the experiment, and tested their chemical composition. In particular, we measured the amount of Nitrogen, Phosphorous, Potassium, Organic matter, and the Ph-level of the sample. Table VII shows the results of estimating the effects of the treatment(s) on these soil quality indicators. We do not find any significant differences in terms of soil quality of the plots in different treatment arms. While this suggests that the high incentive contract did not come at a cost to the soil quality in the short run, it does not rule out long-run negative effects or changes in unobservable dimensions of soil quality.

[51] Findings in Table III showed that tenants in T1 were more likely to invest in tools for their plots. This may generate positive spillover effects on their households' other plots, which may explain the larger effect on their household income relative to their personal labor income.

[52] Supplementary Table XXVIII displays bounds for differential attrition for the effects reported in Table VI.

V. Discussion

In this section we interpret the experimental findings, discuss their welfare implications and make note of possible limitations.

V.A. Understanding Tenants' Choices

The theory in Section II highlights three drivers of the tenants' output and risk-taking choices: incentives, risk exposure, and income. We next revisit these predictions and show that the empirical results are highly consistent with them.

Prediction 1 says that a higher tenant share s leads to higher input levels, x, while income and risk exposure have no such effects. In Section IV.C we show that tenants with a higher output share use more inputs, both capital and labor. Additional income or risk exposure does not lead to substantial changes in input levels.

Prediction 2 says that risk-taking increases with income w under DARA and decreases with risk exposure. An increase in the tenant's share s has both of those effects on risk-taking, in addition to an incentive effect; the sign of the total effect on risk-taking and hence output is theoretically indeterminate. In Section IV.D we show that tenants with a higher output share also take on additional risk. Additional income leads, if anything, to a small increase in risk-taking, whereas background risk discourages risk-taking.

Prediction 3 says that whenever an increase in s induces risk-taking, the aggregate impact on output should be positive; and the effect of income and risk exposure on output has the sign of their effect on risk-taking. In Section IV.B we present output results that are highly consistent with these predictions. We find that an increase in the tenant's share by 25% leads to 60% higher output. Additional income leads, if anything, to a small increase in output; while additional risk exposure leads to a small decrease in output.

Note that the combined income and risk exposure induced by treatment T2B would, in theory, discourage risk-taking less than the combined income and risk exposure induced by treatment T1: treatment T2B induces uncorrelated background risk, while T1 induces additional exposure to risk perfectly correlated with yields; and additional risk in T1 includes the possibility of crop failure which T2B does not allow for and which farmers might be particularly averse to.[53] Therefore the positive effect on risk-taking associated with treatment T1 is likely to be a lower bound on the incentive effect of a higher share s on risk-taking.

Our theory does not allow for income effects resulting from a labor-leisure trade-off (often highlighted in labor economics). Further, the additional income might relax credit constraints (often highlighted in development economics), even though it was to be realized in the future at the time of the agricultural decisions in both T1 and T2. To the extent that such effects are present in the setting under study, our empirical results suggest the sum of these effects is at most small.[54]

V.B. Accounting for Output Effects

Next we discuss whether and under what assumptions the output effects can also be accounted for quantitatively by the observed changes in input use and risk-taking.

Taking logarithms of equation (1) gives:

$$\log y = \log[a(\theta - 1) + 1] + \log f(x). \tag{6}$$

Equation (6) suggests that the change in log-output can be decomposed into the additive effects of changes in risk-taking and changes in inputs.

[53] Both these differences between the risky income induced by T1 and in T2B also might imply relatively better credit access in T2B; again this would imply that the positive effect on risk-taking associated with treatment T1 is likely to be a lower bound on the incentive effect.

[54] In the Online Appendix V.F., we use the tenant survey data to assess whether the contractual changes had any impact on tenants' access to credit. In particular, we examine whether the tenants in T1 or T2 had more outstanding loans (both on the extensive and intensive margins) and whether they believe they would be able to borrow 25.000 UGX and 300.000 UGX, respectively, for 6 months. Results presented in Supplementary Table XXIX show no effect along any of these dimensions, nor on the aggregate index. We also examined whether the treatment(s) had any effects on the sources of borrowing (whether the tenant had borrowed from friends, MFIs or NGOs, cooperatives or moneylenders). Supplementary Table XXX shows that the treatment had no effect on the source of borrowing either. Our interpretation is that additional income realized by the farmers in T1 and T2 does not appear to substantially improve credit access.

Effects via Inputs. First, let us quantify the change in $[\log f(x)]$ resulting from altered input choices we observe. To that end, we assume a parametric form for $f(x)$. In particular, let $x = (k, l, z)$ and $f(x) = \psi k^{\alpha} l^{\beta} z^{\gamma}$, where k denotes capital, l labor, z is land and ψ is the farm TFP. Substituting into equation (6) yields:

$$\log y = \zeta + \alpha \log k + \beta \log l + \gamma \log z \tag{7}$$

where $\zeta := \log[a(\theta - 1) + 1] + \log \psi$. This formulation is consistent with the literature estimating factor shares in agriculture. To assess the contribution of changes in input levels to the output effects, we require estimates of the treatment effects on k, l and z, as well as estimates of the factor shares. Table VIII presents the results of estimating the treatment effects on the log values of total output (y), capital (k), labor hours (l) and size of the plot area cultivated (z).[55] In column (4) we estimate log output to increase by 0.38 log-points for tenants in T1 relative to tenants in control. Columns (1) to (3) show that tenants in T1 increase their investments in k by 0.20, l by 0.10 and z by 0.29 log-points, respectively. Factors shares have been estimated, amongst others, by Valentiyi and Herrendorf (2008) and Restuccia and Santaeulalia-Llopis (2017). Using the results for the U.S. agricultural sector in Valentiyi and Herrendorf (2008) (which are $\alpha = 0.36$, $\beta = 0.46$, $\gamma = 0.18$) implies that the observed changes in input levels explain an increase in output of 0.17 in log-points; using Restuccia and Santaeulalia-Llopis (2017)'s estimates of factor shares in Malawi ($\alpha = 0.19$, $\beta = 0.42$, $\gamma = 0.39$), the predicted output increase is 0.19 log-points. Therefore, the observed changes in input levels explain approximately half of the output effect we observe. This also implies that $\frac{f(x_{T1})}{f(x_C)} = e^{0.19} \approx 1.21$.

[55] Since we do not observe the quality of labor hours (i.e. effort) or land cultivated, our measures are, at best, imperfect proxies for the true input levels. To calculate aggregate capital used, we sum the values of fertilizer, insecticide and households tools. When aggregating the labor hours, we need to combine own labor hours (reported for a typical week during the season) and numbers of days of hired labor used during the season. To do so, we assume that each worker-day corresponds to 8 hours; and each season lasted for 3 months. While the size of the plot allocated to tenants in different treatment arms was identical on average (due to the randomization), the tenants could decide to cultivate any fraction of the plot. The land size variable corresponds to the cultivated area as observed during the crop assessment survey.

Effects via Risk-Taking. Quantifying the contribution of risk-taking to output increases requires information about both the level of risk-taking a in each treatment group, and the returns to risk-taking, $\mathbb{E}[\theta]$. We first discuss how to quantify the relative level of risk-taking across treatment arms. The theory suggests that the slope coefficient of a regression of output y on θ identifies $a \cdot f(x)$. For farmers in developing countries, an important subset of variation in θ is weather risk. Therefore the relative responsiveness of output to weather shocks in T1 relative to C is informative about the ratio $\frac{a_{T1} f(x_{T1})}{a_C f(x_C)}$. We obtain an estimate of this ratio through three steps, the details of which are explained in Online Appendix IV. We first obtain satellite-imagery based rainfall data for each month of the agricultural season and match it to the geolocation of the experimental plots. Second, using data from T2 we find a predictive model of how the multidimensional rainfall data maps into a unidimensional measure of weather conditions, scaling proportionately with output. Applying this model we calculate a measure of weather conditions for plots in C and T1. Third, we estimate how strongly output on plots in treatment arms C and T1, respectively, responds to the measure of weather conditions. Denote the estimated coefficients as $\hat{\rho}_k$, respectively, where k_i indicates that plot i is in treatment arm $k \in \{C, T1\}$. The ratio $\frac{\hat{\rho}_{T1}}{\hat{\rho}_C}$ is then a consistent estimate of $\frac{a_{T1} f(x_{T1})}{a_C f(x_C)}$. For tenants in control C the responsiveness of output to weather conditions is estimated to be 0.614 (p-value $= 0.008$), and in treatment group T1 it is estimated to be 1.393 (p-value $= 0.002$).[56] These point estimates suggest a ratio $\frac{a_{T1} f(x_{T1})}{a_C f(x_C)}$ of 2.27.[57] Above we found that $\frac{f(x_{T1})}{f(x_C)} \approx 1.21$. Together these results imply that $\frac{a_{T1}}{a_C} \approx 1.88$.[58]

Lastly, we need to quantify the returns to risk-taking. We can not quantity these, as our experiment does not allow to estimate the distribution of θ separately from

[56] Reassuringly, this suggests that our measure of weather conditions as constructed in the first and second step is indeed meaningful.

[57] The responsiveness to weather shocks of plots in C and T1 should *not* be compared to the estimated responsiveness to weather shocks in T2, which by construction is 1. The measure of weather conditions is constructed using T2 data, implying that in the second step we likely overfit the predictive model towards output of T2 plots.

[58] An alternative approach is to note that $\frac{SD(y|T1)}{SD(y|C)} = \frac{a_{T1} f(x_{T1})}{a_C f(x_C)}$, where $SD(y|k)$ is the standard deviation of output in treatment arm k. Our results suggest a ratio of standard deviations of 1.71. This approach is simpler, but its results are less straight-forward to interpret. While differential variation in output across treatment arms is consistent with differential risk-taking and input levels, it may also reflect heterogeneity in the tenants' responses to incentives. Nonetheless, it is comforting that the results of those two unrelated approaches are in the same ballpark.

the level of a_C. However, existing evidence suggests that the gross returns to risky agricultural techniques are large; and their adoption rates are low in many developing countries and especially in Africa. For example, Duflo et al. (2008) summarize related literature as finding that fertilizer and hybrid seeds increase yield from 40% to 100%. Both hybrid seeds and the fertilizers studied are risky investments, since they are highly complementary with rainfall. The same authors report adoption rates for fertilizer of 35% to 40% for farmers participating in their study in Kenya. If we take adoption rates as rough measure of a, and further assume that adoption of hybrid seeds and fertilizer is akin to moving from the safest input mix to the most risky input mix, these numbers are informative about the extent to which the additional risk-taking in T1 translates into additional output. At the midpoints of the given ranges we have $a_C = 0.375$ and $\mathbb{E}[\theta - 1] = 0.7$, which suggests that the additional risk-taking of tenants in T1 explains 0.17 log points of the 0.19 log points in output difference unexplained by input choices.[59]

These results suggest the estimated output effects can be almost fully explained by additional input use and risk-taking of tenants, each contributing about half of the total output effect. It should be kept in mind that the exact quantitative decomposition depends on assumptions about functional forms as well as effect sizes. The main take-away of these calculations is not to provide an exact decomposition, but rather that a set of reasonable assumptions exists under which the total output response can be rationalized as being the effect of the input and risk-taking choices we observe.

Moral hazard models are typically phrased in terms of the agent's 'effort'. Effort is then often interpreted as a metaphor for factors that are non-observable and therefore non-verifiable. Such factors that are truly unobservable by the landlord might exist. They will also not be observable to us as researchers, and such factors would contribute to the small unexplained output increase in T1 relative to control. However, taking the decomposition exercise at face-value, a more suitable interpretation of the standard moral hazard model is to think of both input choices and risk-taking as 'effort'. While these factors are in fact partly observable, they

[59]This results is obtained as $\log[(0.375 \times 1.88 \times 0.7 + 1)/(0.375 \times 0.7 + 1)] \approx 0.17$.

are non-verifiable. Contracts are typically not written contingent on these choices, presumably because the informational costs are prohibitively high and such contingent contracts might be particularly difficult to enforce given the state of courts and other enforcement mechanisms. (In the end, at some cost many if not all important dimensions of agricultural practice are observable. But observing them is costly. As researchers, a large fraction of our research budget was spent on conducting high-intensity pre-harvest land measurements, crop assessment surveys and soil tests.)

V.C. Welfare Implications

Tenants with a 75% output share generate 60% higher output than tenants with a 50% output share. Consequently income increases by 140%. However, these tenants are also exposed to a higher variance of income, both mechanically and because they increase their input levels and risk taking. If tenants are risk-averse, this begs the question whether and by how much welfare increases when tenants' are allowed to keep a higher share of outcome.

In order to make any welfare statement, we need to gauge the distribution of income that tenants are facing in T1 and C.[60] We obtain an estimate of the distribution of income over time on each plot. To that end, we use satellite-imagery based data on monthly rainfall in 0.1 degree grid cells for the 16 seasons preceding our experiment as well as the seasons of our experiment, and match it to the geolocation of the experimental plots.[61] Then we use the treatment arm specific estimates of the responsiveness of output to weather conditions to calculate the predicted value of output for every plot i in season t (see Section V.B and Online Appendix IV. for details). To this plot specific vector of agricultural gross income we add the average income obtained from other sources net of costs of inputs by farmers in C and T1, respectively. This procedure yields an estimate of the distribution of income across time t for each plot i, assuming that output reacts to weather in the

[60]Note that the cross-sectional variation in income is not a suitable approximation, since it also reflects unobserved but fixed productivity differences across tenants and space.

[61]In Uganda there are two agricultural seasons per calendar year, and we use rainfall data from 2006 through 2013.

time series the same way as it does in-sample, and that all variation in output is driven by weather shocks.

We then calculate the certainty equivalent of the income stream of T1 for agents with the baseline risk exposure of tenants in C for a range of levels of risk aversion. Figure VI plots the results. There is limited agreement on what level of risk aversion characterizes choices under uncertainty, and estimates of risk aversion yield wildly different results across different methodologies and settings (Rabin, 2000). However, for levels of risk aversion that appear to characterize well choices over larger stakes, such as $\eta \in [1, 2]$, we find substantial welfare gains for tenants who are given a higher share s of output (T1) relative to control (C). Tenants in control, who operate under a 50-50 sharecropping contract, would need to be given 45 to 55 USD (PPP) for sure to be as well off as tenants who are residual claimants on 75% of output.

These large benefits for tenants of providing incentives need to be weighed against a moderate loss for landlords. For landlords the high-incentive contract implies a fall of expected income by 20%, or roughly 10 USD (PPP).

V.D. Limitations

When extrapolating from the findings reported in this paper it is important to keep in mind at least four limitations of our study.

Selected Sample. First, the sample of farmers was not chosen explicitly to be representative of a general population of farmers. On the contrary, being female and of young age were implicit inclusion criteria. We assess how the experimental sample compares to a general population of farmers, by comparing farmer characteristics to the 2013/2014 wave of the Uganda National Panel Survey, a survey that is part of the World Bank's "Living Standards Measurement Study" (LSMS) program and designed to be nationally representative. Supplementary Table III presents summary statistics on key characteristics of farmers in the control group of our experimental sample (column 1) and all tenant farmers in the UNPS sample (column 2).[62] It also reports normalized differences of means between the UNPS sample and the control group of the experimental sample. We adopt the Imbens and Wooldridge (2009) criterion and judge sample differences as large when the normalized difference exceeds 0.25. Relative to the general population of Ugandan tenant farmers (column 2), tenants in our experiment are younger, less likely to be married and have one and a half more years of education. Both of the latter differences are partially the consequence of gender and cohort effects.[63] In terms of their agricultural production though the farmers in the experimental sample are not dissimilar from the general population of Ugandan farmers: The farmers in the experiment have very similar levels of yields even though the plots we rented out are somewhat smaller than typical plots; the experimental sample of farmers use similar levels of tools and similar amounts of total labor, but less fertilizer; and they grow more of the crops for which they were given seeds, maize, beans and peanuts. With the exception of plot size and the value of peanut output, these differences are not substantial in terms of the normalized differences.[64]

An important question is whether the specific characteristics of the experimental sample are related to particular responses to contractual terms. We provide suggestive evidence by exploiting within sample variation in terms of marital status, age, schooling and plot size to gauge whether responses are heterogeneous along these dimensions. Supplementary Tables V through VIII provide results from this exercise. We do not find significant heterogeneity in the response to the experimental treatments along age, marital status or plot size. We do observe that farmers with more schooling respond more strongly to both the incentive treatment as well as to the safe cash transfer. This suggests that the treatment effect of T1 would

[62]The UNPS survey is based on self-reported output measures. Since self-reported and plot based measures of output tend to yield substantially different results Lobell et al. (2018), we compare responses to the UNPS survey to self-reported responses from the experimental sample of farmers. See Online Appendix III. for details.

[63]Differences in marital status and schooling are somewhat smaller when restricting the UNPS sample to female and below-median age farmers ('UNPS - restricted', column 3). The differences may also reflect effects of the ELA program, which is designed to empower young women (Bandiera et al., 2018).

[64]Conclusions are similar when comparing the UNPS sample to the full experimental sample rather than just farmers in the control group, see Supplementary Table IV.

have been lower on a less educated sample of farmers.[65] In terms of magnitudes, the coefficient of the interaction term "T1 $\times$ Schooling (years)" in Supplementary Table VII is 13.7, while the difference in schooling levels between the tenant farmers in our sample and the average tenant farmer in Uganda is 1.7 years (see Supplementary Table III). None of the heterogeneity analysis is highly powered, but taking the coefficient estimates at face value this suggests the treatment effect of T1 (56.3) might be lower by roughly 40% for a farmer with average education level.

Seasonal Effects. Rosenzweig and Udry (2018) recently made the important observation that estimating the average effects of interventions on agricultural output is difficult both for farmers and researchers given variation in weather conditions across seasons.

One important and predictable source of such variation is that the two agricultural seasons observed in the tropics typically have different yield potential. For that reason we ran the experiment both during the long (Season 1) and short rains (Season 2). Supplementary Table XIX presents the main output results of Table II disaggregated by season. In line with expectations the agricultural output in Season 2 is substantially lower than in Season 1, roughly half for the control group. We also find a significantly stronger output response of treatment T1 in the first season: while in Season 1 farmers in treatment group T1 produce 69% more output than farmers in control, in Season 2 the corresponding increase is 27%. Note that any such heterogeneity test has low power. We therefore report the average results throughout the paper, which should be interpreted as weighted average across both seasons (with higher weight on Season 1 given Season 2 attrition).

On top of the variation in the output potential across the two types of agricultural season, there is variation within the same type of season across years. This is particularly important in our setting since risk-taking by farmers in treatment T1 implies even larger than usual variance in output, and results from any given year or season correspond to a particular realization of that risk. In Section V.C

[65] At the same time the effect of T2 is also increasing in the tenant's schooling level, and the differences between the inventive and income effects (i.e. T1 v.s. T2) is if anything larger on a less educated set of farmers.

we describe a structured approach to extrapolate from the experimental setting: we exploit variation in the weather conditions across experimental plots and seasons to estimate the responsiveness of farmers in each treatment group to weather conditions; we then use the distribution of weather conditions across several past seasons to proxy for the distribution of weather conditions faced by farmers; combining these allows to estimate the average output response across plots in a treatment arm in a typical season. This exercise suggests that on average across seasons farmers in treatment group T1 would produce 57% more output than farmers in control.[66] These results do not suggest strong reasons to think the experimental results are specific to the agricultural seasons during which the experiment was conducted.

The same exercise also predicts output of 188.2 USD and 116.4 USD in T1 and C, respectively, in Season 1 and 83.2 USD and 68.7 USD, respectively, in Season 2. Therefore treatment fixed effects and average responsiveness to weather conditions across season in T1 and C – together with realized weather conditions in Season 1 and Season 2 – explain an output increase in T1 relative to C of 61.7% in Season 1 and of 21.0% in Season 2, very close to what we find in Supplementary Table XIX. The differently sized responses across seasons might also reflect that treatment effects decrease with experience, or that output sharing rules have other dynamic effects, or the implementation challenges we describe in Section III.C. We cannot ultimately reject those hypotheses.[67] But these results suggest that the differences in responses might plausibly be driven by the combination of different levels of inputs and risk-taking across treatment groups and the particular weather realizations of Season 1 and 2.

Prevalence of Sharecropping. Third, formal sharecropping contracts are not as common in rural Uganda as in other places, in particular Southeast Asia.[68] To

[66] A less structured approach is to observe that the rainfall patters during the two experimental season were no particular outlier relative to historic rainfall patterns, see Supplementary Figure I). However, this masks that any given plot might have been exposed to an extraordinary rainfall pattern.

[67] We think the dynamic effects of the terms of output sharing rules are an interesting area for future research. Our results cannot contribute to that debate.

[68] However, when tenancy contracts exist, a 50% output sharing rule is very common around the world, see Otsuka et al. (1992) or Banerjee et al. (2002).

the extent that this implies that the tenants are imperfectly aware of the functioning of sharecropping contracts, this would again imply a muted response toward contractual changes relative to a situation where sharecropping contracts are well understood. However, the fact that sharecropping contracts are largely absent in Uganda might also be the consequence of underlying differences between rural Uganda and other areas where sharecropping contracts are more prevalent. If such differences are related to the elasticity of tenant responses towards changes in s – as would for example be the case if the underlying agricultural production function is different – our findings are unlikely to be externally valid.

Externalities. Finally, we find that tenants respond to higher incentive contracts both by acquiring more inputs, and by taking on more risk. To the extent that either of these responses is having externalities, such responses may not be socially optimal. For example, the tenants could be depleting their land of nutrients such that land quality is substantially reduced in the long run. We do not find any such evidence, but we cannot exclude that unmeasured negative effects do exist. Also, tenant choices might have pecuniary externalities on crop prices; if insurance markets are incomplete, the optimal level of private risk-taking might be different from the socially optimal level of risk-taking.

VI. Conclusion

The question of how output sharing rules affect economic agents' incentives for investment and risk-taking is central to development economics, contract theory, and public economics. In the context of agricultural tenancy contracts, the idea that a tenant who has to share a large part of her output with the landowner will have little incentive to invest in cultivation has been long established. Yet, the empirical evidence on this is scant. We find that an increase in the output share from 50% to 75% leads tenants to invest more in inputs, especially capital (fertilizer and tools) and take on more risk. As a result of these changes, they produce 60% more output.

We find the effects of high-incentive output sharing rules on agricultural input choices and output are largely to be interpreted as an incentive effect. Taken at face value, our results suggest that increasing the tenants' income is unlikely to trigger the same type of output response. However, this interpretation ought to be cautioned. The income treatment in this paper promised future income to tenants – to mirror the income effect of the high output share and gauge its size. This should not be compared with policies such as the unconditional cash transfers studied by Haushofer and Shapiro (2016) which might have a stronger effect on relaxing liquidity constraints, inducing changes in labor supply and consumption. Their evaluation also considers cash transfers which were at least an order of magnitude larger than our income treatment.

Moreover, we find that one effect of strengthening the cultivator's position as residual claimant is increased uptake of profitable but risky agricultural techniques. This finding speaks to the large theoretical literature in public finance that studies the effect of taxation on entrepreneurial risk-taking (Domar and Musgrave, 1944; Mossin, 1968; Stiglitz, 1969; Feldstein, 1969). This literature highlights that even the sign of the effect is theoretically indeterminate in the absence of strong assumptions. Our findings suggest that – in our context – output taxation discourages risk-taking.[69]

Our findings are also consistent with the recent work by Karlan, Osei, Osei-Akoto, and Udry (2014) who find that farmers in Ghana make riskier and presumably profitable production choices when provided with insurance. The socially inefficient production choices induced by incomplete insurance markets will best be addressed by effectively providing insurance. However, in the absence of perfectly functioning insurance markets, our results suggest that increasing the tenant's share in output, may also encourage profitable risk-taking, in addition to the incentive effects on input levels.

[69] Recent experimental studies highlight the importance of kinship taxes in the African context. This literature suggests that demands from individuals' social networks to share output may lower individuals' incentives to invest in high-return projects (Jakiela and Ozier, 2016) and lower enterprise growth (Squires, 2017). Studying the interaction of kinship taxes with formal output sharing rules (such as sharecropping contracts) can be valuable for future research.

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Tables

Table I: DESCRIPTIVES CHARACTERISTICS AND BALANCE TESTS

	(1)	(2)	(3)	(4)	(5)
		Difference between			
	C	T1-C	T2-C	T1 - T2	N
Panel A:		*Farmer Characteristics*			
Young (Age $\leq$ 21)	0.557	-0.044	0.027	0.071	262
	(0.500)	[0.552]	[0.721]	[0.313]	
Low Schooling ($\leq$ 8 years)	0.550	-0.028	0.005	0.033	265
	(0.501)	[0.731]	[0.946]	[0.671]	
School enrolment	0.089	-0.010	-0.038	-0.028	264
	(0.286)	[0.823]	[0.317]	[0.519]	
Raven test score (0-100)	51.54	2.88	5.02	2.13	269
	(24.95)	[0.419]	[0.159]	[0.527]	
Health status (0-10)	8.111	0.190	0.044	-0.146	269
	(1.643)	[0.391]	[0.865]	[0.418]	
Married	0.512	-0.004	-0.029	-0.026	268
	(0.503)	[0.962]	[0.718]	[0.761]	
Number of children	1.750	-0.197	-0.026	0.171	268
	(1.710)	[0.426]	[0.906]	[0.484]	
Labor income (USD)	29.3	3.8	-5.7	-9.5	264
	(38.1)	[0.574]	[0.270]	[0.123]	
Cash savings (USD)	122.2	-13.3	-7.9	5.4	266
	(145.5)	[0.523]	[0.725]	[0.826]	
Consumption (USD)	142.6	11.0	-17.3	-28.3	261
	(91.2)	[0.495]	[0.187]	[0.062]*	
Household size	5.346	-0.213	0.010	0.223	269
	(2.001)	[0.488]	[0.970]	[0.431]	
Household sex ratio	0.425	-0.041	-0.002	0.039	269
	(0.208)	[0.174]	[0.957]	[0.212]	
Household income (USD)	194.6	10.8	-3.6	-14.4	235
	(171.9)	[0.666]	[0.872]	[0.550]	
Household assets (USD)	1,506.9	-273.9	-518.4	-244.5	265
	(2,714.3)	[0.480]	[0.135]	[0.409]	
Agricultural tools (USD)	49.12	-6.76	-3.49	3.27	265
	(33.04)	[0.178]	[0.422]	[0.499]	
Panel B:		*Plot Characteristics*			
Distance to Market (km)	2.712	-0.452	-0.347	0.105	270
	(4.629)	[0.329]	[0.477]	[0.756]	
Soil type: loam	0.386	-0.018	0.083	0.101	270
	(0.490)	[0.860]	[0.297]	[0.296]	
Soil type: clay	0.084	-0.003	-0.038	-0.035	270
	(0.280)	[0.937]	[0.427]	[0.388]	
Soil type: sandy	0.108	-0.006	0.000	0.006	270
	(0.313)	[0.897]	[0.990]	[0.875]	
Soil type: rocky	0.048	-0.010	-0.031	-0.021	270
	(0.215)	[0.754]	[0.301]	[0.474]	
Slope: steep	0.084	-0.001	0.014	0.015	270
	(0.280)	[0.983]	[0.752]	[0.642]	
Slope: gentle	0.602	0.016	-0.037	-0.054	270
	(0.492)	[0.816]	[0.550]	[0.390]	
Slope: valley	0.072	-0.041	0.022	0.064	270
	(0.261)	[0.352]	[0.649]	[0.137]	
Slope: flat	0.241	0.026	0.001	-0.025	270
	(0.430)	[0.667]	[0.992]	[0.674]	
Latitude (degrees N)	0.361	0.014	0.012	-0.003	270
	(0.625)	[0.026]**	[0.061]*	[0.692]	
Longitude (degrees E)	32.168	0.000	-0.012	-0.012	270
	(1.403)	[0.980]	[0.100]	[0.056]*	
Total rainfall (dm)	5.096	0.040	0.020	-0.020	479
	(1.670)	[0.427]	[0.729]	[0.743]	

Notes: Column 1 shows the mean (and standard deviation in brackets) of each baseline characteristics in the control group. Columns 2 through 4 show differences in characteristics assigned to treatment and control groups. These are calculated from a regression a regression of the characteristic on dummy variables for treatment status, controlling for strata fixed effects. In square brackets we provide the randomization inference p-value of a test of the null hypothesis that C, $T1-C$, $T2-C$ and $T1-T2$ is equal to 0, respectively. Panel A presents tenant characteristics measured before the start of Season 1. All monetary values are in PPP USD. Panel B presents plot level characteristics which are unaffected by treatment, measured at the end of Season 1. Detailed variable definitions are provided in Section .

Table II: EFFECTS ON OUTPUT

	Output, y		Yield, y/m^2	
	(1)	(2)	(3)	(4)
High s (T1)	56.28***	56.07***	0.074**	0.073**
	(18.52)	(18.58)	(0.031)	(0.031)
	[0.004]	[0.004]	[0.024]	[0.027]
High w (T2)	5.36		-0.000	
	(17.17)		(0.030)	
	[0.765]		[0.995]	
High w, safe (T2A)		18.29		0.043
		(25.84)		(0.048)
		[0.543]		[0.403]
High w, risky (T2B)		-7.25		-0.043
		(15.82)		(0.032)
		[0.641]		[0.206]
H_0: T1 = T2	0.023		0.046	
H_0: T1 = T2A		0.218		0.590
H_0: T1 = T2B		0.001		0.002
H_0: T2A = T2B		0.343		0.120
Mean Outcome (C)	95.13	95.13	0.174	0.174
Observations	473	473	473	473

Notes: The table reports ordinary least square estimates based on specification (4) at the plot level, for both Season 1 and Season 2. *Output, y* is the expected output of the plot measured through the pre-harvest crop assessment survey. It is calculated by multiplying the expected quantity of output of each crop with the price of the relevant crop measured on local markets, and summing over crops. *Yield,* y/m^2 is the expected output of the plot divided by the area (in square meters) cultivated. Values are in PPP USD. T1 is a dummy variable equal to 1 if the tenant/plot was randomized to receive high (75%) output share, T2 is a dummy variable equal to 1 if the tenant/plot was randomized to receive same output share as control (50%) and an additional cash transfer. T2A and T2B indicate subgroups of treatment group 2 (T2). T2A received a fixed income transfer, and T2B received a stochastic income transfer, with mean equal to T2A. All specifications control for strata fixed effects. Standard errors are clustered at the village level and given in round brackets. In square brackets randomization inference p-values of the null hypothesis of no effect are provided; *** (**) (*) indicates significance of that test at the 1% (5%) (10%) level. Additionally randomization inference p-values for the specified compound hypotheses are reported.

Table III: EFFECTS ON CAPITAL INPUTS

	Fertilizer	Insecticide	Tools	Index
	(1)	(2)	(3)	(4)
Panel A:		*Extensive Margin*		
High s (T1)	0.094 (0.061) [0.176]	-0.010 (0.053) [0.860]	0.086 (0.055) [0.123]	0.201 (0.133) [0.162]
High y (T2)	0.027 (0.060) [0.690]	-0.064 (0.055) [0.261]	0.007 (0.053) [0.901]	-0.049 (0.140) [0.739]
Within-Equation Test				
H_0: T1 = T2	0.310	0.320	0.142	0.080
Cross-Equations Test				
H_0: T1 = 0		0.283		-
H_0: T2 = 0		0.594		-
H_0: T1 = T2		0.375		-
Mean Outcome (C)	0.277	0.276	0.500	0.000
Observations	432	423	432	423
Panel B:		*Intensive Margin (USD)*		
High s (T1)	1.13* (0.55) [0.056]	0.43 (0.51) [0.416]	11.36** (5.04) [0.039]	0.436*** (0.153) [0.008]
High y (T2)	0.59 (0.43) [0.205]	-0.50 (0.47) [0.282]	1.59 (4.32) [0.727]	0.029 (0.126) [0.808]
Within-Equation Test				
H_0: T1 = T2	0.350	0.046	0.059	0.008
Cross-Equations Test				
H_0: T1 = 0		0.039		-
H_0: T2 = 0		0.274		-
H_0: T1 = T2		0.044		-
Mean Outcome (C)	0.96	1.81	37.81	0.000
Observations	419	413	427	402

Notes: The table reports ordinary least square estimates based on specification (4). T1 is a dummy variable equal to 1 if the tenant/plot was randomized to receive high (75%) output share, T2 is a dummy variable equal to 1 if the tenant/plot was randomized to receive same output share as control (50%) and an additional cash transfer. All specifications control for strata fixed effects. Standard errors are clustered at the village level and given in round brackets. In square brackets randomization inference p-values of the null hypothesis of no effect are provided; *** (**) (*) indicates significance of that test at the 1% (5%) (10%) level. Cross-Equations Tests report the randomization inference p-value for a test of the hypothesis that the specified restriction holds in all estimating equations across columns. Within-Equation Tests report the randomization inference p-value for a test of the specified compound hypothesis. In Panel A, "Fertilizer (Insecticide)" is a dummy variable equal to 1 if the tenant said she used fertilizer (insecticide) on her plot during the past season; "Tools" is a dummy variable equal to 1 if the tenant said she bought agricultural tools to cultivate her plot. In Panel B, the dependent variable is the monetary value of the input used in PPP USD terms. For agricultural tools, the intensive margin is the value of agricultural tools owned by the respondent's household at the time of the survey. The "Index" combines the four indicators by first standardizing each outcome into a z-score (by subtracting the control group mean at the corresponding survey round and dividing by the control group standard deviation), then takes the average of the z-scores, and again standardizes to the control group.

Table IV: Effects on Labor Inputs

	Own labor	Paid	Unpaid	Index
	(hours/week)	(days/season)		
	(1)	(2)	(3)	(4)
High s (T1)	0.34	-0.05	8.02*	0.20
	(1.28)	(1.98)	(4.03)	(0.12)
	[0.781]	[0.982]	[0.065]	[0.157]
High y (T2)	-0.03	1.06	1.79	0.05
	(1.22)	(2.08)	(3.31)	(0.12)
	[0.984]	[0.628]	[0.626]	[0.721]
Within-Equation Test				
H_0: T1 = T2	0.783	0.550	0.173	0.280
Cross-Equations Test				
H_0: T1 = 0		0.277		-
H_0: T2 = 0		0.909		-
H_0: T1 = T2		0.575		-
Mean Outcome (C)	17.13	4.28	12.54	-0.00
Observations	417	432	432	417

Notes: The table reports ordinary least square estimates based on specification (4). T1 is a dummy variable equal to 1 if the tenant/plot was randomized to receive high (75%) output share, T2 is a dummy variable equal to 1 if the tenant/plot was randomized to receive same output share as control (50%) and an additional cash transfer. All specifications control for strata fixed effects. Standard errors are clustered at the village level and given in round brackets. In square brackets randomization inference p-values of the null hypothesis of no effect are provided; *** (**) (*) indicates significance of that test at the 1% (5%) (10%) level. Cross-Equations Tests report the randomization inference p-value for a test of the hypothesis that the specified restriction holds in all estimating equations across columns. Within-Equation Tests report the randomization inference p-value for a test of the specified compound hypothesis. "Own labor" is the number of hours that the tenant said she worked on the plot in a typical week during the past season. The dependent variables in columns 2 and 3 are the number of worker-days of paid and unpaid labor respectively that the tenant said she had working on the plot for throughout the season. The "Index" combines the three indicators by first standardizing each outcome into a z-score (by subtracting the control group mean at the corresponding survey round and dividing by the control group standard deviation), then takes the average of the z-scores, and again standardizes to the control group.

Table V: EFFECTS ON CROP CHOICE

	Maize (1)	Beans (2)	Peanuts (3)	Tomatoes (4)	Potatoes (5)
Panel A:	*Extensive Margin*				
High *s* (T1)	0.112**	0.049	0.055	0.021***	0.012
	(0.047)	(0.042)	(0.040)	(0.010)	(0.008)
	[0.025]	[0.253]	[0.212]	[0.008]	[0.201]
High *w* (T2)	0.090*	0.032	0.049	-0.001	0.002
	(0.048)	(0.041)	(0.038)	(0.004)	(0.003)
	[0.084]	[0.447]	[0.239]	[0.805]	[0.686]
H_0: T1 = T2	0.652	0.720	0.899	0.013	0.217
Mean Outcome (C)	0.620	0.300	0.327	0.000	0.000
Observations	479	479	479	479	479
Panel B:	*Intensive Margin: Number of Plants*				
High *s* (T1)	159.82	4.53	330.43	41.02**	3.40
	(145.70)	(391.33)	(179.11)	(19.14)	(2.85)
	[0.295]	[0.994]	[0.128]	[0.020]	[0.318]
High *w* (T2)	-66.01	-85.58	-39.70	1.48	0.67
	(131.88)	(362.02)	(154.24)	(10.48)	(1.31)
	[0.635]	[0.841]	[0.818]	[0.912]	[0.841]
H_0: T1 = T2	0.147	0.760	0.094	0.013	0.205
Mean Outcome (C)	861.96	867.83	577.09	0.00	0.00
Observations	479	479	479	479	479
Panel C:	*Intensive Margin: Value of Output*				
High *s* (T1)	4.51	5.40	32.77***	7.67*	0.27
	(4.85)	(6.17)	(11.04)	(4.23)	(0.24)
	[0.384]	[0.389]	[0.003]	[0.051]	[0.447]
High *w* (T2)	-2.43	1.78	4.72	-0.25	0.05
	(4.40)	(6.84)	(9.38)	(1.89)	(0.11)
	[0.591]	[0.820]	[0.655]	[0.917]	[0.814]
H_0: T1 = T2	0.152	0.613	0.065	0.074	0.318
Mean Outcome (C)	28.43	15.78	22.44	0.00	0.00
Observations	479	479	479	479	479

Notes: The table reports ordinary least square estimates based on specification (4). T1 is a dummy variable equal to 1 if the tenant/plot was randomized to receive high (75%) output share, T2 is a dummy variable equal to 1 if the tenant/plot was randomized to receive same output share as control (50%) and an additional cash transfer. All specifications control for strata fixed effects. Standard errors are clustered at the village level and given in round brackets. In square brackets randomization inference p-values of the null hypothesis of no effect are provided; *** (**) (*) indicates significance of that test at the 1% (5%) (10%) level. Additionally the randomization inference p-value of a test of the null hypothesis that the effect of T1 and T2 are equal is provided for all estimating equations. Dependent variables in Panel A are dummy variables equal to 1 if at the time of the pre-harvest crop assessment survey, any harvestable plants of the specified crop were observed on the plot: maize in column (1), beans in column (2), peanuts in column (3), tomatoes in column (4), and potatoes in column (5). In Panel B, the dependent variable is the number of plants of the relevant crop; and in Panel C, the dependent variable is the output value from the relevant crop on the plot – calculated by multiplying the quantity of output of each crop with the price of the relevant crop measured on local markets. All monetary values are in PPP USD.

Table VI: Socio-economic Status

	Labor income (1)	Consumpt. (2)	Cash savings (3)	Household income (4)	Household assets (5)
High s (T1)	4.07	4.43	56.83	33.04*	656.54*
	(7.33)	(9.60)	(35.39)	(18.34)	(332.13)
	[0.626]	[0.678]	[0.127]	[0.076]	[0.060]
High w (T2)	14.98*	-3.98	66.12	0.49	183.46
	(8.35)	(7.84)	(39.27)	(18.04)	(209.29)
	[0.086]	[0.652]	[0.102]	[0.982]	[0.396]
H_0: T1 = T2	0.214	0.372	0.852	0.064	0.164
Mean Outcome (C)	36.65	115.34	143.63	181.80	1242.61
Observations	424	421	427	398	427

Notes: The table reports ordinary least square estimates based on specification (4). T1 is a dummy variable equal to 1 if the tenant/plot was randomized to receive high (75%) output share, T2 is a dummy variable equal to 1 if the tenant/plot was randomized to receive same output share as control (50%) and an additional cash transfer. All specifications control for strata fixed effects. Standard errors are clustered at the village level and given in round brackets. In square brackets randomization inference p-values of the null hypothesis of no effect are provided; *** (**) (*) indicates significance of that test at the 1% (5%) (10%) level. Additionally the randomization inference p-value of a test of the null hypothesis that the effect of T1 and T2 are equal is provided for all estimating equations. All monetary values are in PPP USD terms. "Labor income" is the average monthly labor income of the respondent during the 12 months preceding the survey. "Consumption" is the monthly consumption expenditure of the respondent; it is calculated by adding her monthly personal consumption on non-food items and services with her household's per-capita food consumption where monthly food consumption is imputed from previous 2 days' recall. "Cash savings" is the value of savings that the respondent had at the time of the survey. "Household income" is the response to the question "What is the total income of your household in a typical month?". "Household assets" is the value of durable assets owned by the respondent's household.

Table VII: SOIL QUALITY AT THE END OF THE EXPERIMENT

	N	K	P	Org. M.	Ph	Index
	(1)	(2)	(3)	(4)	(5)	(6)
High s (T1)	-0.11	-0.00	0.06	-0.06	0.05	-0.04
	(0.08)	(0.05)	(0.11)	(0.09)	(0.12)	(0.13)
	[0.216]	[0.975]	[0.598]	[0.515]	[0.685]	[0.793]
High w (T2)	-0.00	-0.02	0.10	0.01	-0.01	0.07
	(0.08)	(0.05)	(0.11)	(0.09)	(0.12)	(0.12)
	[0.993]	[0.711]	[0.369]	[0.912]	[0.917]	[0.574]
Within-Equation Test						
H_0: T1 = T2	0.185	0.760	0.779	0.476	0.592	0.441
Cross-Equations Test						
H_0: T1 = 0			0.711			-
H_0: T2 = 0			0.959			-
H_0: T1 = T2			0.797			-
Mean Outcome (C)	2.29	0.77	2.33	2.11	5.21	-0.00
Observations	324	322	323	321	324	318

Notes: The table reports ordinary least square estimates based on specification (4). T1 is a dummy variable equal to 1 if the tenant/plot was randomized to receive high (75%) output share, T2 is a dummy variable equal to 1 if the tenant/plot was randomized to receive same output share as control (50%) and an additional cash transfer. All specifications control for strata fixed effects. Standard errors are clustered at the village level and given in round brackets. In square brackets randomization inference p-values of the null hypothesis of no effect are provided; *** (**) (*) indicates significance of that test at the 1% (5%) (10%) level. Cross-Equations Tests report the randomization inference p-value for a test of the hypothesis that the specified restriction holds in all estimating equations across columns. Within-Equation Tests report the randomization inference p-value for a test of the specified compound hypothesis. The dependent variables are the results of soil tests conducted on sampled of soil taken from the plots that were part of the experiment. For Nitrogen (N) the index is: 1=lack, 2=inadequate, 3=adequate; for Potassium (K): 0=deficient, 1=sufficient; for Organic Matter: 1=low, 2=high, 3=very high; for Phosphorous (P): 1=very low, 2=moderate, 3=adequate, 4=high. The Ph-level variable reports the ph level of the soil sample. The "Index" combines the five indicators by first standardizing each outcome into a z-score (by subtracting the control group mean at the corresponding survey round and dividing by the control group standard deviation), then takes the average of the z-scores, and again standardizes to the control group.

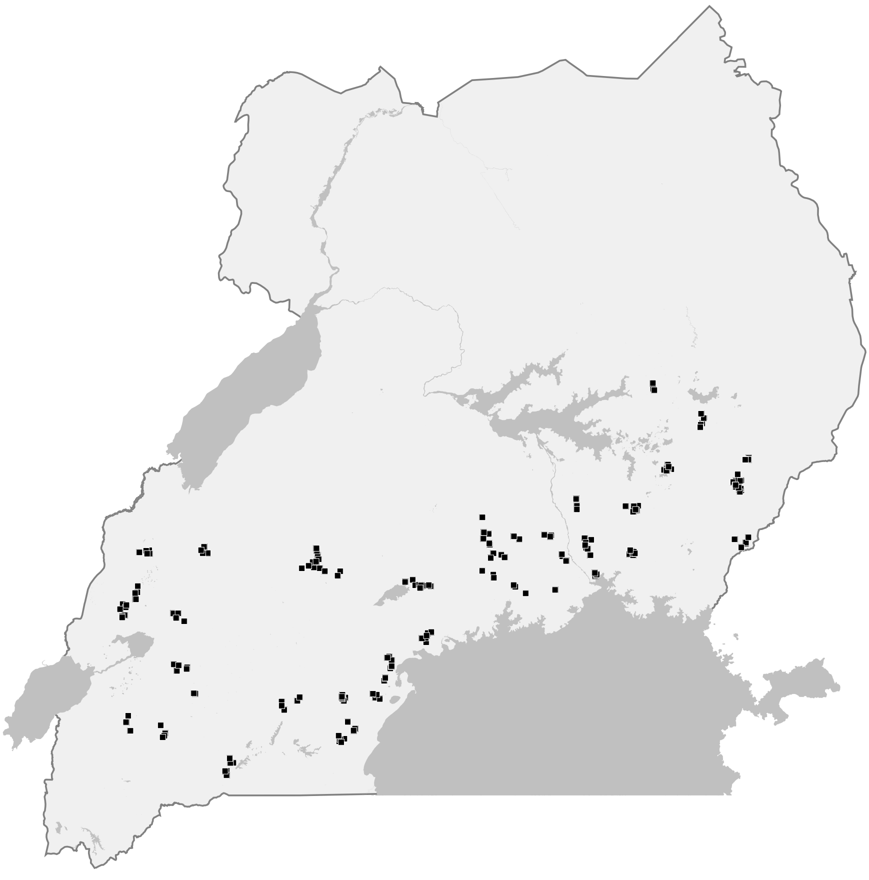
Table VIII: EFFECTS ON INPUT AND OUTPUT VALUES

	Capital	Labor hours	Land size	Output
	(1)	(2)	(3)	(4)
Panel A:		*In Levels*		
High *s* (T1)	12.42**	72.94*	71.37	56.28***
	(5.08)	(38.34)	(59.95)	(18.52)
	[0.027]	[0.086]	[0.277]	[0.004]
High *w* (T2)	2.18	14.91	31.17	5.36
	(4.29)	(34.32)	(57.09)	(17.17)
	[0.646]	[0.686]	[0.639]	[0.765]
H_0: T1 = T2	0.048	0.167	0.481	0.023
Mean Outcome (C)	39.90	338.68	607.13	95.13
Observations	432	417	473	473
Panel B:		*In Logs*		
High *s* (T1)	0.20	0.10	0.29**	0.38**
	(0.12)	(0.10)	(0.13)	(0.17)
	[0.123]	[0.320]	[0.040]	[0.039]
High *w* (T2)	0.04	0.01	0.13	0.11
	(0.12)	(0.09)	(0.14)	(0.16)
	[0.751]	[0.944]	[0.395]	[0.536]
H_0: T1 = T2	0.204	0.365	0.199	0.122
Mean Outcome (C)	3.40	5.53	5.81	3.53
Observations	432	417	473	473

Notes: T1 is a dummy variable equal to 1 if the tenant/plot was randomized to receive high (75%) output share, T2 is a dummy variable equal to 1 if the tenant/plot was randomized to receive same output share as control (50%) and an additional cash transfer. All specifications control for strata fixed effects. Standard errors are clustered at the village level and given in round brackets. "Capital" is the monetary value (in PPP USD terms) of capital inputs used on the plot, obtained by summing up the values of fertilizer, insecticide and households tools. "Labor hours" is the total hours of labor used on the plot during each season, obtained by summing respondent's labor hours (hours worked in typical week during the season multiplied by 12 weeks/season) and hours of hired labor (numbers of days of hired labor used during the season multiplied by 8 hours/day). "Land size" is the size (in m^2) of the plot area cultivated by the tenant. "Output" is the monetary value of total output (in PPP USD terms) of the plot measured through the pre-harvest crop assessment survey (see notes to Table II for further details on this variable). In Panel B, all dependent variables are the natural logarithm of the value of the relevant variable.

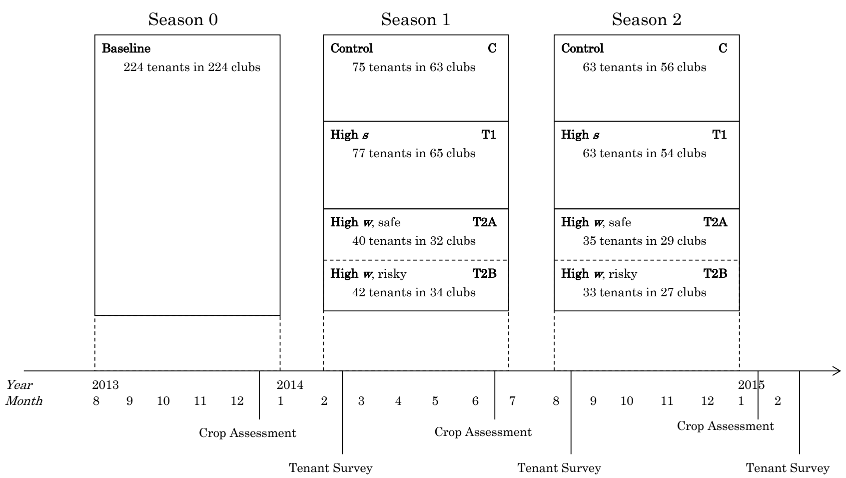
Figures

Figure I: Location of the Plots



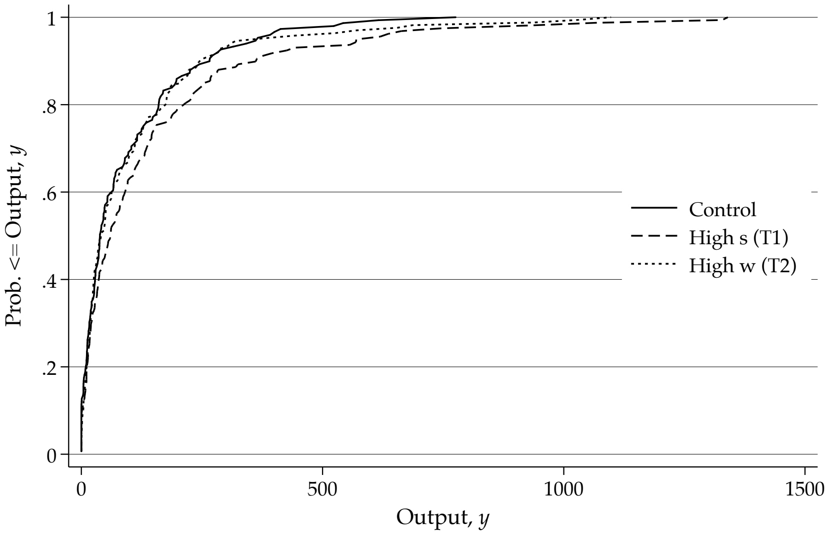
Notes: This map of Uganda shows as black squares the location of BRAC clubs whose farmers participated in the experiment and are covered in the plot-level analysis.

Figure II: EXPERIMENT SETUP



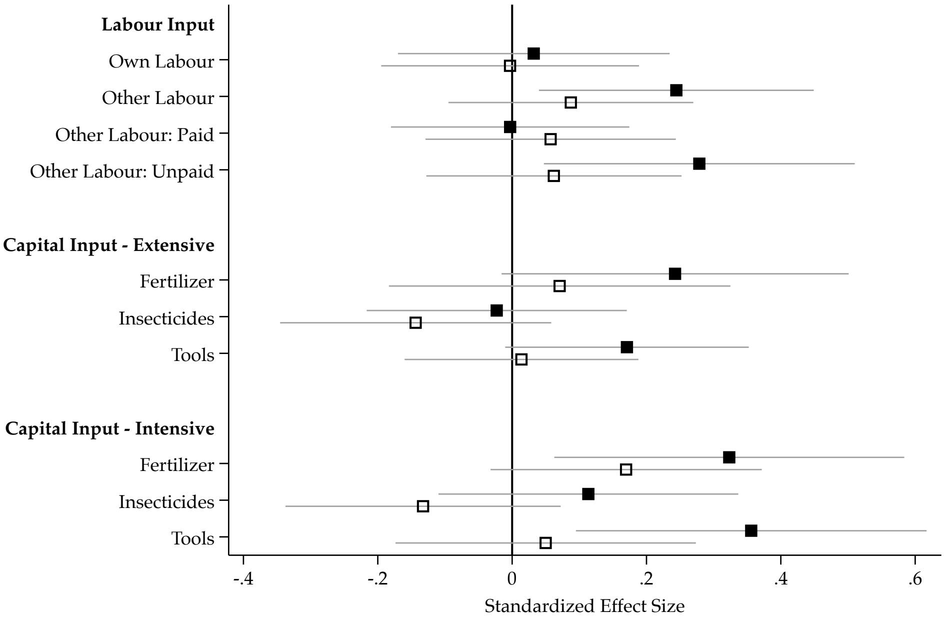
Notes: The figure shows the number of farmers and clubs included in the analysis in seasons 0, 1 and 2 by treatment assignment.

Figure III: CONTRACTS AND DISTRIBUTION OF *Output, y*



Notes: The figure plots the cumulative distribution function of expected output from the plots, by treatment status. Tenants in T1 are those who were randomized to receive high (75%) output share, tenants in T2 received the same output share as control C (50%) and an additional cash transfer. *Output, y* is the expected output of the plot measured through the pre-harvest crop assessment survey. It is calculated by multiplying the expected quantity of output of each crop with the price of the relevant crop measured on local markets, and summing over crops. Values are in PPP USD.

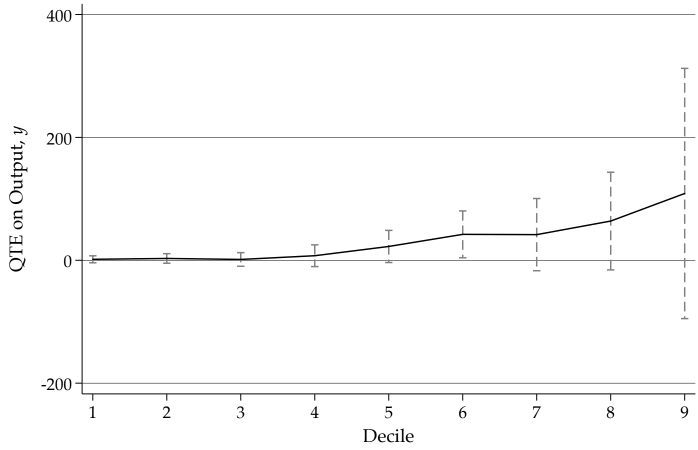
Figure IV: CONTRACTS AND INPUT CHOICE



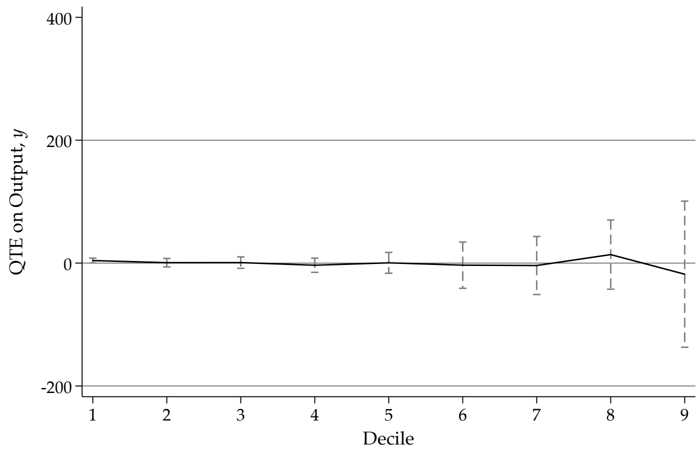
Notes: The figure plots the standardized effect sizes and 95% confidence intervals for labor and capital inputs used for cultivation. The solid squares show the effects of being selected to receive high (75%) output share (T1) relative to the control group, while the hollow squares show the effect of receiving the same output share as the control group (50%) plus an additional cash transfer (T2). The effects are estimated using ordinary least square estimates based on specification (4). All specifications control for strata fixed effects and standard errors are clustered at the village level. For capital inputs, the extensive margins correspond to dummy variables equal to 1 if the tenant used any fertilizer; any insecticide; if she bought any agricultural tools to cultivate her plot. The intensive margins are the monetary value (in PPP USD) of the inputs used on the plot. For tools, the intensive margin gives the value of agricultural tools that the tenant had at the time of the survey.

Figure V: Heterogeneity of Output Effects

(a) Quantile Treatment Effects of T1 vs. C

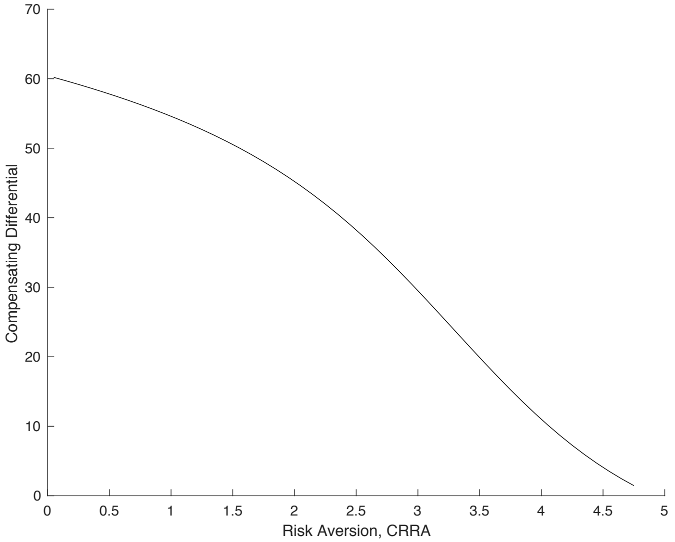


(b) Quantile Treatment Effects of T2 vs. C



Notes: The figure plots quantile treatment effect (QTE) estimates for *Output, y* and 90% confidence intervals based on bootstrapped (with 500 replications) standard errors clustered at the village level (unit of randomization). Each specification controls for the randomization strata. *Output, y* is the expected output of the plot measured through the pre-harvest crop assessment survey. It is calculated by multiplying the expected quantity of output of each crop with the price of the relevant crop measured on local markets, and summing over crops. Values are in PPP USD.

Figure VI: WELFARE – COMPENSATING DIFFERENTIAL C VS. T1



Notes: The graph shows the fixed amount (in PPP USD) that a tenants in control (C) would need to receive per season to be as well-off as tenants in the high share s treatment (T1), for a range of risk aversion levels. We calculate a distribution of potential income levels for each experimental plot in C and T1 (see Section V.C for details). We assume preferences are characterized by the iso-elastic utility function $u(c) = (c^{1-\eta} - 1)/(1-\eta)$, where η is the constant coefficient of relative risk aversion, CRRA, shown on the x-axis above. Given any η, we find the certainty equivalent as the amount e of income such that tenants are, on average, indifferent between the income stream of tenants in C plus e, and the income stream in T1.

Sammanfattning

Den här avhandlingen består av tre kapitel med två gemensamma inslag. Det första är den allmänna forskningsfråga som de försöker besvara: vad driver investeringsbeslut? Varje kapitel utforskar en faktor (olika i varje kapitel) som påverkar investeringsbeslut: externaliteter, begräsningar och avtalsöverenskommelser. Det andra inslaget är den metod jag använder: randomiserade fältexperiment. Randomiserade fältexperiment är ett användbart verktyg för att skapa kausalitet i komplicerade sammanhang med många icke observerbara faktorer när de är noggrant utformade och minutiöst implementerade. I kapitel 1 som har titeln *Kvantifiering av externaliteter vid införandet av teknik: experimentella bevis från bönder i Uganda* (Quantifying Externalities in Technology Adoption: Experimental Evidence from Ugandan Farmers) undersöker jag hur positiva externaliteter påverkar införandet av teknik i jordbruket. Positiva externaliteter inträffar när agenterna inte internaliserar nyttan av sina handlingar för andra, vilket leder till att man antar en jämvikt som är lägre än vad som är socialt optimalt. Kapitlet binder samman punkterna mellan positiva externaliteter och det låga införandet av teknik i Afrika söder om Sahara genom att visa att bönderna är medvetna om att de drar nytta av att andra inför tekniken genom positiva externaliteter. Resultaten gör det möjligt för mig att karakterisera den ideala politik som utnyttjar externaliteter för att förbättra införandet av teknik. I experiment får bönderna möjlighet att köpa en teknik för att få kontroll över skadeinsekter i form av en jordbruksutbildning om skadeinsekter som angriper majs. Bönderna kan antingen köpa utbildningen till sig själva eller till en annan specifik bonde i sin by. Jag mäter böndernas vilja att betala för att köpa tekniken till sig själva eller till en annan bonde; en bondes vilja att betala för en annan bonde är ett monetärt mått på hur mycket en bonde förväntar sig att tjäna på att den andra bonden har tekniken. För att förstå huruvida nyttan beror på positiva externaliteter så genererar jag en slumpvis variation i två slags externaliteter som bonden skulle dra nytta av om den andre erhåller tekniken och mäter hur mycket det påverkar dennes vilja att betala för en annan bonde. När en bonde inför en teknik för att få kontroll över skadeinsekter, drar andra nytta av två slags externaliteter: smitto- och kunskapsexternaliteter. Smittoexternaliteter inträffar när en bonde minskar bördan av skadeinsekter i sin omgivning och andra drar nytta av en mindre risk för skadeinsektsangrepp. Kunskapsexternaliteter sker när andra får kunskap om tekniken genom att prata med eller observera den som antagit den. För det första finner jag att en teknik för att få kontroll över skadeinsekter är önskvärd, eftersom bönderna är villiga att betala för att få utbildningen. För det andra är bönderna villiga att betala för att andra ska få utbildningen; detta innebär att de förväntar sig positiva bieffekter av att andra har infört detta. För det tredje så finner jag att bönder ger gensvar på smittoexternaliteter. När de positiva smittoexternaliteterna från en annan specifik bonde är lägre, minskar bönderna sin vilja att betala för den andra bonden. Mitt experiment ger inte några avgörande bevis vad gäller kunskapsexternaliteterna, men jag ger bevis som tyder på att bönderna lägger större värde vid att utbilda bönder som det är mer sannolikt att de kommer att lära sig av. För det fjärde använder jag värderingsdata och en enkel modell för att beräkna den privata och sociala nyttan av att införa tekniken. Jag finner att den sociala nyttan är avsevärt större än den privata nyttan, men den sociala nyttan av införandet är heterogen mellan bönderna. Sålunda är den optimala politiken nyanserad. På grund av stora externaliteter så är det optimalt att utbilda vissa bönder; eftersom externaliteterna är heterogena och subventionerna är dyra så bör beslutsfattarna rikta sig mot de bönder som skapar de största positiva externaliteterna. De optimala måltavlorna är bönder som är centrala ur ett socialt perspektiv och vars mark ligger geografiskt centralt.

I det andra kapitlet, med titeln *Ett mångfacetterat utbildningprogram för de fattiga och begåvade* (A Multifaceted Education Program for the Poor and Talented), studerar jag MasterCard Foundation Scholars programmet, ett stort stipendieprogram för barn som börjar högstadiet för att sedan gå på gymnasiet i Uganda. Det mest effektiva sättet att allokera utbildningsresurser är att rikta in sig mot elever som är väldigt fattiga och mycket begåvade. Fattigare hushåll kan emellertid ha flera begränsningar när det gäller att investera i utbildning som behöver hanteras samtidigt. MasterCard Foundation Scholars Program förser begåvade fattiga studenter med de resurser som krävs för att gå på högstadiet och sedan gymnasiet; det sammanlänkar en effektiv programinriktning i syfte att undanröja alla begränsningar för investeringar i utbildning samtidigt. MasterCard Foundation Scholars programmet är innovativt ur ett programutformningsperspektiv eftersom det innefattar utbildningskomponenter och interventioner mot fattigdom som inte har implementerats som en helhet tidigare inom samma program, men som enskilt har visat sig vara effektiva. Jag utforskar den slumpvisa fördelningen av lämpliga studenter till en behandlings- eller kontrollgrupp i urvalsprocessen av mottagare för att studera effekterna av programmet. Jag fokuserar på de kortsiktiga (tvååriga) effekterna på skolutfallet för mottagarna och det socioekonomiska välbefinnandet i deras hushåll. Jag finner att MasterCard Foundation Scholars programmet ökar såväl mottagarnas inskrivning i programmet och deras provresultat. Effekten på provresultaten är större än i de flesta andra liknande program. Programmet har effekter på de ekonomiska omständigheterna för andra familjemedlemmar än den direkta mottagaren. Två år efter inskrivningen i programmet så ackumulerar hushållen produktiva tillgångar så som mark, varaktiga konsumtionsvaror, små djur och besparingar i form av pengar. Näringsintaget förbättras i form av en varierad kost, antalet måltider och proteinintaget. Respondenternas mentala hälsa förbättras mätt som minskad depression och ökande livstillfredställelse och lycka. Dessa effekter är i storlek jämförbara med ovillkorade kontantöverföringar eller program för att hjälpa folk ur fattigdom som implementerats i utvecklingsländer. Mina rön finner att en intervention som kombinerar utbildning och åtgärder mot fattigdom fungerar på ett liknande sätt som interventioner som fokuserar på att antingen minska fattigdomen eller förbättra utbildningen. Detta tyder på att dessa två policymål inte konkurrerar med varandra utan kompletterar varandra. I det tredje kapitlet, med titeln *Moral Hazard: Experimentella bevis från arrendekontrakt* (Moral Hazard: Experimental Evidence from Tenancy Contracts), samförfattat med Konrad Burchardi, Selim Gulesci och Munshi Sulaiman, studerar vi effekten av s k sharecropping-kontrakt (kontrakt där arrendatorn betalar en del av skörden i arrende) på valet av insatsvaror när det gäller risktagande och produktion i jordbruket. Sharecropping-kontrakt kräver att en arrendator betalar en del av sin produktion till markägaren. Ekonomer har länge ansett att sådana kontrakt leder till ett ineffektivt beteende från aktörernas sida eftersom aktören inte har rätt till all produktion. Det är en utmaning att undersöka detta påstående med observerbara data. För det första eftersom icke-observerbara egenskaper kan påverka såväl valet av kontrakt som böndernas investeringsval. För det andra eftersom en större andel arrendatorer skapar en incitamentseffekt, en inkomsteffekt och en högre riskexponering, vilka är svåra att särskilja i ett icke-experimentellt sammanhang. Vi utför ett experiment som skapar variation i verkliga arrendekontrakt. Vår implementeringspartner agerar som markägare i utvalda byar i Uganda och erbjuder bönderna ett 50% sharecropping-kontrakt. Efter att kontraktet undertecknats så delades byarna slumpartat in i tre grupper. I den första gruppen C) erhöll hyresgästerna 50% av produktionen. I den andra gruppen (T1) erbjöds arrendatorerna att behålla 75% av produktionen. Denna variation är avgörande för att beräkna incitamentseffekten av ett sharecropping-kontrakt. Arrendatorerna i en tredje grupp (T2) fick behålla samma andel av produktionen som kontrollgruppen (50%) men erhöll en extra fast betalning som var oberoende av deras produktionsnivå. Jämförelsen av T2 med C gör det möjligt för oss att testa för förekomsten av en inkomsteffekt på produktiviteten i jordbruket. Inom den tredje gruppen erhöll hälften av arrendatorerna (T2A) den fasta betalningen i form av en riskfri kontantöverföring medan den andra hälften erhöll sin extra betalning i form av ett lotteri (T2B), den förväntade betalningen i T2A och T2B varande densamma. Jämförelsen mellan T2B och T2A gör det möjligt för oss att testa för förekomsten av en riskexponeringseffekt. När det gäller produktionen finner vi att de områden som har arrendatorer med en 75 procentig produktionsandel genererade en högre jordbruksproduktion än arrendatorer med en 50 procentig andel (T1 gentemot C). Vi finner inte att arrendatorer som erhöll en högre inkomst genererade signifikant mer (eller mindre) produktion jämfört med arrendatorer i kontrollgruppen (T2 gentemot C). När det gäller valet av insatsvaror finner vi att arrendatorer som fick behålla en större andel av sin produktion (T1) investerade mer i kapitalinsatsvaror för att odla sin mark (i form av gödning, jordbruksverktyg och obetalt arbete). I kontrast till detta så investerade arrendatorer som erhöll en högre inkomst (T2) inte mer i kapital eller arbetsinsatser i förhållande till kontrollgruppen. Vad gäller risktagande så finner vi bevis för ett högre risktagande bland arrendatorer med en högre produktionsandel (T1), ett aningen högre risktagande bland arrendatorer som erhåller en riskfri inkomst (T2A) och ett aningen lägre risktagande bland arrendatorer som erhåller den riskfyllda inkomstöverföringen (T2B), samtliga relativt kontrollgruppen (C).

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