Subsidies for agricultural technology adoption: Evidence from randomized experiment in Uganda

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Abstract

Subsidizing an entirely new agricultural technology could aid adoption early in the diffusion process. Based on theoretical framework for technology adoption under uncertainty, we implemented a randomized field experiment among 1,200 smallholders in Uganda to estimate the extent subsidizing an improved grain storage bag *crowds-out* or *crowds-in* commercial buying of the technology. The empirical results show that on average, subsidized households are more likely to buy an additional bag at commercial prices relative to the households with no subsidy who are equally aware of the technology. This suggests that under certain circumstances, such as when there is uncertainty about the effectiveness of a new agricultural technology, and the private sector market for the technology is weak or nascent, a one-time use of subsidy as a means to build awareness and reduce risk could help generate demand for the new technology. In this context, a subsidy can allow farmers to experiment with the technology and learn from the experience before investing in it.

JEL classification: C23, C93, O33, Q12, Q18

Keywords: Subsidy, hermetic technology adoption, uncertainty, crowding-in, RCT, Uganda, sub-Saharan Africa.

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1. Introduction

Over the years, donor agencies, development partners and policy makers in developing countries have recognized and focused on adoption of agricultural technologies such as improved varieties of seeds and inorganic fertilizer to enhance agricultural productivity, growth and development, and to reduce poverty (Evenson and Gollin, 2003; Gollin, Parente, and Rogerson, 2002). In an attempt to accelerate diffusion of these technologies and enhance input use, many countries subsidize these inputs. When subsidies are introduced, however, one of the key issues becomes the extent to which it fosters or hampers commercial market participation (Dupas, 2014; Mason and Ricker-Gilbert, 2013; Ricker-Gilbert et al., 2011). That is, to what extent does subsidy *crowd-in* or *crowd-out* commercial buying of the subsidized agricultural technology? For example, if the subsidized households go out and buy the technology commercially in the future, this means that the subsidy has a *crowding-in* effect. However, if receiving the subsidy reduces future commercial purchases, then the subsidy *crowds-out* the commercial market.

With this in mind, the present article estimates the effects of a one-time subsidy on commercial market participation for an entirely new agricultural technology, hermetic (airtight) storage bags for maize and other grains, among smallholders in Uganda. We use a randomized controlled trial, where subsidy recipients are randomly selected to receive one hermetic bag and are free to go out and purchase additional bags at commercial prices, to make a causal estimate of *crowding-in* and *crowding-out* of the technology. In addition to the main hypothesis tested in this article, we also investigate if there are indirect spillover (information) effects of subsidy by examining the market participation outcomes for households who did not receive subsidy but live in eligible villages where others received the subsidy/treatment.

Some development practitioners fear subsidies could negatively affect adoption in many ways. First, subsidy recipients may anchor on subsidized prices and be unwilling to purchase products at market prices post-subsidy (e.g., see Simonsohn and Loewenstein (2006) for an example of price anchoring), leading to *crowding-out*. Second, recipients may anticipate future subsidies, develop a sense of entitlement, and refuse to buy commercially. Third, subsidies could become a permanent attribute, draining scarce resources. However, for a new technology or an experience good such as hermetic bags, there is also the argument that a one-time or short-term subsidy can create awareness, help with the self-evaluation process and provide information about the technology, thereby accelerating early adoption and diffusion process.

Usually, the debate on whether or not to subsidize agricultural technologies and its impacts has been mostly limited to production inputs such as improved seeds (Mason and Ricker-Gilbert, 2013; Mason and Smale 2013) and inorganic fertilizer (Jayne et al., 2013; Ricker-Gilbert et al., 2011; Xu et al., 2009). Typically, knowledge about these inputs among smallholders are already high and the risk from using them is relatively predictable. However, market participation is relatively low, due to credit constraints, concerns about profitability of using the input and/or lack of supply. As a result, it is difficult to cleanly estimate a *crowding-out/in* impact in the context where farmers are already aware and have the opportunity to buy technologies if they find it optimal to do so.

To inform this debate, we implemented a randomized field experiment in Uganda, where we gave one hermetic bag with the capacity to hold 100 kilograms (kg) of shelled maize to certain randomly selected smallholder households free of charge, and provided free trainings on how to use the technology. These hermetic bags are better and more effective at protecting maize and other grains from insect attacks in storage than the commonly used traditional storage technologies, and they do not require farmers to use insecticides on their stored grains to kill insect pests. However, though more effective at preventing storage loss than traditional storage methods, they are significantly more expensive than traditional storage bags that hold the same amount of maize but offer no protection against insects (e.g.: US $2.50 for a hermetic bag and US $0.50 for at traditional bag). Nevertheless, given the positive characteristics of the technology, the learning effects derived from a one-time subsidy could enhance market participation and adoption of the technology. Conversely, having received a bag for free one time could make recipients very sensitive to the price were they to go out and buy it commercially later, essentially leading to *crowding-out* effect. Ultimately the extent of *crowding-out*, and *crowding-in* of hermetic bags is an empirical question.

The present article makes three important contributions to the literature on input subsidies for smallholder households. First, we contribute to the policy debate on the impact of short-term subsidy on adoption of new technologies. Our article measures *crowding-out* or *crowding-in* using a subsidy for hermetic bags that are a completely new and virtually unknown technology among smallholders at the time of intervention. Our one-time subsidy intervention differs from the government or public subsidy programs usually associated with agricultural inputs implemented in sub-Saharan Africa (SSA). (For examples of such government or public subsidy interventions, see Mason and Ricker-Gilbert, 2013; Mason and Smale 2013; Jayne et al., 2013; Ricker-Gilbert et al., 2011; Xu et al., 2009.) Our article builds on the past literature because knowledge and adoption of hermetic bags was virtually zero prior to subsidy intervention, giving us a clean baseline from which to analyze *crowding-out* and *crowding-in* of the technology.

Second, this article implements a randomized controlled trial (RCT) to cleanly estimate the causal effects of a one-time subsidy on *crowding-out* and *crowding-in*. Usually, agricultural input subsidy programs in SSA are implemented in a non-random manner.[1] Subsidies are either universally available or targeted at specific populations (e.g., Amankwah et al., 2016; Liverpool-Tasie, 2014; Mason and Ricker-Gilbert, 2013; Ricker-Gilbert et al., 2011; Xu et al., 2009). Therefore, participation in the subsidy programs and the quantity of subsidized inputs received, conditional on participation in the program, are typically endogenous if unobserved factors that affect either participation or quantity of subsidized inputs received also affect commercial participation in the markets for these inputs. Previous literature attempt to solve this problem using excluded instruments (e.g., see Amankwah et al., 2016; Liverpool-Tasie, 2014; Ricker-Gilbert et al., 2011). However, the causality derived from estimates from these studies depends on the instrumental variables used (Bound, Jaeger and Baker, 1995; Wooldridge, 2010). With randomized subsidy implemented in this article, the causal inference is internally valid and does not depend on any exclusion instrument. Hence, we cleanly estimate the *crowding-in/out* effect of subsidy on the new technology.

To date, there is little or no rigorous evidence as to how a one-time or short-term subsidy may influence the adoption of entirely new technologies in general, using an RCT framework. The notable exception is Dupas (2014) who finds that short-run subsidies for a new health product, an improved insecticide-treated bed net that is considered an experience good, impacts short-run and long-run adoption through learning effects. Although Duflo et al., (2011) implemented an RCT for subsidized fertilizer in Kenya, the technology was not entirely new in the study area. We build on this sparse literature by using an experimental design to estimate *crowding-out* or *crowding-in* for a new agricultural technology.

[1] A notable exception is Duflo et al. (2011) who implemented a randomized experiment on standard fertilizer subsidies, free delivery, and timing of fertilizer purchase among smallholders in Western Kenya.

Third, we investigate if there are spillover effects of our subsidy intervention on the market participation outcomes for households who were exposed to training on hermetic bags, but did not actually receive the subsidy intervention. With a notable exception in Dupas (2014), spillover effects of subsidy on new technologies are seldom estimated. Our experimental setup allows us to measure these effects, which are important for sustained diffusion and adoption of the technology beyond the lifespan of a one-time subsidy intervention. In addition, for subsidy to be cost effective, the learning experience derived by subsidy recipients from using the subsidized technology, should diffuse informally to non-subsidized households who live in the same villages.

The main result from this study shows that after one year, a one-time subsidy has a positive and statistically significant impact on market participation decisions among treated households. We find that on average, subsidized households are about 5.2 percentage points more likely to buy an additional bag commercially relative to households who were not subsidized but are equally aware of the technology or knowledgeable about it. That is, subsidizing this technology early in the diffusion stage increased market participation. This result suggests that, for an experience good such as hermetic storage bags in this study, short-run subsidy increases market participation because uncertainty associated with the new technology is removed through experience; the subsidy beneficiaries learn of the true value of the technology and are willing to pay for it subsequently. Furthermore, we find a positive (3.4 percentage point) and significant spillover or information effect on adoption of the technology among households who lived within the intervention villages but did not receive the treatment subsidy.

The remainder of this article is organized as follows. In section 2, we discuss the background on storage technologies and rationale for subsidizing hermetic bags in Uganda. Section 3 describes data, experimental design and treatment interventions. The methods including conceptual and empirical frameworks are discussed in section 4. We present the results and discussion in section 5, and conclude with a discussion of policy implication of findings in section 6.

2. Background on Storage Losses and Technologies in Use

2.1. Background

Postharvest losses in grain on-farm in SSA is estimated at about $4b annually (World Bank, 2011). Because of the economic consequences associated with these losses in the region, and sustainability issues from using scarce resources to produce food that is wasted post-production, there is a renewed interest in grain postharvest loss reduction in developing countries to enhance food and income security. Part of this interest and on-going efforts include the promotion of hermetic storage technologies by development partners, donors and policymakers to reduce grain storage losses in SSA region in general.

For example, following the successful dissemination of metal silos to reduce grain postharvest losses in Central and Latin America, the Swiss Agency for Development and Cooperation (SDC) recently promoted hermetic metal silos in Kenya (Gitonga et al., 2013; Tefera et al., 2011). Purdue University, in conjunction with partners and donors under the Purdue Improved Crop Storage Phase III (PICS3) project, is also implementing efforts to promote a triple-layer hermetic bags in Uganda, Ethiopia and Tanzania in Eastern Africa; Nigeria, Ghana and Burkina Faso in Western Africa; and Malawi in Southern Africa. In addition, the World Food Program with donor funding is also implementing a drive for increased hermetic storage technology use in Uganda.[2] Thus, there is a need to examine which tools, economic or otherwise, could enhance early adoption of these technologies.

Although farmer-reported grain postharvest losses in Uganda are relatively low (Kaminski and Christiansen, 2015), precise quantitative assessment of these losses are difficult due to high year-on-year variability in pest infestation (Costa, 2015). Besides, anecdotal evidence suggests that smallholder households take measures including selling at harvest and the use of storage chemicals, among others, to reduce their losses. However, selling early to avoid storage losses may keep households poor because they buy grains at more expensive prices during the postharvest period (Stephens and Barrett, 2011). In addition, the use of synthetic chemicals may be toxic (Williamson et al., 2008). Hence, there is a need and potential market for an effective but chemical-free storage technology.

2.2. Storage Technologies/Practices in Uganda

At study baseline, over 90 percent of households use traditional storage technologies or practices such as granaries, woven polypropylene bags, and heaping maize cobs on bare floor. These technologies/practices are inefficient at storing grains over a long period, and because of this ineffectiveness, as we indicated earlier, smallholder households may be disposing grains earlier than desired to prevent higher rates of postharvest losses. Overall, less than 1 percent of our sample use hermetic (effective) storage technology of any kind at baseline.

At baseline, the most prominent storage technology used by about 73 percent of the households is the regular woven polypropylene bags. These woven bags have a single-layer and oxygen could easily permeate through them, making grains stored in them vulnerable to insect pest

[2] Other agencies or NGOs such as USAID, Feed the Future, One Acre Fund, and Catholic Relief Services (CRS) are currently working to commercialize hermetic storage bags across multiple countries in SSA.

attacks and exposure to storage diseases due to high absorption of moisture content. On the other hand, the triple-layer hermetic bag, the subject of this study, prevents oxygen from the ambient environment, thanks to its two inner layers made from high-density polyethylene. Once insect pests lack access to oxygen for metabolism, they simply become inactive, desiccate and die (Murdock et al., 2012). In general, hermetic storage technologies have proven to be highly effective at preventing grain damage due to insect pest attacks in storage (De Groote et al., 2013; Njoroge et al., 2014; Tefera et al., 2011).

Apart from the difference in functionality of both bags, culturally, the regular and hermetic bags share a distinction that households could use them to store grains in-house. Because the triple-layer hermetic bags are culturally similar to the regular single-layer bags used by majority of households, cultural acceptability should not be a barrier to adoption for the new technology (Rogers, 1995). However, the initial upfront cost of the hermetic bags is higher, which may discourage potential adopters. In our case, the hermetic bags are about 5 times the price of the regular but ineffective woven bags.[3] Therefore, with information and learning derived through experience with the subsidized bags, treated households' valuation of the technology may change and their willingness to pay or participate in the private market increased.

2.3. Subsidy Rationale

Given that hermetic storage technology is new in Uganda, and almost no users present at the baseline, what policy intervention could facilitate its adoption and diffusion relatively quickly, particularly, early in the diffusion process? One of such policies is to create awareness about the

[3] Although the initial upfront cost of the hermetic bags is relatively higher, it is more durable than the regular bags and lasts longer if handled carefully. In addition, the fact that households no longer need to apply storage chemicals on grains stored in the bag at frequent intervals also saves on chemical and labor costs. Over the bag's lifetime, it is presumably cheaper than the regular bags. (See Jones et al., 2014)

technology through large-scale extension activities or mass media, another is to subsidize the technology to facilitate early evaluation—learning by doing and from experience—and adoption; or a combination of both. This article focuses more on the latter albeit in combination with some extension activities.

As earlier indicated, the cost of acquiring a hermetic bag is much higher than the cost of the commonly used regular woven bags. Therefore, smallholders may be unwilling to pay the initial upfront cost for the technology. Furthermore, like many new technologies, there is a level of subjective uncertainty (we discuss this later under the technology adoption framework) or risk associated with using the bags, so risk-averse households may be skeptical about paying for the technology at first. For instance, Donovan (2012), using a dynamic general equilibrium, shows that poor farmers in developing countries put more weight on bad potential outcome, discouraging intermediate input or technology adoption. That is, risk or uncertainty impedes willingness to pay for a new agricultural technology among developing country farmers relative to developed country farmers. Farmers can reduce this uncertainty through self-experience or learning-by-using, or through extension agents or learning from other farmers (Genius, et al., 2013).

However, in Uganda where there is no learning from others or prior experience before the technology was introduced, it is possible that a positive experiential knowledge derived from a one-time subsidy of the technology could enhance learning. Hence, subsidized households may increase their reservation value of the technology, and be willing to pay with their own money, thereby increasing market participation and adoption in the short and long run. Thus, the learning and consequent adoption generated by a one-time or limited-time subsidy may be sustained as evident in a recent study by Fishman et al. (2017) in Uganda. The authors used a novel randomized phase-out experimental design to determine if the adoption of improved seed inputs generated from an implemented short-term subsidy program is sustained beyond the subsidy intervention. They find that there is sustained demand for the inputs even after the subsidy is phased out. Carter et al. (2016) find a similar result of sustained one-time subsidy impacts on agricultural inputs adoption in Mozambique, in SSA. Thus, one-time subsidy may lead to sustained adoption of the hermetic bags.

Economically, if one-time subsidy leads to *crowding-in* effects for this technology, the benefits of the subsidy may indeed generate some external effects leading to *crowding-in* of other production inputs. For instance, Omotilewa et al. (2017) show that in Uganda, households with access to improved storage technology to reduce storage risk and losses are 10 percentage points more likely to cultivate higher-yielding hybrid maize varieties that are known to be vulnerable to insect pest attacks in storage. More so, a recent study by Emerick et al. (2016) in India also find that a new flood-tolerant rice variety, which reduces downside risk from flooding, increased agricultural productivity by crowding in modern inputs such as fertilizer use and cultivation practices. Thus, beyond the potential primary benefit of subsidy leading to *crowding-in* effects for hermetic storage bags, the adoption of the improved storage technology has a potential to increase agricultural productivity in general by *crowding-in* other modern inputs.

3. Data, Experimental Design and Treatment Intervention

3.1. Data Collection and Sampling

The data used in this study come from two waves of household surveys we conducted among 1,200 maize and legume producers in Uganda. The first and second waves of data were collected from October to December 2014 and 2016, respectively, as baseline and post-intervention follow-up surveys. Each wave of data covers two cropping cycles. The baseline survey covers second cropping season of 2013 and first cropping season of 2014. The post-intervention follow-up survey covers the same seasons in 2015 and 2016 respectively. Overall, the entire panel data covers four cropping seasons.

The survey instruments used in both surveys are structured, pre-tested questionnaires that include modules on household demographic characteristics, crop production details, storage technologies used and postharvest grain management practices to preserve stored grains, and marketing activities at harvest and postharvest periods. For the main outcome of this study, we asked if households have ever bought hermetic bag(s) commercially and from which source they acquired it.

To select study participants, we used a multi-level stratified sampling approach. Our target population was smallholder maize and legume-producing households, and we wanted our data to have a semblance of national representation of these producers in Uganda. Therefore, first, we identified the major maize and legume producing districts from within the four main agricultural regions in Uganda, excluding Kampala region, which is largely urban. Using previous data from the publically available Living Standard Measurement Study-Integrated Surveys on Agriculture (LSMS-ISA) from the World Bank, we purposely selected two districts within each region, based on production volume from previous years. The districts are Bukomansimbi (Masaka) and Mubende in Central without Kampala region; Hoima, Kamwenge and Kiryandongo in the West; Oyam and Apac in the Northern region; and Iganga and Sironko (Mbale) in the Eastern region.

Once the districts were selected, within each district, we purposely selected three major maize-producing sub-counties with guidance from the District Agricultural/Production Officer. Subsequently, we randomly selected two parishes within each sub-county; and in each parish, we randomly selected one local council one (LC1) that serve as the community cluster.[4] Within each LC1, we randomly sampled twenty-five households using a computer random number generator to select from a list of community residents provided by the community leader, usually called the LC1 Chairman. Overall, at baseline, we randomly selected 48 LC1s and 1,200 (25 households*48 villages) smallholder households across the country. However, after data clean up, the number of households reduced to 1,190.

3.2. Experimental Design and Treatment Interventions

This study uses a two-level experimental design (see Figure 1 for schematic representation). After the sampling and baseline survey in 2014, in the first experiment at the village level, we randomly assign the LC1s into treatment and control groups, respectively. In our sampling framework, each sub-county has two LC1s and we randomly assigned one into a treatment group and the other into a control group. In total, the LC1s were equally assigned (24 each) into both groups with approximately equal number of households in the treatment and control groups. The treatment LC1s received awareness demonstrations or information sessions on how to use the technology and its effectiveness, whereas the control group of LC1s did not receive any information.

The village-level treatment intervention was implemented by Cooperative League of the USA (CLUSA) Uganda, a non-governmental organization engaged in activities to promote the technology in over 3,000 other villages across Uganda. The organization further engaged many smaller local partners to implement this exercise. In conjunction with CLUSA, we facilitated the

[4] Local council one is the lowest administrative unit in Uganda and it sometimes comprises more than one village. However, we use LC1 and village interchangeably throughout this article. The other administrative units in hierarchical order are region, district, sub-county and parish, respectively.

training of trainers (TOTs) for both the local implementers as well as our enumerators. Thus, the local extension agents and our enumerators received the same training.

Importantly, all households living within the villages randomly assigned to receive demonstration activities were invited to participate in the activities, regardless of whether they were sampled as part of this study or not. Using pilot farmers, the demonstration activities were designed to create awareness about the technology; and to instruct participants on how to use it effectively, how it works, and the potential benefits of using the technology for inter-temporal price arbitrage and food security purpose (see Baributsa et al. (2014) for more details on the demonstration activities). As such, we would expect general awareness level about the technology to be high within the treatment villages.

[Insert Figure 1 about Here]

Subsequently, we conducted the second experiment, which is the centerpiece of this study, at the household level. Within each of the treatment LC1s that had received information about the technology, we gave out a single 100kg-capacity PICS hermetic storage bag to 10 randomly selected households.[5] These 10 households were selected from a sample size of 25 per LC1, based on the baseline sampling (Figure 1). That is, for a household to be eligible to participate in the household-level treatment, such household must be living within a village assigned to treatment.

In addition, to be certain that treated households who received subsidized bags understood how the technology works, our trained enumerators re-affirmed or instructed the households on

[5] The treated households were at liberty to do anything with the bag. We neither compelled them on what crops they could store in it nor told them we would return for 'inspection'. They could either use the bags to store own grains, give them out to friends free of charge, or even sell the bags if they want. Each bag costs 7,000 UGX (about $2.50) at the time of intervention.

how to use the technology effectively, regardless of whether or not they attended the village-level demonstration activities. Recall that our enumerators received equal training as the local extension agents who implemented the village-level demonstration activities. Thus, the second experiment may be considered as the receipt of one subsidized bag plus a refresher instruction on how to use the technology properly. Importantly, however, other than the information provided about how to use the technology, which is essentially the same information equally provided at the village level, our enumerators did not give any other details or suggestion of where households could purchase the technology if they wanted to buy additional bags.

Furthermore, we should mention that the choice to give out a single 100kg capacity bag is for two reasons: First, the one bag is meant to be used as an evaluation tool to examine if the subsidy generates positive learning experience, leading to market demand among recipients. For example, based on our baseline data, an average household should need about six bags to store maize if they were to use hermetic bags alone for storage (see Table 1). Second, we believe that a single bag should not create an overwhelming undue advantage for treated households over their untreated cohorts.

In summary, our experimental design generated three groups of households. First, the group of households living in randomly assigned treatment villages and received subsidy (group 1 in Figure 1). The second group received no subsidy but lived within the treatment villages (group 2 in Figure 1). Lastly, the third group of households living within the control villages with neither access to information nor subsidy (group 3 in Figure 1). For the main analysis in this article, we restrict our comparison of subsidy effects to informed households only (groups 1 and 2); but we also report the full sample analysis including control villages (group 3) which did not receive any information about the technology. That is, our primary analysis is based on the second experiment at the household level within the villages treated with information.

Another important point worth mentioning here is that unlike the Dupas (2014) study, which limited the purchase and supply or sales of subsidized insecticide-treated bed nets to their program or study distribution channel only, participants in our study could purchase the storage technology directly through the private market channel. In fact, the technology supply chain and market distribution is completely out of the scope of this study. This makes our study more realistic and reminiscent of what happens in the nascent markets within the treatment (and other) villages across the country in general. Vendors locally sell the hermetic bags, but they are not necessarily available in all the villages, treatment or otherwise, because the usual supply constraints for any new technology apply.

3.3. Attrition Bias

In an experimental study like this, attrition bias could be problematic if attrition rate is high or attrition occurs in a non-random manner. From our baseline sample, we randomly treated 240 households in 2015 and we were able to re-interview 233 of them in a follow-up survey in 2016. This implies an attrition rate of less than 3 percent in the treatment group. The overall attrition rate when both groups are combined is less than 5 percent. That is, two years after the baseline survey, we successfully re-interviewed slightly over 95 percent of our baseline households in the follow-up (post-intervention) survey. Therefore, our attrition rates are generally comparable or lower relative to other studies in the region. For example, Matsumoto and Yamano (2010) had an attrition rate of 4.8 and 6.7 percent from a two-wave panel surveys in Uganda and Kenyan, respectively. The major reason for attrition is that households migrated away from the sampled villages and our enumerators could not trace or contact them during the follow-up survey period. For a low attrition rate as found in this study, attrition bias should not be an issue to the internal validity of our study estimates.

Nevertheless, to check if attrition occurs in a non-random manner, we regress the baseline outcome variable and some household characteristics on a binary indicator that equals one for attritted households and zero otherwise. Table A1 in the appendix presents these regression results. In general, the results suggest there is no systematic difference across attritted and returning households, ex-ante. There is no statistically significant difference on the market participation variable across both groups. In addition, most of the other household variables such as income, education, ownership of assets and production are random across both groups.

However, we find a systematic difference across age and household size. On average, attritted households are 7 years younger than the returning households are, indicating that younger households are more mobile, probably to seek better opportunities. We further find that attritted households are likely to be one member less, on average, relative to the returning households, suggesting easier mobility for smaller family size. Despite the systematic differences across these two variables, we believe there is no threat to our design because of the relatively low rate of attrition, and there is no reason to believe these two variables were influenced by our randomly assigned treatment. Besides, we include both variables as controls in all analysis and our results are robust across specifications.

4. Methods

4.1. Technology adoption under uncertainty

Households in this study live in rural Uganda and face market failure for this technology in the form of incomplete information, risk and uncertainty, among others. To overcome this market failure, we provide information about the technology through demonstration activities at the village level as well as subsidy at the household level. Previous literature demonstrates that risk-averse farmers may delay partial or total adoption of a technology in the presence of incomplete information or uncertainty (Dercon and Christiaensen, 2011; Feder 1980; Feder and O'Mara, 1982; Feder and Slade, 1984; Feder and Umali, 1993; Hiebert, 1974; Knight et al., 2003; Koundouri et al., 2006; Leathers and Smale, 1991; Saha et al., 1994). We therefore examine subsidy as an economic incentive to induce faster adoption and early diffusion of a post-production technology.

The theoretical approach in this article assumes that farm households are risk-averse and that they maximize the expected utility of random profit (or wealth) following similar approach in Koundouri et al., (2006) and Saha et al., (1994). Because we are dealing with post-production grain storage, the "inputs" are grain quantities stored at harvest (q_h) and the types of technologies used to store the grains: a vector of traditional technologies including storage chemicals, silos, or woven bags ($\boldsymbol{x}$); or the new hermetic storage technology (x_{herm}) that we introduce in this study. Assuming a well-behaved "production" function[6] $f(.)$, the output (q_l) is the quantity of intact grain in storage at a later time in the postharvest period. In general, for simplicity and because we are dealing with post-production decisions, we assume the grain quantity stored is given and that households simply have to allocate grains to either traditional or hermetic storage technology. Hence, a farm household maximizes its expected utility of random profit as in equation (1); with the "output" function defined in equation (2).

[6] A well-behaved production function is continuous and twice differentiable. Production is in quotation marks because it is not the traditional production function using some inputs to produce an output.

$$\max_{\boldsymbol{x},x_{herm}} E[U(\pi)] \equiv \max_{\boldsymbol{x},x_{herm}} E[U(p_l q_l - \boldsymbol{r}\boldsymbol{x} - r_{herm}x_{herm})] \tag{1}$$

$$q_l = f(\, q_h, \alpha(\boldsymbol{x}, x_{herm}, \varepsilon), \boldsymbol{x}, x_{herm}) \tag{2}$$

In the utility equation (1), $p_l, \boldsymbol{r}$, and r_{herm} are exogenous output price in the lean period that occurs some months after harvest and storage, price vector associated with the cost of allocating grains in traditional storage technologies, and price of the hermetic storage technology, respectively. The cost of allocating grains in traditional technologies include chemical and labor costs, and the cost of procuring the traditional technologies such as woven bags used. We further assume a concave and twice-differentiable utility function (U), and E is the expectation symbol.

In addition, the production function contains the term $\alpha(.)$, which is the household's percentage of expected grain loss in storage ($0 < \alpha < 1$) that indicates the level of effectiveness of any given storage technology used. Broadly, alpha is a function of storage technology used and incorporates some level of subjective uncertainty (ε) associated with endogenous use of the new technology. In our case, the subjective uncertainty comes from two sources: First, the households may doubt the effectiveness of the technology and its ability to keep stored grains intact without using storage chemical protectants as in other technologies; second, the households may be uncertain of their own ability to use this new technology properly to prevent storage losses.[7] Hence, we refer to this type of uncertainty as endogenous. There is also a general exogenous "production" uncertainty—such as insect pest infestation in storage—associated with inter-temporal transfer of grain from harvest to lean period, through storage. The households lack control over this second

[7] Even though households may have doubts about the technology or their own ability to use the technology correctly due to lack of experience, however, the technology is risk reducing if used correctly. Thus, this case is different from the typical uncertainty associated with other agricultural inputs such as fertilizer use, which may depend on exogenous rainfall.

type of uncertainty, hence, we refer to it as exogenous. For simplicity and because exogenous uncertainty is common to all regardless of storage technology decisions, we ignore this uncertainty.

Substituting equation (2) into (1), the first order conditions for use of hermetic storage technology is given by[8]

$$E\left[U'(.) * \left\{p_l f_{x_{herm}}(.) - r_{herm}\right\}\right] = 0 \tag{3a}$$

$$\frac{r_{herm}}{p_l} = E\left[f_{x_{herm}}(.)\right] + \frac{cov\,(U'(.),\ f_{x_{herm}}(.))}{E[U'(.)]} \tag{3b}$$

where $U'(.) = \partial U(\pi)/\partial \pi$, and $f_{x_{herm}}(.) = \partial f(.)/\partial x_{herm}$. Equation (3b) equilibrium demonstrates that for a risk neutral household, the input/output price ratio equals expected marginal product of the hermetic storage technology, because the second term (covariance) on the right-hand side (R.H.S) of the equation essentially becomes zero. However, for a risk averse household, the second term is not equal to zero. It captures the diversion from risk neutrality and is equivalent to the marginal risk premium associated with using the new technology.

To apply the general model above specifically to household's decision to participate in commercial buying of the technology, we posit that a household will adopt the more efficient storage technology provided the expected utility derived from it is greater than the expected utility without the technology. Assuming a binary market participation/adoption decision (1 represents adoption and 0 otherwise), where the expected storage losses from adopting the technology is much lower than otherwise, that is, $\alpha^1(.) < \alpha^0(.)$ indicating better effectiveness if hermetic technology is used; then $E[U(\pi^1)] - E[U(\pi^0)] > 0$, where the expected utility maximizing problems and solutions are similar to the general model in equations (1) and (3), respectively. However, due to subjective uncertainty associated with the use of hermetic technology and

[8] We focus our attention solely on the hermetic technology allocation simply because the FOCs for both storage technologies can be solved independently of each other and, hence, are separable.

captured in the second term of the R.H.S in equation (3b), following Koundouri et al. (2006), we posit that households may delay adoption to obtain more information about the technology. Thus, an additional information premium (P) enters the adoption condition, particularly for a risk averse household, as shown below.[9]

$$E[U(\pi^1)] - E[U(\pi^0)] > P \geq 0 \tag{4}$$

As a result, because household's choice of technology is based on subjective probabilities (Feder et al., 1985), direct access to the technology in the form of subsidy provides the ability to self-evaluate the technology, lowering the information premium among subsidy recipients. Given that positive experiential learning reduces subjective uncertainty associated with the technology ab initio, we would expect a positive movement in commercial market participation. However, if negative experiential learning occurs, there might be none or negative private market participation after having received a subsidized hermetic bag.

4.2. Empirical Framework: Modelling Direct Subsidy Effects

As stated in the experimental design, the households are approximately equally divided into the treatment and control clusters. Because the main objective of this article is to estimate the impact of one-time subsidy for hermetic storage bag on market participation for subsidy recipients, and only households within the information treatment clusters were eligible for the subsidy intervention, we analyze data from within the treatment clusters comprising both subsidized and non-subsidized households who are informed about the technology. Nevertheless, the pure control group without access to information or subsidy treatments provides a base to tease out spillover effects within the treatment group.

[9] We think of P as the value of information that is both related to uncertainty associated with the technology use as well as the cost of the technology.

To estimate the effect of subsidized hermetic bags receipt on households' commercial market participation, we compare the average potential outcome between the subsidized group of households and the unsubsidized group. Our randomized treatment allows us to use simple estimation strategies to estimate the effects of subsidy on the recipients. Without the subsidy treatment, the potential outcomes would be the same within the treatment and control groups (Duflo et al., 2007). To determine the subsidy effect on market participation (*crowding-in/out*) as indicated above, we use different estimators that use both the post-intervention data only as well as exploit the panel (baseline and follow-up) nature of our data. If randomization of the treatment status were truly successful, all the estimators should produce similar estimates.[10]

First, we estimate the treatment effect, τ_{SMD}, using the simple mean difference (SMD) from the post-intervention data as follows:

$$Y_{ij} = \alpha + \tau_{SMD} Sub_i + \boldsymbol{\beta X_{ij}} + \sigma_j + \varepsilon_{ij} \tag{5}$$

Where Y_{ij} is a binary outcome variable that equals to one if household i in region j buys the technology commercially, and zero otherwise; Sub_i is a binary indicator variable equals to one if household is treated, (i.e., receives subsidized bags, and zero otherwise). The main parameter of interest that estimates subsidy effects on commercial market participation is τ_{SMD}. The model estimated also includes a vector of household characteristics, $\boldsymbol{X_{ij}}$, such as age, sex, and education status of the household head, and size of the households to enhance precision; and the region fixed-effects, σ_j. The variable ε_{ij} represents the idiosyncratic error term.

[10] We only estimated the intention-to-treat effects of subsidy receipt on commercial market participation in this study. We found that some treated households did not use the technology because they did not have grains to store from a bad production year due to drought. Therefore, ITT effects are effectively lower bounds of the *crowding-in/out* estimates. Estimated effects may be higher among compliant households who used the technology received.

Our second specification, difference-in-difference (DiD) estimator, uses the panel data from before and after our subsidy intervention. This specification adds the time subscript, t, and is estimated as follows:

$$Y_{ijt} = \alpha + \varphi Sub_i + \kappa post_t + \tau_{DiD} Sub_i * post_t + \boldsymbol{\beta X_{ijt}} + \sigma_j + \varepsilon_{ijt} \quad (6)$$

In addition to the variables described in equation (5), $post_t$ indicates one if the observation is from the 2016 post-intervention survey, and zero otherwise. The interaction term between this variable and whether a household got treated is represented by $Sub_i * post_t$. In this case, the parameter of interest is τ_{DiD}, the coefficient on the interaction term.

Finally, we remove any time-invariant unobserved heterogeneity at the household level using the fixed-effect (FE) estimator to demean the observations.

$$Y_{it} = \alpha + \tau_{FE} Sub_i + \boldsymbol{\beta X_{it}} + \eta season_t + c_i + \varepsilon_{it} \quad (7)$$

In addition to variables defined in equations (6), the FE model includes the time-invariant unobserved heterogeneity, c_i, and also include the season fixed-effects, which are binary indicator variables for three of the four agricultural seasons covered in our survey; η is the parameter associated with the season variable. For the FE specification, the parameter of interest is τ_{FE}, the coefficient on the subsidy variable.

4.3. Empirical Framework: Modelling Spillover Effects

We estimate the empirical model for the information spillover effects using similar approaches as the subsidy estimates in equations (5) through (7). The only difference is that we created additional binary variable, *Spillover* to replace the *Sub* variable, indicating one if a household lived in a treatment village where training on hermetic bags occurred, but received no subsidy, and zero otherwise. This group of exposed households (group 2 in Figure 1) is then compared with households in the pure control group that lived in villages that did not receive training on the technology, nor were they in contact with the subsidized households in the treatment villages (group 3 in Figure 1).[11] The variable *Spillover* should be uncorrelated with the error term in the regression equations, provided randomization is successful, because it is equally randomly assigned. In the next section, we show that randomization was indeed successful. We present the estimated equations for spillover effects as follows:

$$Y_{ij} = \alpha + \gamma_{SMD} Spillover_i + \boldsymbol{\beta X_i} + \sigma_j + \varepsilon_{ijr} \quad (8)$$

$$Y_{ijt} = \alpha + \varphi Spillover_i + \kappa post_t + \gamma_{DiD} Spillover_i * post_t + \boldsymbol{\beta X_i} + \sigma_j + \varepsilon_{ijt} \quad (9)$$

$$Y_{it} = \alpha + \gamma_{FE} Spillover_i + \boldsymbol{\beta X_i} + \eta season_t + c_i + \varepsilon_{it} \quad (10)$$

Similar to previous specifications, equations (8) through (10) represent the simple mean difference (SMD), difference-in-difference (DiD), and fixed-effects (FE) estimators, respectively. The variables retain their previous definitions as described in equations (5) through (7). The new variable $Spillover_i * post_t$ is the interaction term between *Spillover* and the binary indicator that the observation is post-intervention, and γ represents the estimated spillover effects.

4.4. Robustness Checks

Considering the fact that the effects of subsidy estimated above used only exposed households sample that is eligible for the treatment in the first place, one may argue that the causal effects estimated may not be direct subsidy effects solely. That is, the estimates may reflect both the subsidy impact and information derived from the demonstration activities in the treatment villages. We argue that this is not the case. Using the full sample that combines observations from groups

[11] The spatial distance between the pure control and treatment villages is far enough to avoid transfer of information between the two groups. Typically, a pair of treatment and control village is identified within one parish (then next level or administrative unit), and the minimum distance recorded between any pair of villages was about 2km. Thus, the two groups are close enough for similarity; but far enough to avoid contamination from subsidized households.

1 (subsidy recipients), 2 (exposed group) and 3 (pure control group) in Figure 1, we can estimate the subsidy and spillover (or potential information) effects jointly and our results should still be consistent.

4.4.1. Empirical Model: Jointly Estimated Effects

Following similar approaches with different estimators used previously, we jointly estimate the direct subsidy and indirect spillover piece together as follows:

$$Y_{ijr} = \alpha + \tau_{SMD} Sub_i + \gamma_{SMD} Demo_j + \boldsymbol{\beta X_i} + \sigma_r + \varepsilon_{ijr} \tag{11}$$

$$Y_{ijrt} = \alpha + \varphi Sub_i + \rho Demo_j + \tau_{DiD} Sub_i * post_t + \gamma_{DiD} Demo_j * post_t + \boldsymbol{\beta X_i} + \kappa post_t + \sigma_r + \varepsilon_{ijrt} \tag{12}$$

$$Y_{ijt} = \alpha + \tau_{FE} Sub_i + \gamma_{FE} Demo_j + \boldsymbol{\beta X_i} + \eta season_t + c_i + \varepsilon_{ijt} \tag{13}$$

Similarly, equations (11) through (13) represent the simple mean difference (SMD), difference-in-difference (DiD), and fixed-effects (FE) estimators, respectively. The variables retain their definitions as before. However, we added a new variable $Demo_j$, to the full-sample model specifications.[12] This variable is a binary indicator variable that equals to one if a village is treated with demonstration activities or information session; and zero otherwise. That is, the $Demo_j$ variable equals to one for the randomly assigned demonstration villages comprising groups 1 and 2 in Figure 1; and zero for group 3. By including all exposed and pure control households in the same estimation equation, the randomly assigned $Demo_j$ variable should in effect be similar to the

[12] Recall, there are two levels of randomized experiment. First is at the village level where we randomly selected 24 villages to receive information or demonstration activities to create awareness about the technology, and the remaining 24 villages received no demonstrations. The second treatment is at the household level where we randomly selected some households within the demonstration villages to receive one subsidized bag each.

$Spillover$ variable earlier described. In these jointly estimated effect equations, similar to previous notations, τ and γ represent the direct subsidy effects and spillover effects, respectively.

The addition of the $Demo_j$ variable in the full-sample estimations cleanly identifies the spillover effects on unsubsidized households within the villages eligible for intervention. This variable represents randomized eligibility for the subsidy treatment at the village level. Therefore, the estimated coefficients associated with this variable may be interpreted as the impact of the living in a village with information from the demonstration activities, a transfer of experiential learning from subsidy, i.e., the spillover effects of the subsidy intervention; or a combination of both. We attempt to address

5. Results and Discussion

To establish a causal effect on whether or not a one-time subsidy influences market participation in any way, we must establish that our attempts to make subsidy receipts exogenous via randomization is successful. Since we have baseline data on participating households prior to random assignment and intervention, we are able to check if observable household characteristics are balanced, ex-ante.[13] Thereafter, we present and discuss the experimental results from the estimation of a one-time subsidy effects on commercial purchase of the technology. These results include both the direct and indirect or spillover effects of the treatments on market participation.

5.1. Baseline Characteristics and Randomization Balance Checks

Table 1 presents the checks for randomization balance at baseline. Specifically, we regress ex-ante market participation and some household and production characteristics on the treatment variable,

[13] In theory, a successful randomization should also balance unobserved characteristics between the two groups, but we have no way of checking this.

including region fixed-effects. In column (1), we show the mean variables for the control group. That is, the group of unsubsidized households living within the LC1s with information about the technology. Column (2) shows the standard deviation, while column (3) shows the regression coefficients associated with subsidy receipt at baseline. That is, column (3) shows the ex-ante mean difference between the randomly assigned group of subsidy recipients and the control group of households. Columns (4) present the p-values associated with the coefficients in column (3) for inference, and column (5) shows sample size for each variable.

First, all the coefficient estimates are negligible and none is statistically different from zero, indicating that randomization was indeed successful at making the subsidy treatment exogenous. Second, on average, private market participation or number of households who bought hermetic bags at commercial price was 0.3 percent, at baseline. On household characteristics, the average age of the household head is 45 years, household size is about six persons, 18 percent of the households are female-headed, and 16 percent are polygamous. The annual average household income is about 2.3m Ugandan Shillings[14], and most households have access to information through radio (78 percent) and mobile phone (71 percent).

In addition, regarding maize production and postharvest grain management practices, the average annual acreage cultivated is about one-half hectare, indicating these households are indeed smallholders. On average, annually, households produced 840kg of maize and stored nearly 600kg. By implication, as earlier indicated, an average household would need about six (100kg) hermetic bags to store their grains effectively, indicating a potential for private market participation or investment. Furthermore, the self-reported expected postharvest loss is about 4.5 percent. The balance of this variable across both treatment and control groups is important. If this variable were

[14] At baseline in 2014, 1 USD=2,800 UGX

systematically different across groups, it would indicate that one group expects a higher level of losses, suggesting they are more inclined to seek remedial technologies to reduce losses relative to the other group. Lastly, the types of storage technologies used are balanced across both groups with the majority of households using traditional storage technologies.

[Insert Table 1 about here]

5.2. Direct Effects of the Subsidy

Table 2 presents the direct effect of subsidy (receiving a free bag) on households buying additional bag commercially. For each estimator, we first present the results without additional covariates and subsequently added covariates in the next column. In all specifications, including household covariates such as age and education level of the household head, household size, and being a female-headed household in the regressions did not affect the estimates indicating that the estimates are robust.

In columns (1) and (2), the SMD estimator shows that on average, for each free bag received by a treated household, their likelihood to participate in the private market afterwards increases by 5.4 percentage points, relative to a household who did not receive a bag. Similarly, the DiD estimates in columns (3) and (4) shows a 5.2 percentage point increase in likelihood of commercial participation. Lastly, the FE estimator in columns (5) and (6) also shows estimates of about 5 percentage points increase in likelihood. All estimated parameters are statistically significant at $p\text{-value}<0.05$. As expected, the estimated coefficients across multiple specifications are robust regardless of the estimator used because the randomization process was successful.

Our findings show that receiving a subsidized bag actually *crowds-in* commercial market participation. These results are consistent with literature that have shown evidence of input subsidies leading to *crowding-in* effect where private market for the input is nascent in SSA (Amankwah et al., 2014; Liverpool-Tasie, 2014; Xu et al., 2009). For instance, Liverpool-Tasie (2014) find evidence of *crowding-in* of fertilizer subsidies on commercial market participation in Kano state, Nigeria, and Xu et al. (2009) in Zambia similarly finds that fertilizer subsidy generates demand and *crowds-in* the private sector in poor areas with relatively inactive private sector. In our case, the private sector market activities for the hermetic technology are very low at the time of this study. Moreover, because the technology is completely new in Uganda, there is no previous use or prior experience derived from the technology other than through the subsidy program.

[Insert Table 2 about here]

Similarly, the results from the present article is consistent with Dupas (2014) who finds that short-run subsidies for a new health product, an improved insecticide-treated bed net that is considered an experience good, impacts short and long-run adoption through learning effects. The evidence from our findings suggests that household's personal experience derived from using the subsidized technology has a potential to affect short-term adoption through learning effects. Survey evidence further suggests that households who used the technology had a positive learning experience from it (see appendix table A.2).

Although the magnitude of the subsidy effect estimates might appear small at 5.2 percentage point increase, it is important to put this magnitude into perspective. First, this impact is measured after just one year (two agricultural seasons) following a one-time intervention.

Second, the private market for the technology is nascent and supply-side is relatively underdeveloped. For instance, when asked why a household aware of the technology is not using it, majority (63%) of the households responded not knowing where to buy it or that there is no vendor selling the bags around them (see Figure 2). This indicates that there is a large supply-side constraint. Hence, in general, market participation may increase once the supply side is up to speed with the demand.

[Insert Figure 2 about Here]

Third, in the two agricultural seasons that we asked about in the follow-up post-intervention survey, households experienced drought leading to lower outputs. For example, relative to the baseline survey, households produced 360 kilograms of maize less and stored 150 kilograms less. This suggests that lowered production and storage could equally have lowered demand for the technology. Lastly, relative to the speed of previous technology adoptions in Uganda, this effect is not a bad return. For instance, Matsumoto and Yamano (2011) find that inorganic fertilizer and hybrid maize variety adoptions is at 3 and 21 percent respectively in Uganda, relative to 74 and 59 percent respectively in Kenya; despite these technologies being available to smallholders for many years.

On another hand, Fishman et al. (2017) find a persistent learning effect and sustained market participation from a temporary input subsidy intervention for higher-yielding maize seeds implemented in Uganda. If this finding holds true in our case and the supply constraints reported in Figure 2 are relaxed, based on the estimates above, it is possible that a short-term or one-time subsidy coupled with some extension efforts could lead to sustained adoption of this new improved storage technology in Uganda.

5.3. Indirect Spillover/Information Effects

Does this positive experience and market participation extend to others living within the treatment villages with no subsidies? Table 3 presents the estimated spillover effects comparing groups two and three in our experimental setup. Overall, we find evidence of positive spillover effects on commercial market participation. The estimated effects using similar specifications as in the direct effect estimations suggest that living in a village where others received a free bag increases the likelihood that an untreated household will participate in the market by 3.2 to 3.5 percentage points, on average. As expected, the results are also robust to controlling for household-level controls.

[Insert Table 3 about Here]

Given the supply-side constraints described earlier, and that the private sector was relatively inactive at the time of study, these marginal estimates are meaningful for diffusion of the technology. In fact, we should point out that the spillover effect may be the portion associated with demonstration activities (village-level trainings) to create awareness about the technology within the treatment villages. If that is the case, it appears experiential knowledge derived from receiving a free bag plus training (subsidy) has a higher impact than the village-level information sessions captured partly in the spillover effects. The direct subsidy effect is about 1.6 times the magnitude derived from information session or indirect spillover effects.

Nevertheless, the spillover effects, albeit smaller in magnitude, are crucial to sustained diffusion of the technology beyond the subsidy intervention, as well as cost-effectiveness of subsidy. Besides, for a subsidized household, buying an additional hermetic bag indicates demand for a second bag; whereas for unsubsidized households, it reflects demand for their first hermetic bags, which may be an attempt by these households to self-evaluate the technology. This may explain why market participation among subsidy recipients is nearly twice as much of the spillover households.

We acknowledge that we are not able to separate the whole spillover effects into whether they had come from information received through demonstration activities at the village level, or through informal learning from subsidized households; figure3 shows the sources of awareness or learning about the technology from within each experimental group. The results show that while almost 90 percent of the subsidized households (top graph) reported demonstration activities or direct contact with hermetic bag technicians as their main source of information, only 48 percent of the spillover households (middle graph) reported the same sources. Informal sources of information such as learning from other farmers, friends or relatives accounted for the source of awareness for 34 percent of the spillover households. These results suggest that the spillover effects may have been divided almost equally into effects from both the village-level demonstration activities as well as the informal learning from subsidized households who constitute the other farmers, friends and relative source of information.

Relative to the spillover group, other sources of information category, which includes radio/TV, NGOs, input dealers, village markets, etc., account for nearly 40 percent of awareness for the pure control households from villages with no training on hermetic bags (bottom graph).

In addition, 27 percent of these households reported awareness through informal sources of information.[15]

[Insert Figure 3 about Here]

5.4. Robustness Checks Results: Jointly Estimated (Direct and Indirect) Effects

Table 4 presents estimated results from the full-sample equations. Again, columns (1) and (2) present the SMD without and with household covariates, respectively. The same applies to the DiD and FE estimates in columns (3&4) and (5&6), respectively. The estimated subsidy effects of about 5 percentage points from the full-sample estimates are practically the same as those from the sample eligible for the treatment only (Table 2), regardless of the model estimated. For instance, the *crowding-in* effect estimated through the DiD estimator is 5.2 percentage points in the full-sample (Table 4) and constrained-sample (Table 2). This confirms that the estimates from table 2 are indeed consistent and represent the subsidy effects.

[Insert Table 4 about Here]

Importantly, like the direct subsidy effects estimates, the spillover effects (gamma in rows 3 and 4) when jointly estimated with subsidy effects using the full sample produces similar results to the spillover effects estimated in table 3, for all the estimators. Specifically, for instance, in

[15] Some village-level demonstrations were inadvertently implemented in the pure control villages by the implementing partner, CLUSA Uganda. This explains why 12% reported knowing about hermetic bags through demonstrations. In addition, few of the trained extension agents were entrepreneurial and tried marketing the bags in some control villages where they lived. That accounts for the 22% who reported knowing through hermetic technicians in the pure control villages. This may lead to an underestimation of our spillover estimates if control households bought hermetic bags from these agents.

columns (1) and (2), the simple mean difference estimator that compared mean effects between both groups, using the post-intervention data only, shows that living is a village eligible for subsidy increases the chances of market participation by 3.4 percentage points (same as the estimated spillover results in table 3). In addition, using the panel data for the DiD and FE estimators in the full-sample models produce statistically identical results (table 4) as the eligible-sample model results presented in table 3. The additional covariates included also lack statistical significance as with the restricted-sample estimates.

Therefore, we conclude that using either the full-sample model specification or the eligible-sample specification does not change the estimated results for both subsidy and spillover effects, respectively. As indicated earlier, one limitation to the interpretation of the spillover effects estimated is the difficulty to separate the spillover effects into either effects of information from subsidized group or the village-level demonstration activities. Moreover, we discussed this issue already using figure 3 to describe the sources of information about awareness of the technology. We approximated what fraction of the spillover effects is derived from learning from others vs. learning from demonstration activities, using share of information from each source of information.

6. Conclusion and Policy Implications

This article rigorously evaluates the use of an economic tool, a one-time subsidy, among smallholders in Uganda to create incentives to enhance commercial market participation or adoption of hermetic storage bags under uncertainty. We use randomized field experiments to estimate the direct and indirect effects of subsidy on commercial market participation for this new product. We provide a new experimental evidence for a rigorous evaluation lacking in a developing country context. Randomization of our subsidy program enables a clean unbiased estimate of the impacts of the intervention on an entirely new technology. Although the hermetic technology evaluated is similar to the regular woven polypropylene storage bags used by majority of the households prior to our intervention, it is more effective at preventing insect pest attacks in stored grains. It also has the added benefit that there is no need for application of synthetic storage chemicals on stored grains, if the bags are used or sealed properly. However, because the cost of the new technology is much higher than the ordinary woven bags, smallholders may be reluctant to purchase the technology due to uncertainty.

The empirical findings from this study are as follows: First, the receipt of a one-time subsidy enhances the likelihood of households participating in the commercial market to buy additional bag(s). Results from multiple model specifications and estimators show a 5.2 percentage points increase in likelihood, and statistically significant at 5% level or lower. Second, an indirect or spillover effect of the subsidy intervention exists among neighboring households. These households did not get a subsidy intervention, but live in villages that were eligible for the intervention. However, at 3.2 percentage points and statistically significant at 1% level, the indirect effect is lower in magnitude relative to the direct effects. Lastly, we perform robustness checks that lend credence to the consistency and unbiasedness of the estimated effects.

The main policy implication from this study is that a one-time or short-term subsidy may be an effective tool at spurring demand for a new post-production agricultural technology. In this case, subsidy creates a positive experiential learning effect that reduces the level of uncertainty associated with the adoption of the technology. Provided there is a persistent learning effect such as demonstrated in Fishman et al. (2017), demand for the technology should be sustained beyond the subsidy intervention. Moreover, we need to take into account that the supply side of the market for this technology is barely functional at the time of this study, due to insufficient dense network of agricultural input retailers (see Coulibaly et al., 2012 for such challenges in West and Central Africa). Hence, subsidy effects on market participation may potentially be larger in the long run once supply-side constraints are addressed. In addition, because early adopters of the technology provide information and learning that affect subsequent potential adopters, there is a positive externality generated by subsidizing these early adopters.

Acknowledgments

The authors gratefully acknowledge funding from the Bill and Melinda Gates Foundation (BMGF), Seattle, Washington, under the Purdue Improved Crop Storage (PICS3) project, and supplementary funding from the Norman E. Borlaug Leadership Enhancement in Agriculture Program (Borlaug LEAP) for post-intervention survey. We also thank our team of enumerators led by George Sentumbwe for data collection activities, and the International Institute of Tropical Agriculture (IITA) Kampala for logistical support.

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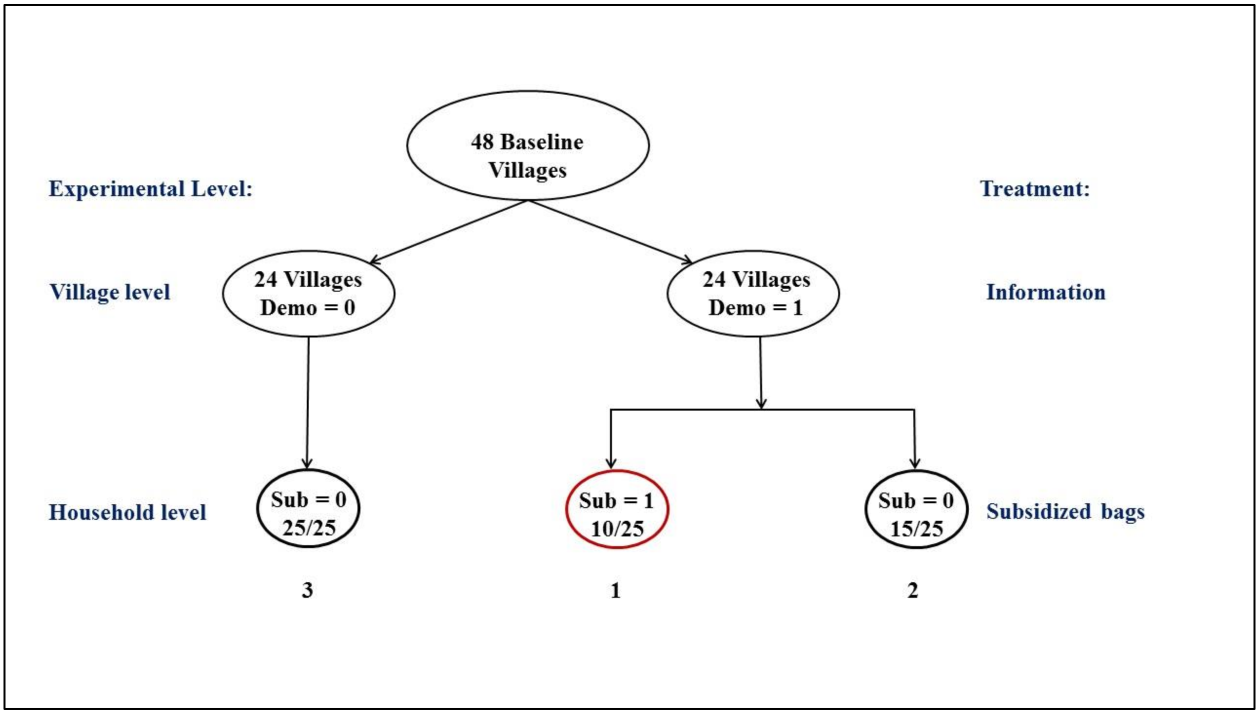

Figure 1: Experimental design

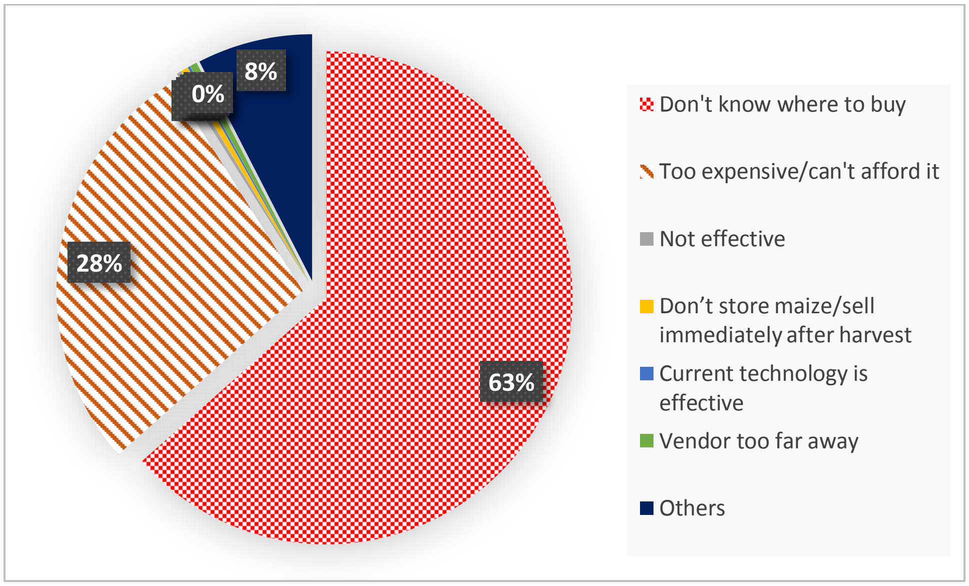

Figure 2: Reasons for not buying hermetic bags, if aware of it

Note: 50% of the total sample are aware at endline survey.

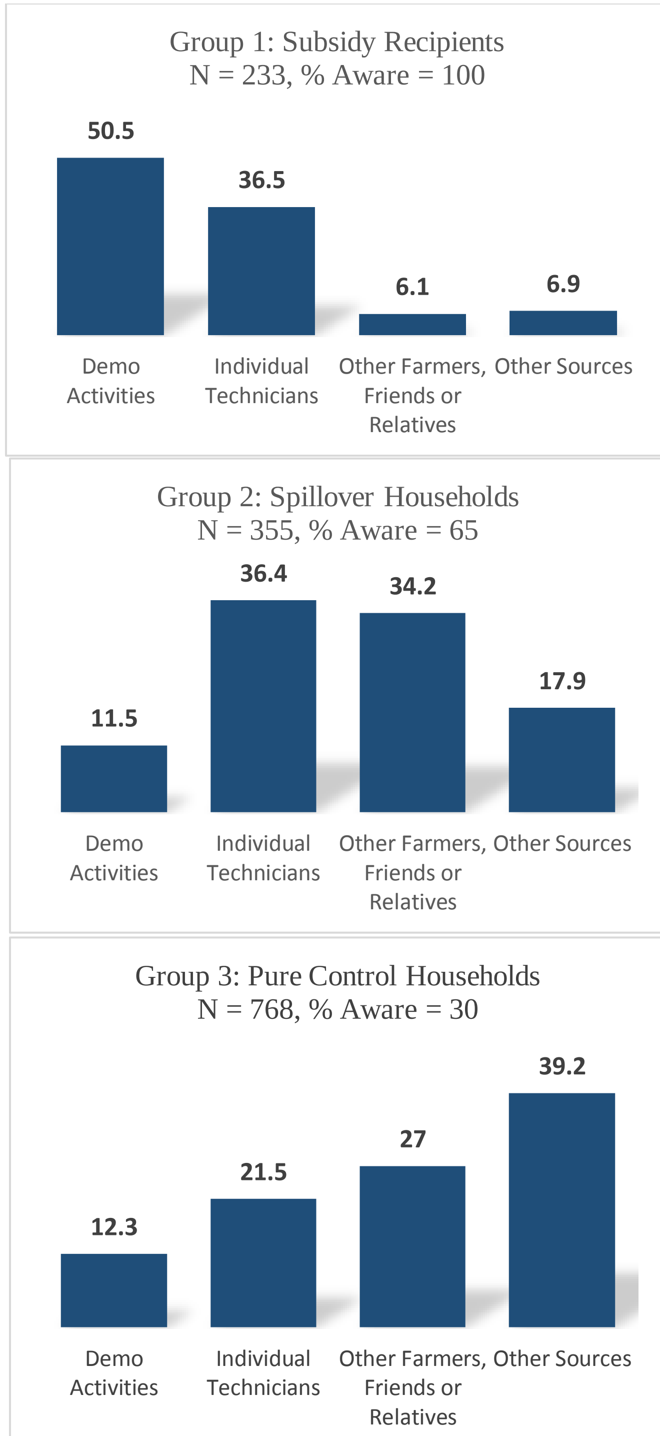

Figure 3: Percentage sources of information about hermetic bags by experimental group

Source: Author's compilation from 2016 follow-up survey
Notes: Each household may list up to two sources of information
Other sources include awareness through Radio/Tv, other NGOs working in the area, market, input dealers and some unrelated government extension officers.

Table 1: Baseline characteristics and balance check

	Control		Treated		
	Mean	SD	OLS Coeff.	p-value	N
Variables	(1)	(2)	(3)	(4)	(5)
Outcome Variable(s)					
=1 if HH bought hermetic bag (adopter) at baseline	0.003	0.058	0.001	0.319	1186
=1 if HH is aware of hermetic bag at baseline	0.001	0.038	-0.001	0.329	1186
Household Characteristics					
Age of household head (years)	45.49	14.54	0.32	0.784	1192
Household size	6.45	3.21	0.165	0.524	1192
=1 if female-headed household	0.18	0.38	-0.005	0.854	1192
=1 if Polygamous	0.16	0.38	0.018	0.542	1192
=1 if HH head has any form of education	0.89	0.32	-0.007	0.730	1192
Total household revenue ('000 UGX)[a]	2280	5091	-14	0.974	1190
=1 if HH has radio	0.77	0.42	-0.008	0.802	1188
=1 if HH has mobile phone	0.70	0.46	-0.017	0.713	1188
=1 if HH has a bicycle	0.60	0.49	-0.041	0.258	1188
Production and PH practices					
Total maize area (ha.)	0.52	0.44	-0.001	0.989	1115
Total quantity harvested-maize (kg)	861	11263	51	0.603	1115
Total quantity of maize stored (kg)	598	1119	67	0.421	1186
Expected postharvest loss (%)	4.13	8.52	-0.715	0.171	1069
=1 if Traditional storage technology use	0.85	0.37	-0.000	0.999	1186
=1 if hermetic storage technology use	0.005	0.07	-0.005	0.163	1186
Region Effects					
=1 Eastern region	0.25	0.43	-0.003	0.184	1190
=1 Northern region	0.25	0.43	0.000	1.000	1190
=1 Western region	0.25	0.43	0.000	1.000	1190
=1 Central w/o Kampala region	0.25	0.43	0.003	0.374	1190

Notes: Columns 1 and 2 report means and standard deviations for control villages in the baseline. Columns 3 through 5 report results from an OLS regression comparing households in treated and control villages in the baseline controlling for region effects and clustering standard errors at the village level. Columns 3 and 4 report the OLS coefficient and p-value corresponding to the treatment dummy and column 5 reports the sample size for each regression.

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

[a] 1USD = 2800 UGX at baseline.

Table 2: Direct effects of subsidy on market participation

VARIABLES	(1) SMD	(2) SMD	(3) DiD	(4) DiD	(5) FE	(6) FE
Sub (τ_{SMD},τ_{FE})	0.054**	0.052**	0.001	0.001	0.049**	0.049**
	(0.022)	(0.022)	(0.001)	(0.001)	(0.022)	(0.022)
Sub*post (τ_{DiD})			0.052**	0.052**		
			(0.021)	(0.021)		
=1 if observation is post-intervention			0.051***	0.051***		
			(0.011)	(0.011)		
Age of household head		0.000		0.000		-0.000
		(0.001)		(0.000)		(0.001)
=1 if HH head is educated		0.005		0.005		-0.021
		(0.029)		(0.014)		(0.037)
Household size		0.005		0.003		0.007*
		(0.006)		(0.003)		(0.004)
=1 if female headed household		-0.024		-0.013		-0.006
		(0.030)		(0.015)		(0.016)
=1 if Eastern region	0.007	-0.001	0.003	-0.001		
	(0.032)	(0.031)	(0.016)	(0.016)		
=1 if Western region	-0.000	-0.003	0.007	0.005		
	(0.034)	(0.033)	(0.021)	(0.021)		
=1 if Northern region	0.024	0.019	0.012	0.009		
	(0.033)	(0.038)	(0.017)	(0.019)		
Season binary indicators?	Yes	Yes	Yes	Yes	Yes	Yes
Constant	0.046**	0.010	-0.003	-0.020	0.002	-0.026
	(0.022)	(0.053)	(0.010)	(0.025)	(0.006)	(0.072)
Observations	1,176	1,176	2,362	2,362	2,362	2,362
R-squared	0.011	0.017	0.044	0.047	0.079	0.084
Number of HH_ID					619	619

Notes: the highlighted rows (1-2) show the ITT estimates from different estimators. 'Sub' (row 1) highlighted in columns (1) and (2) are the simple mean difference estimates; 'Sub*post' (row 2) highlighted in columns (3) and (4) are the DiD estimates; and 'Sub' (row 1) highlighted in columns (5) and (6) are the treatment effect estimates from the FE estimator. Robust standard errors, clustered at the LC1 level, are shown in parentheses.

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

Table 3: Spillover effects of subsidy on market participation

VARIABLES	(1) SMD	(2) SMD	(3) DiD	(4) DiD	(5) FE	(6) FE
Spillover (γ_{SMD},γ_{FE})	0.035***	0.034***		0.003	0.033**	0.033**
	(0.013)	(0.012)		(0.003)	(0.013)	(0.013)
Spillover*post (γ_{DiD})			0.032**	0.032**		
			(0.013)	(0.013)		
=1 if data is post-intervention			0.019***	0.020***		
			(0.007)	(0.007)		
Age of household head		-0.000		-0.000*		-0.000*
		(0.000)		(0.000)		(0.000)
=1 if HH head is educated		0.011		0.007		-0.001
		(0.011)		(0.006)		(0.007)
Household size		0.004*		0.002*		0.002*
		(0.002)		(0.001)		(0.001)
=1 if female headed household		0.012		0.007		0.002
		(0.012)		(0.007)		(0.005)
=1 if Eastern region	0.019	0.015	0.010	0.008		
	(0.014)	(0.012)	(0.008)	(0.006)		
=1 if Western region	0.019	0.015	0.012	0.010		
	(0.015)	(0.014)	(0.008)	(0.008)		
=1 if Northern region	0.024*	0.020	0.013*	0.010		
	(0.014)	(0.014)	(0.007)	(0.008)		
Season binary indicators?	Yes	Yes	Yes	Yes	Yes	Yes
Constant	0.003	-0.009	-0.009**	-0.016	-0.000	0.001
	(0.007)	(0.026)	(0.004)	(0.013)	(0.001)	(0.013)
Observations	2,246	2,246	4,124	4,124	4,124	4,124
R-squared	0.011	0.019	0.022	0.027	0.021	0.026
Number of HH_ID					1,179	1,179

Notes: the highlighted rows (1-2) show the spillover effect estimates from different estimators. 'Spillover' (row 1) highlighted in columns (1) and (2) are the simple mean difference estimates; 'Spillover*post' (row 2) highlighted in columns (3) and (4) are the DiD estimates; and 'Spillover' (row 1) highlighted in columns (5) and (6) are the spillover effect estimates from the FE estimator. Robust standard errors, clustered at the LC1 level, are shown in parentheses.

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

Table 4: Subsidy and spillover effects from joint estimates

VARIABLES	(1) SMD	(2) SMD	(3) DiD	(4) DiD	(5) FE	(6) FE
Sub (τ_{SMD},τ_{FE})	0.053**	0.053**	0.001	0.001	0.049**	0.049**
	(0.022)	(0.022)	(0.001)	(0.001)	(0.022)	(0.022)
Sub*post-int. (τ_{DiD})			0.052**	0.052**		
			(0.021)	(0.021)		
Demo village (γ_{SMD},γ_{FE})	0.034**	0.034***	0.003	0.003	0.033**	0.033**
	(0.013)	(0.013)	(0.003)	(0.003)	(0.015)	(0.015)
Demo village*post-int. (γ_{DiD})			0.031**	0.031**		
			(0.013)	(0.013)		
=1 if observation is post-intervention			0.019***	0.020***		
			(0.007)	(0.007)		
Age of household head		-0.000		-0.000		-0.000
		(0.000)		(0.000)		(0.000)
=1 if HH head is educated		-0.001		0.001		-0.021
		(0.013)		(0.007)		(0.020)
Household size		0.003		0.002		0.004
		(0.002)		(0.001)		(0.002)
=1 if female headed household		-0.004		-0.002		-0.005
		(0.013)		(0.007)		(0.008)
=1 if Eastern region	0.011	0.008	0.006	0.003		
	(0.015)	(0.014)	(0.008)	(0.008)		
=1 if Western region	0.018	0.015	0.013	0.011		
	(0.018)	(0.018)	(0.011)	(0.011)		
=1 if Northern region	0.026	0.022	0.014	0.011		
	(0.017)	(0.018)	(0.009)	(0.010)		
Season binary indicators?	Yes	Yes	Yes	Yes	Yes	Yes
Constant	0.006	-0.001	-0.008*	-0.012	0.000	0.009
	(0.010)	(0.025)	(0.005)	(0.013)	(0.004)	(0.028)
Observations	2,712	2,712	5,068	5,068	5,068	5,068
R-squared	0.027	0.031	0.044	0.046	0.066	0.069
Number of HH_ID					1,419	1,419

Notes: the highlighted rows (1-2) show the ITT estimates from different estimators. 'Sub' (row 1) highlighted in columns (1) and (2) are the simple mean difference estimates; 'Sub*post' (row 2) highlighted in columns (3) and (4) are the DiD estimates; and 'Sub' (row 1) highlighted in columns (5) and (6) are the treatment effect estimates from the FE estimator. Robust standard errors, clustered at the LC1 level, are shown in parentheses. The spillover effects are similarly highlighted in rows (2-4).

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

Appendix A

Table A1: Mean difference between returning and attritted households

	Returning		Attritted		
	Mean	SD	Coeff.	p-value	N
Variables	(1)	(2)	(3)	(4)	(5)
Outcome Variable(s)					
=1 if HH bought hermetic bag (adopter) at baseline	0.004	0.06	-0.002	0.352	1186
=1 if HH is aware of hermetic bag at baseline	0.001	0.030	-0.001	0.304	1186
Household Characteristics					
Age of household head (years)	45.80	14.65	-6.85***	0.015	1192
Household size	6.50	3.24	-1.28**	0.026	1192
=1 if female-headed household	0.17	0.38	0.066	0.286	1192
=1 if Polygamous	0.16	0.37	0.006	0.934	1192
=1 if HH head has any form of education	0.89	0.32	-0.009	0.873	1192
Total household revenue (‘000 UGX)	2303	5200	-609	428	0.168
=1 if HH has radio	0.78	0.42	-0.027	0.750	1188
=1 if HH has mobile phone	0.70	0.46	-0.063	0.652	1188
=1 if HH has a bicycle	0.60	0.49	-0.054	0.485	1188
Production and PH practices					
Total maize area (ha.)	0.51	0.44	0.097	0.443	1115
Total quantity harvested-maize (kg)	854	1241	182	0.601	1115
Total quantity of maize stored (kg)	591	1091	185	0.572	1186
Expected postharvest loss (%)	4.12	8.50	0.206	0.840	1069
=1 if Traditional storage technology use	0.83	0.38	0.039	0.463	1186
=1 if other improved storage tech. use	0.010	0.098	-0.011**	0.023	1186
=1 if hermetic storage technology use	0.004	0.06	0.013	0.362	1186
Region Effects					
=1 Eastern region	0.25	0.43	0.036	0.776	1192
=1 Northern region	0.25	0.43	0.075	0.497	1192
=1 Western region	0.26	0.44	-0.150*	0.079	1192
=1 Central w/o Kampala region	0.25	0.43	0.039	0.594	1192

Notes: Columns 1 and 2 report means and standard deviations for control villages in the baseline. Columns 3 through 5 report results from an OLS regression comparing households in treated and control villages in the baseline controlling for region effects and clustering standard errors at the village level. Columns 3 and 4 report the OLS coefficient and p-value corresponding to the treatment dummy and column 5 reports the sample size for each regression. *** p<0.01, ** p<0.05, * p<0.1

Table A2: Survey evidence showing positive learning experience

Learning experience questions	Response	Percentage
Panel A		
Does household consider hermetic storage bag effective?	Yes No	94 6
Panel B		
Preference for hermetic storage bag vs. storage chemical use.	Hermetic Chemical	95 5
Panel C		
Perception about ease of use of hermetic bag relative to other types of technologies used.	Very easy Easy Same level Difficult Very difficult	48 40 10 2 0
Panel D		
Grain quality stored in hermetic bag relative to other types of storage technologies used.	Better Indifferent Worse Others	61 12 0 27
Panel E		
Grain taste when stored in hermetic bags relative to other storage technologies used.	Better Indifferent Worse Others	44 20 0 36

Source: Author's compilation from 2016 follow-up survey

Table A3: Baseline statistics and randomization balance check between the spillover group in the treatment villages vs the pure control group in the control villages

	Control		Treated		
	Mean	SD	Coeff.	p-value	N
Variables	(1)	(2)	(3)	(4)	(5)
Outcome Variable(s)					
=1 if HH bought hermetic bag (adopter) at baseline	0.001	0.033	0.003	0.305	1878
=1 if HH is aware of hermetic bags at baseline	0.002	0.041	-0.000	0.869	1878
Household Characteristics					
Age of household head (years)	43.97	14.63	1.40	0.258	1900
Household size	6.33	2.88	0.050	0.831	1900
=1 if female-headed household	0.15	0.36	0.032	0.274	1900
=1 if Polygamous	0.17	0.38	-0.018	0.528	1900
=1 if HH head has any form of education	0.88	0.32	0.008	0.764	1900
Total household revenue (‘000 UGX)	2272	4275	6.57	0.984	1888
=1 if HH has radio	0.78	0.42	-0.001	0.982	1892
=1 if HH has mobile phone	0.68	0.47	0.030	0.478	1892
=1 if HH has a bicycle	0.63	0.49	-0.017	0.712	1892
Production and PH practices					
Total maize area (ha.)	0.54	0.53	-0.021	0.680	1787
Total quantity harvested-maize (kg)	980	1338	-134	0.240	1787
Total quantity of maize stored (kg)	627	1063	-55	0.528	1886
Expected postharvest loss (%)	4.54	8.55	-0.083	0.878	1703
=1 if Traditional storage technology use	0.83	0.37	0.006	0.846	1886
=1 if hermetic storage technology use	0.01	0.10	-0.004	0.338	1886
Region Effects					
=1 Eastern region	0.25	0.43	0.000	0.998	1900
=1 Northern region	0.25	0.43	-0.001	0.995	1900
=1 Western region	0.25	0.43	0.001	0.995	1900
=1 Central w/o Kampala region	0.25	0.43	0.000	0.998	1900

Notes: Columns 1 and 2 report means and standard deviations for control villages in the baseline. Columns 3 through 5 report results from an OLS regression comparing households in treated and control villages in the baseline controlling for region effects and clustering standard errors at the village level. Columns 3 and 4 report the OLS coefficient and p-value corresponding to the treatment dummy and column 5 reports the sample size for each regression. *** p<0.01, ** p<0.05, * p<0.1