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Spoiler alert! Spillovers in the context of a video intervention to maintain seed quality among Ugandan potato farmers

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Spoiler alert! Spillovers in the context of a video intervention to maintain seed quality among Ugandan potato farmers

Senne Vandevelde*, Bjorn Van Campenhout†, Wilberforce Walukano‡

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Abstract

Pervasive use of poor quality seed remains an important reason for low yields throughout the developing world. We explore cost-effective ways to increase the quality of the local stock of seed tubers among a sample of Ugandan potato farmers. We do this by providing agricultural extension information on (i) how to select the best seed tubers and (ii) how to properly handle and store seed tubers until the next planting season. The information is conveyed in the form of engaging videos, shown to individual farmers on mobile devices. The relative effectiveness of the information interventions is tested using an individually randomized controlled trial with a 2x2 factorial design. However, such interventions are prone to spillovers, and there are indications that control farmers might have learned about seed selection, storage and handling through their proximity to, or contacts with, farmers in the treatment group. Therefore, we explicitly model spillovers ex post using a randomization-based framework and use both farmers' GPS locations as well as survey data on actual interactions between treatment and control farmers to define the social networks through which information travels. After accounting for spillovers, we find evidence that especially the video containing information on seed selection translated into a higher awareness and adoption of recommended practices, a higher probability of using improved inputs as well as higher consumption.

Keywords: Video, extension, potato, Uganda, spillovers, seed quality

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1 Introduction

Throughout the developing world, the majority of smallholder farmers rely on informal seed systems (McGuire and Sperling, 2016). Seed recycling, where part of the previous harvest is used as seeding material for the next season, is often the norm. Over time, successive cycles of vegetative propagation is likely to reduce seed quality, resulting in lower yields. The effect of recycling on quality and yield varies by crop. For example, seed degeneration due to the accumulation of pathogens and pests in planting material has been a long-standing production challenge for Irish potato (*solanum tuberosum*) growers around the world (Thomas-Sharma et al., 2016). In developed countries, the frequent use of clean planting material derived from foundation seed safeguards quality. While guaranteeing access to quality seed should remain a key policy priority in developing countries, institutional constraints often prevent formal seed systems from having a significant impact in the short to medium term. For instance, in Uganda, a recent study established that certified maize seed obtained from the market contains less than 50% authentic seeds (Bold et al., 2017). The time and effort it takes to produce high-quality foundation seed also depends on the crop, which is particularly salient in the case of potato as a single potato plant only produces a limited number of tubers.

We argue that, in the presence of institutional constraints, policies and programs aimed at improving the practice of seed recycling are meaningful. Hence, in this study, we investigate the effectiveness of an information campaign to improve the quality of seed tubers among potato growers in Uganda through two farm level interventions: one aimed at improving seed selection and another at improving seed storage and handling. The causal effect of both interventions is then established through a field experiment implemented in southwestern Uganda.

Our study also explores the potential for ICT-mediated video in agricultural extension information campaigns targeted at individuals, rather than groups. Audiovisual messages have become common in agricultural extension as a tool to demonstrate improved farming practices to potentially illiterate farmers. One popular application of videos are so-called Video Viewing Clubs (VVCs) where farmers are brought together to watch instructional videos and participate in facilitator-led discussions afterwards (David and Asamoah, 2011). In recent years, for instance, a non-governmental organization called Digital Green facilitated the production and dissemination of more than 5,000 locally relevant agricultural extension videos in more than 50 languages (Bernard et al., 2015; Gandhi et al., 2009; Van Mele et al., 2010; Zossou et al., 2009). However, in general, video plays only an ancillary role and extension advisory services are still heavily skewed to the traditional teacher student model. With smartphones and other viewing devices becoming more widespread (penetration of smartphones is predicted to reach 57% by 2020 in Sub-Saharan Africa), the potential for videos to be used as a stand-alone tool in agricultural extension is increasing rapidly. In this paper, we mimic the conditions in which farmers would watch a video privately in their home on their smartphone, tablet or computer (as if they would watch a video on YouTube, for example). This allows us to provide evidence on the potential impact of independent video messages targeting individual farmers as a possible complement to existing extension tools with respect to a range of outcomes like adoption of improved agricultural practices, yield and ultimately, welfare.

Finally, our paper also contributes to the growing literature on interference in agricultural experiments and experimental design more broadly. Randomized controlled trials (RCTs) are ubiquitous, especially in development and agricultural economics. A common problem with many experimental settings is interference (or spillovers), the fact that a given subject's outcomes are not only influenced by its own treatment but also by the treatment in its social or geographical surroundings (Cox, 1958). If there is interference, the stable unit treatment value assumption (SUTVA) is violated and simple comparisons between treatment

and control will be biased (Rubin, 1990). Understanding how treatment administration influences untreated subjects is important in the evalutation of many different programs such as the provision of cash transfers (Barrera-Osorio et al., 2011), learning about a new technology (Conley and Udry, 2010) and public health programs (Miguel and Kremer, 2004). The issue of interference and social networks is particularly salient in information campaigns aimed at stimulating technology adoption, as the inherent intangible nature of the provided treatment makes it hard to track the flow and the direction of spillovers (Bandiera and Rasul, 2006; Conley and Udry, 2010). Some experiments try to account for possible interference in the design phase of the experiment. Examples include partial population experiments (Duflo and Saez, 2003) and randomization saturation designs (see Hudgens and Halloran, 2008 and, more recently, Baird et al., 2018). Instead of accounting for spillovers in the design phase of our experiment, we opt to analyze spillovers ex post. More specifically, we implement a methodology developed by Aronow and Samii (2017), which makes use of the randomized nature of experiments to be able to account for spillovers in the analysis phase. The methodology offers researchers an option to conduct spillover analyses in cases where spillovers are unexpected (as is the case in this study) and not an intentional design choice. To the best of our knowledge, we are among the first to operationalize this methodology in the context of agricultural field experiments.

When spillovers are not taken into account, we find no differences between treatment and control for either video on outcomes at different levels, such as the awareness and adoption of practices, inputs used, yield or welfare even though the vast majority of treatment farmers report positive experiences with both videos. However, looking at the information flows between farmers in the sample, we do find important evidence of interference between treatment and control. Hence, when spillovers are modeled explicitly using both survey network data as well as farmers' locations, the results change substantially, with especially the seed selection video leading to the adoption of recommended practices, a higher use of improved inputs and higher welfare. The lack of positive results emanating from the seed storage video is explained through the precedence of cash and credit constraints.

The remainder of the paper is organized as follows: in Section 2, the setting of this experiment is motivated and explained. We also focus on the content and the implementation of the two video treatments. Section 3 then outlines the design of the experiment, the data collection process and the considered outcomes. It also pays considerable attention to the methodology used to model spillovers. Section 4 focuses on the results of this paper. We first look at farmers' experiences with the videos after which we show the results respectively without and with taking spillovers into account. After a broad overview of the results, we unpack the different possible channels through which these might have materialized. We further provide evidence on the accuracy of our spillover specification and the robustness of our results. Section 5 provides a summary of the main results and speculates about what they could mean for agricultural research and decision-making in developing countries.

2 Setting and Treatments

We test the effectiveness of audiovisual information campaigns aimed at improving seed quality among a sample of potato farmers in Uganda. In Uganda, Irish potatoes (*solanum tuberosum*) are important both as a cash crop and for home consumption (FAO, 2015), but they are particularly vulnerable to seed degeneration. At the same time, seed potato breeding is a slow process, and the government organization responsible for the production and distribution of foundation seed to private seed multipliers, the Kachwekano Zonal Research and Development Institute (KAZARDI), is struggling with severe capacity limitations.

Our study population consists of potato farmers in the southwestern Kigezi region in Uganda, which accounts for about 47 percent of total potato production in the country (UBOS, 2010). Baseline data collected from 500 potato farmers in 2013/14 (with the help of the Uganda Bureau Of Statistics, UBOS) reveals that the majority of farmers in the area are smallholders with an average area allocated to potatoes of about 0.5 acres.[1] Many farmers use fertilizer (40%) and hire in labor (35%). Median yields in the area amount to about 3 MT per hectare, considerably less than what can be achieved in these agro-ecological conditions (25 MT per hectare according to Fermont and Benson, 2011). Seed recycling as a practice is widespread in the area: on average (across two agricultural seasons) 60 percent of farmers report using part of their own harvest as seeding material for the next season. Further, Figure 1 shows that seed quality is indeed an important constraint in the context of our study. It demonstrates that the most successful farmers in the sample (at least, in terms of yield) are those that use high quality propagation (seeding) material. Applying fertilizer, on the other hand, does not seem to make a big difference in the study context.

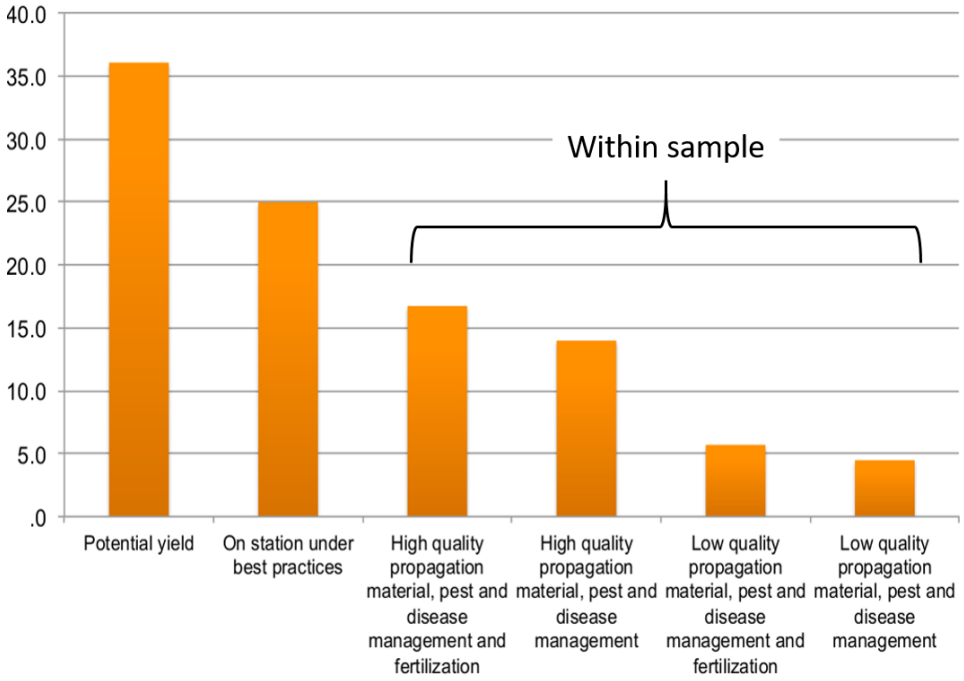


Figure 1: Yields (in MT/hectare) obtained under different conditions (source: Van Campenhout et al., 2016)

After extensive consultations with local farmers, extension agents, potato experts (including at IFDC and CIP) and local officials, it was decided to focus on simple, low-cost, but potentially effective messages that could be conveyed using video and that could be implemented by even the most constrained farmers. Two videos were produced, corresponding to two important, but clearly separate issues that each have an impact on the quality of recycled seed.

A first video promotes Positive Seed Selection (henceforth referred to as the PSS treatment) as a response to the observed practice where farmers consume or sell the best tubers first and as a consequence are left with inferior seeding material for the next planting season. PSS denotes careful selection of only the best potatoes as seed for the subsequent season. This process of PSS already starts during the growing stage of

[1] This sample was obtained using a two-stage sampling procedure. In the first stage, Enumeration Areas (EAs) were selected proportional to the size of the population in different villages. In the second stage, potato farming household were selected randomly within the selected EAs.

the seed potato in the season prior to the season when the seed tuber will be used to produce potatoes. At this stage, the most virile plants should be earmarked for the production of seed tubers. Therefore, a first key message in the PSS video is that, during the growing season, potato plants that show the least evidence of disease (i.e., the tallest plants that do not have curled, yellow, or deformed leaves) should be pegged (with a wooden stick for example), and farmers are encouraged to keep checking these plants thoroughly for signs of disease up until the moment of harvest. A second key message is related to the actual selection of the tubers: at the time of harvest, farmers are advised to harvest the pegged plants first and keep only potatoes that have roughly the size of an egg, exhibit no signs of disease, and have at least four eyes (which offers an indication of the number of stems that will sprout from it).

Several (agronomic) studies have already demonstrated the effectiveness of PSS. For example, Kakuhenzire et al. (2012) find large effects of PSS among potato farmers in both Kenya and Uganda. Gildemacher et al. (2011) show that in on-farm trials in Kenya, PSS increases yields by 34 percent. In follow-up research, Schulte-Geldermann et al. (2012) show that positive selection reduces the incidence of several viruses that affect potato seeds. However, most of these studies are largely agronomic in nature. In this study, we want to go beyond the boundaries of the trial field and also focus on the adoption of the methodology in a more realistic setting.

The second treatment video promotes Proper Seed Storage and Handling (henceforth referred to as the PSSH treatment), focusing on what happens with seed potatoes between the moment of harvesting and planting. Key informant interviews show that farmers tend to store seed in bags in a dark corner inside the house without proper ventilation, increasing the likelihood of tuber rot and dormancy. The second video thus contains several key messages to improve storage and handling of seed tubers. First, seed potatoes should be spread out on racks, or on dried grass on the floor. Second, potatoes should be kept in a separate store or room, away from other crops and animals. Third, potatoes are to be stored in well ventilated and diffuse lighting conditions. And finally, clean materials should be used when handling seed potatoes to reduce contamination.

Less is known about the impact of Proper Seed Storage and Handling on potato yields and farmer well-being, probably because it is less well defined and delineated than PSS. There is some evidence that storage time and temperature are important for yields (Loon, 1987). Gachango et al. (2008) recommend storing seed potatoes in diffuse light in order to get short and strong sprouts, but their study stops short of investigating yield differences related to differing light intensity exposure. In our study, we will test how recommending a set of storage and handling practices will affect a range of different outcomes.

The video messages feature successful model farmers who followed a fixed script. They refer to their own experiences and talk about how using PSS or PSSH has allowed them to become successful potato farmers. The use of a role model in conveying information has been shown to be effective in many different contexts (Bernard et al., 2015; Riley, 2017; Rogers, 2003). Videos are produced in two local languages (Rukiga and Rufumbira) and are subtitled in English. Both videos are about 7 minutes long.[2] They were shown to individual farmers by an enumerator using Android tablets at the beginning of May 2016, but no information was given apart from asking for their consent.[3] After having seen the video, the farmer was asked to answer 6 multiple-choice questions (two on PSS, two on PSSH and two general questions on potato farming). These were used to gauge potential immediate increases in awareness as a result of the video, the results of which

[2]The PSS video can be viewed here: https://www.youtube.com/watch?v=JcacGR0EU94, while the PSSH video can be viewed here: https://www.youtube.com/watch?v=Je8qLqH5vLY.

[3]Consent was obtained orally and marked digitally on the tablet by the enumerator in order to avoid stigmatization on the part of illiterate farmers. This procedure was approved by IFPRI's Institutional Review Board (IRB) on February 2nd, 2016 (IRB Approval Number 2016-12-DSGD-M).

are reported in Van Campenhout et al. (2017). Everything was done to mimic the conditions of the farmer privately watching the video as closely as possible, thus avoiding the potential group dynamics that would make evaluation of the experiment difficult.

3 Data and Methodology

3.1 Experimental design

From the 500 farmers for which baseline data was collected in 2013/14, 248 potato farmers are randomly selected to take part in this study. The 248 farmers are allocated randomly to one of four treatment conditions in a randomized controlled trial with a 2x2 full factorial design: 62 farmers only watched the PSS video, 62 farmers only saw the PSSH video, 62 farmers watched both videos while another 62 farmers did not see any video. The experiment is designed to only have power for testing main effects. In other words, in what follows, comparisons are made between half of the sample (or 124 farmers) that have watched one of the two videos (either the PSS video or the PSSH video) and the other half of the sample that has not watched that particular video.

Prior to random assignment to the four treatment cells of the 2x2 design, farmers were stratified into groups of four through an ex-ante matching procedure. Matching was performed by minimizing the euclidean distance between a set of ten baseline characteristics, including household size, age and gender of the household head, area of potatoes grown, the logarithm of potato yields and logarithm of welfare per capita. We thus end up with 62 groups of 4 matched farmers. Within each group, each farmer is then randomly assigned to one of the four treatment cells (pure control, PSS, PSSH and PSS+PSSH). Such a procedure has the benefit of increasing statistical power, especially in small samples (Bruhn and McKenzie, 2009).[4]

3.2 Data

To measure the relative effectiveness of the PSS and the PSSH video treatments, all 248 farmers in the sample were re-visited one year (and two agricultural seasons) after having been shown the videos (around the end of April 2017). This paper focuses on outcomes as measured during that survey. Attrition is an issue in our sample, but not because respondents could not be located or had moved. In the year between the video treatment and the survey, 35 farmers (or about 15 percent of the sample) did not plant potatoes, generally due to poor rains. This means that for these farmers, we do not have data on outcomes related to potato farming, but only on knowledge and questions about e.g., the appreciation of the video. One concern related to attrition is that the lost farmers would in some way be different from the remaining farmers, which could potentially mean we lose orthogonality, which was guaranteed by our group-wise matching strategy. For this reason, a balancing test of the different treatments taking attrition into account is conducted, the results of which can be found in Table 1.[5] On the whole, it seems that balance between treatment and control (for both PSS and PSSH) is preserved, even after attrition.

[4] More information on the sampling, matching procedure, balancing tests and power calculations can be found in this study's pre-analysis plan which is registered at the American Economic Association's registry for randomized controlled trials (https://www.socialscienceregistry.org/trials/1014).

[5] In general, in matched designs where attrition is an issue, balance is preserved by dropping entire groups if one individual of the group is lost. However, in our case, this would reduce the already limited sample size even further, hence the (re-)balancing test.

Table 1: Descriptive statistics and orthogonality after attrition.

Variable	Mean	PSS	PSSH
Household size	5.832	0.042	0.375
		(0.336)	(0.336)
Age of head (years)	44.059	3.009	0.432
		(2.200)	(2.201)
Male head (0/1)	0.855	-0.018	-0.024
		(0.052)	(0.052)
Head can read and write (0/1)	0.757	-0.005	-0.004
		(0.060)	(0.060)
Farming experience (years)	19.619	3.927*	1.094
		(2.077)	(2.077)
Welfare (log consumption)	14.075	-0.243	0.104
		(0.159)	(0.159)
Seed recycling (0/1)	0.637	-0.038	0.077
		(0.047)	(0.047)
Fertilizer use (0/1)	0.411	0.030	0.099
		(0.069)	(0.069)
Received training (0/1)	0.441	0.008	0.004
		(0.069)	(0.069)
Received credit (0/1)	0.834	-0.117*	-0.043
		(0.060)	(0.060)
Closest input provider (km)	7.248	-1.108	-0.432
		(1.180)	(1.181)
Nearest market (km)	6.144	0.399	-0.266
		(1.000)	(1.001)
Time to reach parcel (min)	40.223	-3.063	-4.282*
		(2.543)	(2.537)
Harvest sold as ware (%)	0.354	-0.012	0.067*
		(0.038)	(0.038)
Harvest kept as seed (%)	0.222	-0.009	0.011
		(0.019)	(0.019)
Potato production (kg)	816.010	2.186	127.793
		(121.380)	(121.456)

Notes: Standard errors in parentheses. Number of observations=211. Baseline data collected in 2013/2014. *, **, *** denote significance at the 10, 5 and 1 percent level respectively. A joint orthogonality test was also conducted, with F-test p-value of 0.559 .

The starting point of this paper is the information effect identified immediately after the showing of the video, as described in Van Campenhout et al. (2017). The goal of this study is then to establish whether this effect persists one year on and whether it translates into increased welfare for the farmer's household. The pathways through which such a change in welfare might materialize, are numerous and difficult to predict ex ante. Nevertheless, here, it is hypothesized that watching a video could directly trigger (a combination of) two effects: a (sustained) information effect on the one hand and a motivational effect on the other hand. Both effects might in turn engender a set of intermediate outcomes, such as adoption of improved farming practices, adoption of improved inputs or intensification of available farm land. Finally, those intermediate outcomes could then lead to improvements in final outcomes such as yield or selling more on the market, both of which could lead to higher farmer welfare. This theory of change is summarized using Figure 2. Below, each category of outcomes and the method of measuring them is briefly described.

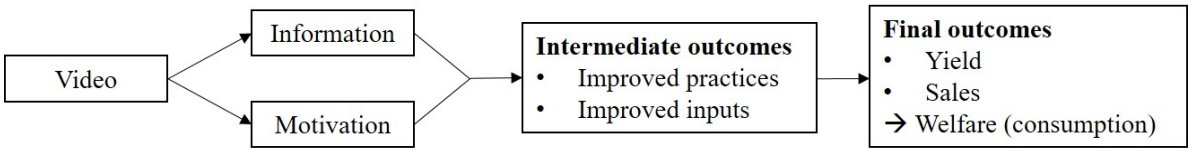


Figure 2: Theory of Change

The transmission of information is measured in the same way as in Van Campenhout et al. (2017). The same six multiple-choice questions that were asked immediately after the showing of the video (or without having seen the video in the case of pure control farmers) are submitted again to the farmer during the endline survey. Three extra questions are added to the six existing ones (one on PSS, one on PSSH and one general question related to potato farming). This is done to be able to measure farmers' learning effect from the video rather than from the quiz questions they received one year prior to the endline survey. Additionally, the order of the questions is randomized to prevent survey priming. The full list of questions, together with their multiple-choice answer possibilities can be found in Appendix I.

Noncognitive skills of farmers in developing countries or in this case, their motivation, are notoriously difficult to measure (Laajaj and Macours, 2017). This could be due to a lack of understanding on the part of the farmer or due to a lack of cultural appropriateness of the questions themselves. For that reason, it is impossible to know for sure that what is being measured amounts to anything more than just measurement error. As such, it is decided to not include any motivation questions in the analysis (even though some are asked in the survey). This does not mean, however, that it should be disregarded as a potential (direct) pathway.

The next set of outcomes that are considered in this study are intermediate outcomes which might be a result of increases in awareness about farming practices or of a higher motivation. A first logical consequence of increases in awareness about farming practices is the actual adoption of those practices. In line with the two treatments, farmers are asked to report which techniques they use with regards to 1) seed selection and 2) seed storage. In particular, enumerators go over 12 practices on a list (six for each treatment, see Appendix II) and ask whether or not each of them is applied by the farmer (i.e., in the form of aided questioning). Other intermediate results of increases in information awareness and/or increases in motivation could be the adoption of better inputs or crowding-in of other investments. To measure this, farmers are asked to report both the use of chemicals, fertilizer, seed and hired labor as well as how it was acquired.[6] For instance, the videos might make farmers more aware of the importance of good quality seed which might induce them (paradoxically) to buy their seed potatoes from a recognized seed seller. Similarly, increased motivation through the video could lead to the farmers applying more labor (their own or hired) on their plots.

Finally, these intermediate outcomes might in turn engender outcomes which have a direct impact on farmers' (and their households') welfare. First, improved practices, the use of improved inputs or the intensification of existing methods should in theory lead to higher yields, calculated using the farmer's self-reported production and plot size. A second final outcome that is considered in this paper is the farmer's attitude towards selling potatoes on the market. This could be a result of increased yields, but also more directly through exposure to the example of a successful farmer who approaches potato farming as a business in the videos. Finally, the ultimate question this paper is trying to answer is whether agricultural extension videos

[6] All data related to inputs, yields, adoption of improved practices are collected at the plot level for the second season of 2016 (which is the season ending around November/December). More specifically, farmers are asked to list all plots on which potatoes were grown during that season. One plot is randomly picked from that list (generated by the survey program itself). As a result, all reported results relate to one random plot per farmer which on average should generate unbiased results.

have the potential to increase farmers' welfare. Welfare in this study is measured by adding up the value of a household's consumption (including from own production) over the week prior to the day of interview. This is in accordance with common practice in agricultural and household economics (see, among others, Deaton, 2003). The consumption basket considered consists of the most common food items in the context (vegetables, grains, fruits, potatoes, etc.) and some more general consumption items such as airtime, personal hygiene items and cooking oil.

In order to account for the issue of multiple hypothesis testing (the probability that a false rejection of the null increases as outcomes are added), the different questions for each category of outcomes (information, practices, input use and final outcomes) are also grouped together in one index. This approach, which has its origins in the biostatistics literature (O'Brien, 1984), has the added benefit of being able to show whether or not our treatments have a general effect on a set of broad categories. Following Anderson (2008), the following steps are undertaken to construct indices for each category. First, each single outcomes is standardized by demeaning its value and dividing by the standard deviation of the control group. Second, weights for each outcome are obtained using the inverted variance-covariance matrix of the standardized outcomes. Finally, indices are constructed as weighted averages of the standardized individual outcomes in each category.

3.3 Modeling spillovers

While treatments at the individual level yield highest statistical power, such designs are prone to spillover effects. A farmer that is used as a control in the PSS experiment may live close to a farmer that received the PSS treatment, and learn from observing his or her behavior. A farmer that did not receive the PSSH treatment may be friends with a farmer that did get to see the PSSH video, and learn from it through discussion. In both cases, spillover effects are likely to be positive. As the farmer in the control group also benefits from the treatment, his or her outcome variables are likely to change in the same direction as the outcomes of the treated farmer. This would result in a downward bias in the estimated average treatment effect. In this paper, we explicitly take into account the possibility of spillovers using a methodology developed by Aronow and Samii (2017).

To be able to assess how information from the videos would spread in the different villages, a separate, specific module on farmers' networks was included in the survey. More specifically, farmers were asked two questions for each other farmer in their village that was also part of the study: whether or not they knew that other person and if so, whether or not a video was discussed with that other person.[7] A first look at this data offers an indication that spillovers might indeed be an issue in the sample. Each farmer reports knowing on average six other farmers in the sample (which amounts to about 80 percent of the within-village sample) while 58 percent of respondents have discussed one of the videos with someone else (in the village). More importantly for the purposes of our analysis, about a third of treatment farmers report having discussed the video with at least one control farmer, which could have had a confounding impact on most (if not all) results.[8]

The evolution of the information effect over time also suggests that spillover effects may be playing a role in our study. In Figure 3, the proportion of correct answers for four questions in the survey (two related to seed selection and two related to seed storage) is shown both at the time when farmers saw the video

[7] Reference to the video was kept deliberately vague so as to not overly confuse the control farmers who of course had not seen any video.

[8] It should be noted that the analysis of spillovers was not an intentional design choice of this experiment and as such does not feature in the pre-analysis plan (https://www.socialscienceregistry.org/trials/1014). However, the overwhelming evidence supporting the importance of spillovers combined with the existence of a methodology that allows us to rigorously analyze them, have convinced us of the legitimacy of pursuing this in the paper.

(time 1) as well as at the time of the endline survey (time 2). Particularly for questions where there is a significant difference in knowledge immediately after having been shown the video (sel2 and store1), the difference between treatment and control reduces over time.[9] Especially information on lighting conditions (store1) seems to have spread within the communities: while immediately after showing the video, only about 60 percent of farmers in the control group knew potatoes should be stored in indirect light, this group has caught up with the control group by the time the follow-up survey was conducted (one year later). In a similar study where rice farmers are provided agricultural extension information using video, control farmers also catch up over time in terms of awareness of information about what is shown in the videos (Van Campenhout et al., 2017).

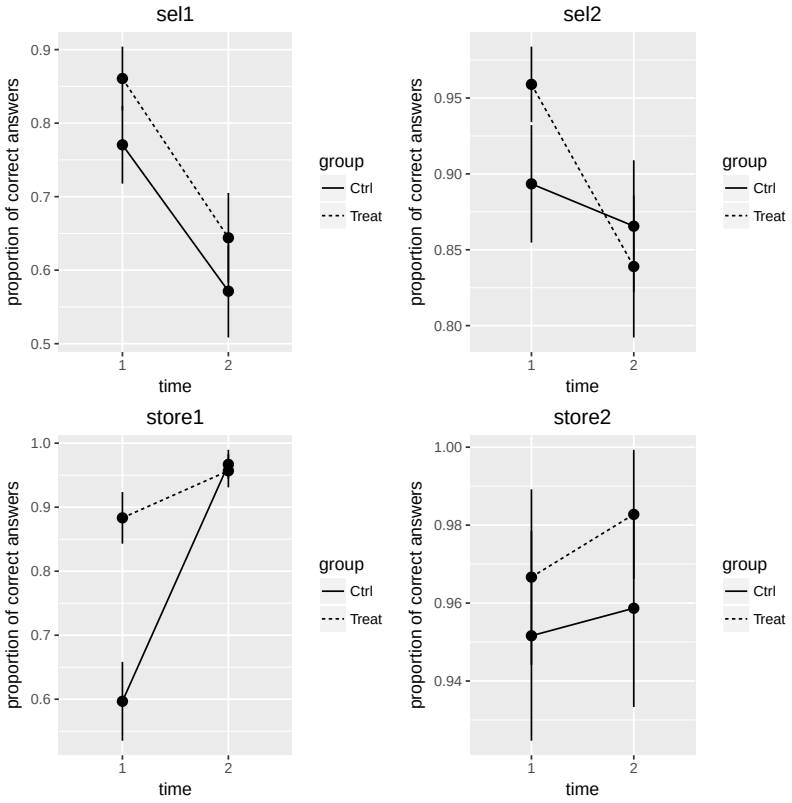


Figure 3: Evolution of information awareness over time

To formally account for the possibility of spillovers, we use a general, randomization-based framework for estimating causal effects under interference between units as proposed by Aronow and Samii (2017). The basic idea behind this methodology is that, even though the researcher has full control over the allocation of the treatment, the probability of an experimental unit being affected by another experimental unit depends on its position within a network. For example, the probability of being assigned to the control may be 50 percent in a generic randomized control trial without spillovers. However, the probability that this control unit is not contaminated will decline with the number of other treated units that are in its proximity (which could refer to physical proximity or distance within a social network, see below). Therefore, when estimating treatment effects, one needs to account for these differential probabilities of assignment to the spillover condition, for instance through inverse probability weighting.

[9] Questions and answering possibilities can be found in Appendix I.

To define proximity, we use two sources of data. Farmers' locations (using Global Positioning System (GPS) coordinates) together with the social network questions from the survey provide us with village-specific spillover information on two levels: geographically and socially. It is difficult to establish which of the two specifications represents the best approximation of actual spillovers. On the one hand, it could be argued farmers are most likely to learn from the farmers in their social network (i.e., social learning), which would lend the most credence to the specification using survey data. On the other hand, one could also claim farmers do not need to know fellow farmers in order to be able to copy their methods (i.e., observational learning), which would validate the GPS specification.[10] Most likely, in reality, a combination (or an interaction) of the two is occurring, with some farmers learning some techniques from their social network and others from observing nearby farmers.

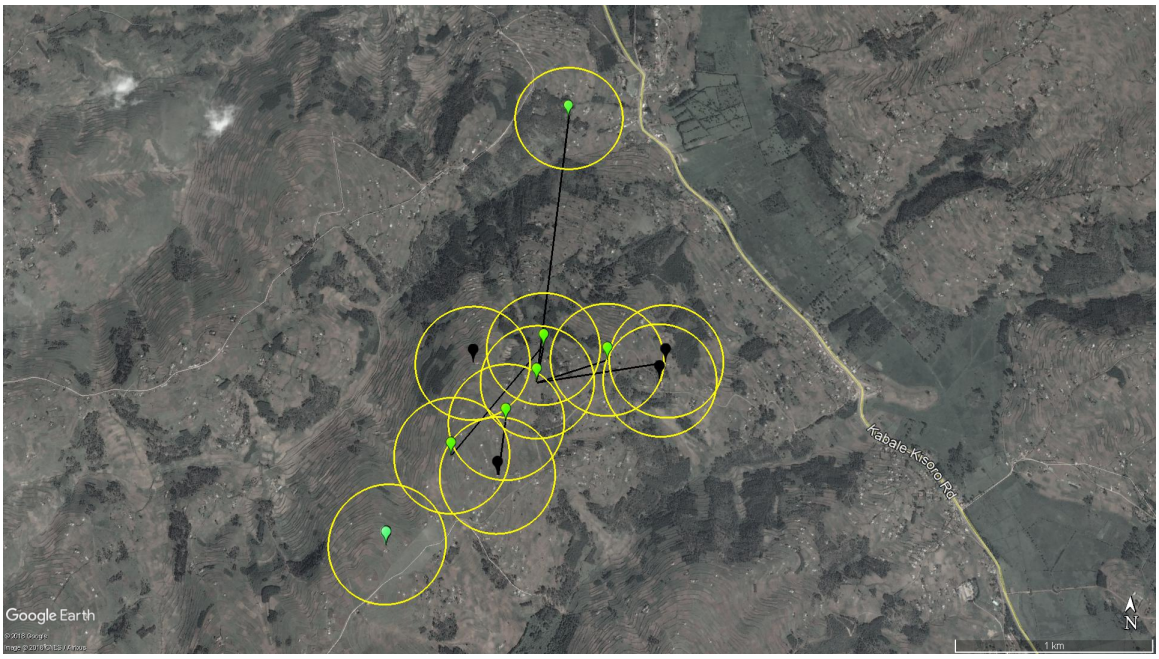


Figure 4: Treatment (dark and light dots refer to pure control and any treatment farmers respectively) and spillover conditions (circles and lines refer to the GPS and the network specification respectively) of farmers in one sample village

Given the small sample size, we model spillovers as a binary event: farmers are either in a spillover condition or they are not. For the GPS specification, we use the location of a farmer using GPS coordinates recorded at the time of the intervention, constructing a euclidean distance matrix and defining a geographical spillover radius based on a distance threshold.[11] The assumption here is that farmers who live close to each other are more likely to have learned about the techniques in the videos. Second, we exploit the collected network data, which enables us to construct a network matrix with dummies indicating whether farmers have discussed the video with each other.[12] As an illustration, Figure 4 shows both spillover specifications visually for one of the villages in our sample. When farmers are in each other's circle, they are said to be in

[10] While geographic proximity is not necessarily a a good proxy for the transmission of information (Chandrasekhar and Lewis, 2016), it is more likely to apply in the adoption of easily observable agricultural techniques (like pegging plants for instance).

[11] In particular, we first calculate median distance between farmers within each village, from which we then take the average and divide by 2.

[12] More concretely, the dummy gets a value of one if one of the farmers in any given relationship reports having discussed the video with the other.

a geographic spillover relation with one another. A line between two farmers indicates that they discussed the video with each other and are thus in the network spillover condition. Figure 4 also demonstrates that there is some degree of overlap between the two specifications, but that they are certainly not equivalent. As such, both sources of data will be used separately in the analysis.

Modeling spillovers in this way results in 4 mutually exclusive potential treatment conditions (for each specification) that are a function of both the farmer's own treatment assignment and the treatment assignment of any other units either within the geographical spillover radius or within his or her 'video' network. First, there is a pure control group (d_{00}), where both the unit itself and all other units within the spillover radius or network are assigned to the control. Second, there is a direct treatment group (d_{10}), where the unit has been assigned to the treatment while all others in the neighborhood or in the network are assigned to the control. Third, there is an untreated group that is contaminated (d_{01}) if there is any unit that is treated within the network of a unit that is assigned to the control. Finally, there are units that are assigned to the treatment and also have units in their vicinity or network that are treated (d_{11}).[13]

For each unit, probabilities of being in a certain spillover condition are estimated using simulation. We conduct 10,000 simulated random treatment assignments (within groups to take into consideration the matched group design). For each simulation, farmers are thus categorized into one of the four categories defined above (d_{00}, d_{01}, d_{10}, d_{11}). The probability that a particular farmer gets assigned to a particular group can then be calculated by simply counting the number of occurrences in each group and dividing by 10,000. These probabilities are then used for inverse probability weighting in regression models of pairwise comparisons of outcomes of the groups. For instance, one can compare outcomes of the pure control (d_{00}) with the direct treatment group (d_{10}) to see what the impact without interference would look like, but one can also compare the pure control (d_{00}) to the contaminated controls (d_{01}) to estimate the size of the spillover effect.

4 Results

4.1 Experience with treatments

Before delving into the actual results of this study, it is worth considering the farmers' experience with the video. Since these questions were not asked to control farmers, they can only be used for descriptive analysis. This is summarized in Table 2. Results are reported for the whole sample (first column), as well as for the PSS and PSSH video separately (second and third column respectively).[14]

[13] Modeling spillovers in this way is precisely what prohibits us from using the data related to whether or not farmers know each other: our sample contains too few farmers who do not know anyone, which would have resulted in not enough observations in the d_{00} group. For the GPS specification, the number of observations in each group is $\{d_{00}, d_{10}, d_{01}, d_{11}\} = \{53, 69, 50, 72\}$ for the PSS treatment and $\{d_{00}, d_{10}, d_{01}, d_{11}\} = \{49, 75, 56, 64\}$ for the PSSH treatment, while for the video network specification, this amounts to the following numbers: $\{d_{00}, d_{10}, d_{01}, d_{11}\} = \{78, 39, 56, 62\}$ for the PSS treatment and $\{d_{00}, d_{10}, d_{01}, d_{11}\} = \{76, 43, 60, 56\}$ for the PSSH treatment.

[14] As there is overlap between those who saw the PSS and those who saw the PSSH video, it is possible to have a lower mean for the total sample than for either video separately.

Table 2: Video experience descriptive statistics

Statistic	Total	PSS video	PSSH video
Remember topic (%)	88.11	88.62	89.34
Remember specifics (%)	63.24	62.70	67.21
Video useful (%)	92.43	93.50	92.62
Why useful?	Learning + motivation		
Rating (0-5)	4.46	4.49	4.46
Trust person (%)	92.97	94.31	94.26
Contact with others			
Yes/No	68.65	70.73	70.49
How many?	5.95	6.09	6.69
Who?	Exclusively locals		
What?	Mostly general content		
Willing to pay (%)	81.08	77.24	85.25
Possible to watch (%)	23.78	24.39	22.95

Notes: PSS=Positive Seed Selection, PSSH=Proper Seed Storage and Handling. Descriptive statistics are based on 185 observations.

Overall, farmers' experiences with the two videos are positive. Almost all farmers find the videos useful, while rating the quality of the video at about 4.5 out of a possible score of 5. Most farmers report having learned new things and feeling more motivated through the videos, which provides evidence with respect to the appropriateness of the theory of change outlined above. Farmers are also asked what they remember about the video: a vast majority (almost 80%) report they remember the general content of the video (i.e., that it was related to potato farming) while most (between 62% and 67%) recall the specific topic of the video (PSS or PSSH). The use of a model farmer in the video also seems to have increased the credibility of the information provided: more than 90% of farmers indicate they trust the person in the video. A couple of questions about the contact of treatment farmers with other farmers are also included, complementing the village-specific network data (see Section 3.3). A majority (about 70%) have talked to other locals (family members, neighbors or fellow farmers) about the video. Those that have talked to others, have talked to about 6 people, discussing mostly general content (rather than specific details). Interestingly, the willingness to pay for such videos is quite high: about 80% report they are willing to pay for these and similar videos. As a comparison, only 16 percent of farmers report being willing to pay for extension services in general at baseline. Finally, about a quarter of farmers would be able to access the video independently, a number which is likely to increase drastically in the coming years.

4.2 Results without spillovers

To get a general sense of the main results when spillovers between treatment and control are not taken into account, we first look at simple comparisons between treatment and control for the different outcome indices as defined in Section 3.2. Table 3 reports average treatment effects of the PSS treatment (first column) and the PSSH treatment respectively (second column). They are obtained from an Ordinary Least Squares (OLS) regression of the outcome on a treatment indicator. To account for the matched randomization procedure (as described in Section 3.1), group fixed effects are also included in the regressions. In principle, this should be sufficient to obtain the correct standard errors (Bruhn and McKenzie, 2009). However, since the sample size is small and given the binary nature of some outcomes, randomization inference (RI) is used. RI is a method whereby the difference in outcomes between treatment and control of the actual experiment is compared

to the distribution of differences in outcomes between treatment and control of (a sample of) all possible permutations of the treatment assignment.[15] The method has the distinct benefit of generating exact results regardless of the regression specification (Young, 2017).

Looking at Table 3, when spillover effects are disregarded, we find no evidence of direct impacts (knowledge), intermediate impacts (the adoption of practices or input use) or final impacts (yield, commercialization and consumption) of the PSS video treatment (the video related to seed selection), nor of the PSSH video treatment (the video related to seed storage and handling). The PSS video treatment does seem to have led to a 0.286 standard deviations increase in the adoption of agricultural practices related to that treatment, but the effect is only significant at the 10 percent level. In the next section, we show how the results change when spillovers are taken into account.

Table 3: Results without spillovers

	PSS	PSSH	N
Information	0.214	0.021	236
(3 questions per treatment)	(0.104)	(0.880)	
Practices	0.286*	0.012	196
(6 per treatment)	(0.052)	(0.937)	
Input use	0.040	0.055	202
(fertilizer, chemicals, hired labor)	(0.768)	(0.688)	
Final outcomes	0.142	0.125	198
(yield, proportion sold, consumption)	(0.329)	(0.389)	

Notes: Dependent variables are indices, based on the outcomes between brackets. All regressions include a constant and group fixed effects. P-values are in brackets and are based on randomization inference, with a random sample of 10,000 permutations used. *, **, *** denote significance at the 10, 5 and 1 percent level respectively.

4.3 Results with spillovers

We start by replicating Table 3, but now also model potential spillovers between treatment and control units. In Table 4, we show how the PSS (top panel) and the PSSH intervention (bottom panel) affect the different outcome indices. Using the methodology outlined in Section 3.3, we include comparisons between the pure control group (d_{00}) and the contaminated control group (d_{01}), which is referred to as the spillover effect, as well as comparisons between the pure control group (d_{00}) and the direct treatment group (d_{10}), which quantifies the direct treatment effect. Both the results where spillovers are assumed to be driven by proximity (GPS) as well as results where we assume social networks are important for spillovers (N'work) are presented.

Modeling spillovers seems to affect both the estimated effect size and significance of the results of our study, particularly for the PSS video intervention. Looking at the direct treatment effect, we observe that the PSS video has led to a significant increase in awareness about seed selection-related information, to the adoption of (some of) the practices explained in the video and to an increase in the probability of improved

[15] Here, a random sample of 10,000 permutations of the treatment assignment are run using Stata's *ritest* command (see Hess, 2017).

input use. However, these impacts did not translate into significant improvements in final outcomes, which may be explained by the fact that more time may need to elapse for the different channels to materialize into higher yields, improved farmer welfare or commercialization. These results are robust to using either spillover specification, which confirms there is overlap between the GPS and the network data. Or, in other words, if farmers live close enough to each other, they also have a higher chance of having discussed the video with each other. While we do seem to get a clear cut treatment effect when comparing treatment and pure control farmers, the direction and size of the spillover effects is less precisely estimated: most coefficients are insignificant or negative.

We do not find similar impacts for the PSSH video treatment (see the bottom panel in Table 4). It appears that providing information on storage practices for seed potatoes does not lead to improvements in the awareness about those practices, does not lead to the adoption of those practices and does not lead to a significant increase in input use. Given the lack of any direct or intermediate impacts, it is somewhat surprising to see that the coefficient of the impact of the PSSH video on the final outcomes index (which includes outcomes such as yield and consumption) is large and significant in both spillover specifications. As with the results for the PSS video, the spillover effects are mostly insignificant and small. In what follows, we further unpack these results (looking at some of the individual questions or outcomes behind each index) to explain some of results and to disentangle the channels that are driving the observed impacts.

Table 4: Main results with spillovers

	PSS							
	Information		Practices		Input Use		Final	
	GPS	N'work	GPS	N'work	GPS	N'work	GPS	N'work
spillover effect	-0.197	-0.513*	0.666**	-0.022	-0.074	-0.407	-0.105	-0.485*
	(0.348)	(0.063)	(0.038)	(0.951)	(0.714)	(0.177)	(0.636)	(0.090)
direct treatment effect	0.448***	0.313**	1.329***	0.683***	0.420**	0.312*	0.151	0.002
	(0.003)	(0.046)	(0.000)	(0.001)	(0.020)	(0.080)	(0.434)	(0.992)
pure control average	-0.080	-0.010	-0.648	-0.141	0.039	0.154	0.069	0.117
	PSSH							
	Information		Practices		Input Use		Final	
	GPS	N'work	GPS	N'work	GPS	N'work	GPS	N'work
spillover effect	0.162	-0.162	-0.212	-0.112	0.183	0.438	0.666***	0.035
	(0.330)	(0.450)	(0.305)	(0.658)	(0.370)	(0.133)	(0.003)	(0.924)
direct treatment effect	0.042	0.081	0.006	0.442***	0.246	0.323*	0.976***	0.770***
	(0.838)	(0.616)	(0.980)	(0.006)	(0.250)	(0.078)	(0.000)	(0.001)
pure control average	-0.091	0.008	0.129	0.085	-0.181	-0.107	-0.549	-0.057

Notes: Analysis based on GPS coordinates and survey network data. Dependent variables are indices of different survey questions pertaining to a given outcome category. P-values are in brackets and *, **, *** denote significance at the 10, 5 and 1 percent level respectively. Numbers of observations per outcome category are the same as in Table 3.

We start by looking at information, which is reported in Table 5. We report how the PSS intervention affects selection-related knowledge (top panel). The bottom panel reports how PSSH affects storage and handling-related knowledge. We find that about 60 percent of farmers in the pure control group (d_{00}) know that the largest plants in the field need to be pegged for seed selection (*sel1*). This proportion increases to between 74 and 79 percent in the pure treatment group (d_{10}), depending on the spillover specification.

However, the difference is only statistically significant for the network spillover specification. The reverse is true for the question about the size of potatoes to be selected for seed (*sel2*). In the GPS specification, the pure control average is about 75 percent and the spillover and direct effect are significant, but this does not hold for the other spillover specification. The question that was added at the endline (*sel3*), did not really work: apparently, most of the farmers already knew the answer to the question, leading to too little variation to arrive at credible estimates. Our pre-analysis plan states we would discard such variables from the analysis.

Table 5: Spillover analysis for information effects.

	PSS					
	sel1		sel2		sel3	
	(GPS)	(N'work)	(GPS)	(N'work)	(GPS)	(N'work)
spillover effect	-0.085	-0.129	0.157*	-0.051	-0.128***	-0.118*
	(0.409)	(0.330)	(0.061)	(0.597)	(0.005)	(0.068)
direct treatment effect	0.143	0.194**	0.198***	0.004	0.014	0.055*
	(0.126)	(0.015)	(0.004)	(0.945)	(0.549)	(0.089)
pure control average	0.599	0.600	0.754	0.883	0.980	0.936
	PSSH					
	store1		store2		store3	
	(GPS)	(N'work)	(GPS)	(N'work)	(GPS)	(N'work)
spillover effect	-0.024	-0.021	0.005	-0.037	0.247***	0.055
	(0.241)	(0.537)	(0.878)	(0.405)	(0.005)	(0.607)
direct treatment effect	-0.032	-0.006	0.001	0.024	0.153*	0.000
	(0.321)	(0.862)	(0.980)	(0.291)	(0.084)	(0.997)
pure control average	0.988	0.968	0.963	0.968	0.681	0.842

Notes: Analysis based on GPS coordinates and survey network data. See Appendix I for questions and answering possibilities. P-values are in brackets and *, **, *** denote significance at the 10, 5 and 1 percent level respectively. N = 236

The bottom panel shows that for the two questions that were also asked immediately after the intervention (*store1* and *store2*), we do not find any significant spillover or direct treatment effects, confirming the findings from Table 4. However, as for *sel3* both of these variables suffer from limited variability and awareness about this type of information seems to have been high overall, even in the control group, which means these results should be interpreted with caution. The storage and handling related question that was added in the endline survey (*store3*) does seem to be affected by interference, but only in the spillover specification that uses GPS coordinates. If we compare only pure control to directly treated farmers, the proportion of farmers that answers correctly increases from 68 to 83 percent, albeit the difference is only significant at 10 percent. The spillover effect for this question is large, amounting to almost 25 percentage points. On the whole, it should be noted that the awareness of most of the practices in the videos is high for both treatments (and particularly for the PSSH video), which to some extent invalidates the original purpose of the intervention, namely to spread information about seed related agricultural practices. However, it could still be the case that, even for the information that the farmers already possess, the videos serve as a reminder or a confirmation of the value of those practices and in that way still lead to changes in behavior.

In Table 6, we look at how interference between treated and untreated farmers affects our findings related

to the adoption of practices shown in the video. Looking at the top panel, we find that around 27 percent in the pure control group pegs healthy plants ((1) in the top panel), and that this percentage increases by roughly 35 percentage points in the direct treatment group for both spillover specifications. We also find large and significant effects of the PSS video on the practice of removing pegs from diseased plants (2), harvesting pegged plants first (3) and selecting potato tubers with at least four eyes (5), regardless of how the network is defined. Especially for the latter two practices, contamination of the control group seems to have been an issue. The remaining two practices, namely throwing away rotten potatoes (4) and selecting egg-sized potatoes as seeding material (6), were already being applied widely with control averages at least around 90 percent.

Table 6: Spillover analysis for adoption of practices

PSS

	(1)		(2)		(3)		(4)		(5)		(6)	
	GPS	N'work	GPS	N'work	GPS	N'work	GPS	N'work	GPS	N'work	GPS	N'work
spillover	-0.048	-0.087	0.118	0.292***	0.195**	-0.268**	-0.094**	0.097	0.263**	0.175	0.103	-0.068
	(0.647)	(0.504)	(0.135)	(0.003)	(0.010)	(0.045)	(0.036)	(0.319)	(0.020)	(0.214)	(0.114)	(0.195)
direct	0.365***	0.331***	0.511***	0.454***	0.525***	0.227**	-0.031	0.052	0.323***	0.224***	0.109**	-0.007
	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.016)	(0.437)	(0.319)	(0.001)	(0.005)	(0.018)	(0.752)
ctrl average	0.279	0.264	0.098	0.077	0.062	0.285	0.982	0.892	0.508	0.646	0.891	0.989

PSSH

	(1)		(2)		(3)		(4)		(5)		(6)	
	GPS	N'work	GPS	N'work	GPS	N'work	GPS	N'work	GPS	N'work	GPS	N'work
spillover	-0.098*	-0.032	0.032	-0.184*	0.031	0.123	-0.261***	-0.051	0.023	0.068	0.011	0.000
	(0.096)	(0.786)	(0.616)	(0.062)	(0.768)	(0.388)	(0.008)	(0.675)	(0.768)	(0.481)	(0.462)	(1.000)
direct	-0.078	0.027	-0.071	0.013	0.008	-0.068	0.030	0.456***	0.058	0.066	0.027	-0.007
	(0.275)	(0.686)	(0.390)	(0.785)	(0.940)	(0.463)	(0.783)	(0.000)	(0.455)	(0.247)	(0.362)	(0.791)
ctrl average	0.915	0.852	0.863	0.927	0.378	0.391	0.402	0.245	0.838	0.866	0.008	0.024

Notes: Analysis based on GPS coordinates and survey network data. A description of practices can be found in Appendix II. P-values are in brackets and *, **, *** denote significance at the 10, 5 and 1 percent level respectively. N = 196

The PSSH treatment seems to be far less effective in changing agricultural practices. The bottom panel in Table 6 shows that for none of the storage and handling practices we inquire about, the direct treatment effect is significantly different from zero (at least not consistently across both spillover specifications). However, it should be noted that control averages for 3 out of 6 practices are high, suggesting these practices had already been adopted by a majority of farmers, even before the video treatment. It could also be that cost plays an important role in explaining the lack of adoption of some of the storage related practices as opposed to the seed related practices which are labor-intensive, but do not require major investments. Indeed, the storage practices that are least applied require the purchase of an insecticide or a disinfectant (practices (3) and (6) respectively) or at least some changes to the potato storage facilities (practice (4)). Even though the associated costs are relatively minor, we cannot rule out that, in our experiment, the informational constraint is secondary to more binding cash or credit constraints.

We also check if, when spillovers are modeled as a function of location or farmers' social networks, the videos lead to a change in input use (see Table 7). Results suggest that the PSS treatment increases

the probability of purchasing seed. Between 32 and 35 percent of farmers in the pure control reports to have purchased seed. This proportion increases significantly (with some notable variation in the size of the difference depending on the spillover specification used) in the directly treated group. While this may seem somewhat counter-intuitive, the PSS video might have made farmers more aware of the importance of good quality seed, inducing them to purchase seed from agro-input dealers instead of relying on recycled seed. For the PSSH treatment, we do not find significant direct effects on the probability of purchasing seed potatoes. Fertilizer use, on the other hand, seems to only have been affected by the PSSH treatment and not (significantly) by the PSS treatment. We do not find any effects from either video on the use of chemicals, but there is some evidence to suggest that the PSS video crowds in the use of hired labor, which could be a result of the labor-intensity of some of the activities promoted in the PSS video such as pegging the plants, checking them periodically and harvesting them first.

Table 7: Spillover analysis for input use

	PSS							
	seed		fertilizer		chemicals		labor	
	GPS	N'work	GPS	N'work	GPS	N'work	GPS	N'work
spillover effect	0.033	0.013	-0.074	-0.157	-0.037	-0.159*	0.047	-0.012
	(0.736)	(0.920)	(0.517)	(0.276)	(0.550)	(0.091)	(0.649)	(0.925)
direct treatment effect	0.414***	0.215**	0.167	0.183**	0.022	-0.018	0.195**	0.138*
	(0.000)	(0.013)	(0.114)	(0.047)	(0.679)	(0.727)	(0.017)	(0.051)
pure control average	0.319	0.353	0.487	0.530	0.923	0.934	0.712	0.760
	PSSH							
	seed		fertilizer		chemicals		labour	
	GPS	N'work	GPS	N'work	GPS	N'work	GPS	N'work
spillover effect	-0.049	-0.139	0.111	0.283**	-0.099*	0.056	0.225**	0.059
	(0.640)	(0.334)	(0.279)	(0.048)	(0.094)	(0.572)	(0.024)	(0.627)
direct treatment effect	-0.035	0.110	0.242**	0.386***	-0.042	0.091	0.068	-0.210**
	(0.728)	(0.204)	(0.026)	(0.000)	(0.486)	(0.125)	(0.525)	(0.021)
pure control average	0.501	0.500	0.327	0.391	0.938	0.841	0.618	0.781

Notes: Analysis based on GPS coordinates and survey network data. Dependent variables indicate probability of using (purchasing in the case of seed) the indicated item (0/1). P-values are in brackets and *, **, *** denote significance at the 10, 5 and 1 percent level respectively. N = 202

Finally, we conduct the same type of analysis for a set of final outcomes, including yield, market participation and household welfare (approximated by consumption). The results are reported in Table 8. We find that both the PSS video and the PSSH video have a significant impact on potato yields. In particular, while farmers in the pure control group report to have harvested about 2.2 MT/acre, this increases to about 3.6 MT/acre in the group that was directly treated with the PSS video. This corresponds to an increase of about 60 percent, albeit from a low base and not robust to the use of the other spillover specification. For the PSSH experiment, yields among the directly treated are essentially twice those in the pure control (here, for both specifications). We also find that the PSS video reduces the proportion of potatoes sold to the market, but only in case of the GPS specification. In the pure control group, about half of the farmers report selling to the market. However, among the directly treated, the rate of market participation is 15 percentage points lower. We find a significant spillover effect that goes in the same direction as the direct effect, which would

most likely lead to a failure to find a significant effect in an analysis that assumes no spillovers. We find an opposite effect for the PSSH treatment: here, farmers in the treatment group are more likely to sell potatoes in the market. Again, there are spillover effects that go in the same direction, making it harder to detect a significant effect if spillovers would not be incorporated in the analysis. Both findings can be explained as follows: while the PSS treatment might induce farmers to save more potatoes as seed rather than selling them as ware in the short run, the PSSH treatment is aimed at ensuring potatoes (primarily seed, but also ware) survive longer in storage, which in turns explain why more can be sold by PSSH treated households. Finally, after controlling for spillovers, household welfare also increases, particularly in the PSS experiment. Farmers in the pure control group spend on average about 79,000 UGX per week.[16] This increases to somewhere between 95,000 and 110,000 UGX (depending on the specification) in the directly treated group, corresponding to an increase of 20 to 40 percent. We do not find the same increase in consumption as a result of having seen the PSSH video.

Table 8: Spillover analysis for final outcomes

	PSS					
	yield		prop sold		cons	
	GPS	N'work	GPS	N'work	GPS	N'work
spillover effect	-0.022	-0.469**	-0.168***	-0.049	0.061	-0.102
	(0.894)	(0.025)	(0.008)	(0.496)	(0.567)	(0.438)
direct treatment effect	0.492**	-0.040	-0.149***	-0.011	0.313***	0.187**
	(0.010)	(0.807)	(0.000)	(0.825)	(0.001)	(0.010)
pure control average	7.691	7.852	0.508	0.412	11.271	11.003
	PSSH					
	yield		prop sold		cons	
	GPS	N'work	GPS	N'work	GPS	N'work
spillover effect	0.326*	0.258	0.128*	0.181**	0.202**	0.009
	(0.083)	(0.303)	(0.050)	(0.040)	(0.047)	(0.945)
direct treatment effect	0.728***	0.590***	0.172**	0.197***	0.175*	0.102
	(0.000)	(0.003)	(0.010)	(0.001)	(0.090)	(0.214)
pure control average	7.285	7.563	0.297	0.335	11.215	10.901

Notes: Analysis based on GPS coordinates and survey network data. Yield, prop sold and cons refer to respectively yield (in log of kg/acres), the proportion of harvest that was sold (0/1) and consumption (in log of weekly UGX expenditures). P-values are in brackets and *, **, *** denote significance at the 10, 5 and 1 percent level respectively. N = 198

We see two potential explanations for the fact that we do seem to find significant impacts of the PSSH video on final outcomes, but not on direct or intermediate outcomes. First, we may have missed important channels towards these final outcomes other than those included in the survey or the analysis. For instance, while farmers might have already been aware of the information in the PSSH video and did not apply any of the practices for a variety of reasons, the video might have triggered an increased motivation on the part of farmers and induced them to adopt other beneficial practices. Second, it could simply be the case that our final outcomes are measured with considerable bias. This argument can certainly be made for yield, the notoriously difficult measurement of which has spawned an entire branch of literature in agricultural

[16] At the time of the survey, 1 USD was roughly equivalent to 3,650 UGX, which means that weekly expenditures of 79,000 UGX amount to about 22 USD.

economics (see, for instance, Gourlay et al., 2017). The discrepancies between the two spillover specifications in case of the PSS video provide further evidence into the direction of measurement error. Whichever of the two explanations carries most weight, is difficult to say at this point and most certainly warrants further inspection after more agricultural seasons have elapsed.

4.4 Validity of results

Using the methodology of Aronow and Samii (2017) comes with an important caveat. They warn that, while not taking into account spillovers in the presence of interference will most certainly lead to biased results, the results would still be biased (but possibly in a different direction) if the specification of spillovers is done incorrectly. Effectively, the methodology exchanges one set of assumptions (a non-violation of SUTVA) for another, namely that the characterization of spillovers is correct. Translating this to our context, this means that we have to ensure that the spillover radius in the case of the GPS spillover specification correctly represents reality and that our survey network data is indeed an accurate representation of how information spreads among study participants. Most crucially, we have assumed that spillovers only occur locally and that information only travels within and not between villages. The overlap in results between the two specifications strengthens us in the belief that spillovers are indeed being captured adequately, but we nevertheless present two pieces of evidence to further validate this.

First, we vary the spillover radius, which refers to the threshold within which spillovers are taken into account in the GPS specification and which was hitherto arbitrarily fixed at 300 meters. As an illustration, the direct treatment effect for the PSS treatment on a set of key outcomes (the different indices of outcomes) is calculated for different spillover radiuses (from 100 to 700 meters) following the same procedure as above. This should then be interpreted as follows: as the spillover radius is increased, more control farmers will be allocated to the spillover condition, with eventually only the most remote control farmers still in the comparison group (the d_{00} condition). One issue with this strategy is that eventually a point is reached where the pure control group becomes too small for any comparisons to still make sense. If we believe spillovers are specified correctly, we should observe the direct treatment effect to be rather stable for different spillover radiuses around 300 meters, as you would not expect a small variation in distance between farmers to have an impact on how they interact with one another (and thus generate interference). This is precisely what can be seen in Figure 5. It demonstrates our results are indeed robust to varying the spillover radius between roughly 300 and 600 meters. It also offers an indication that spillovers in our case are a preeminently local phenomenon.

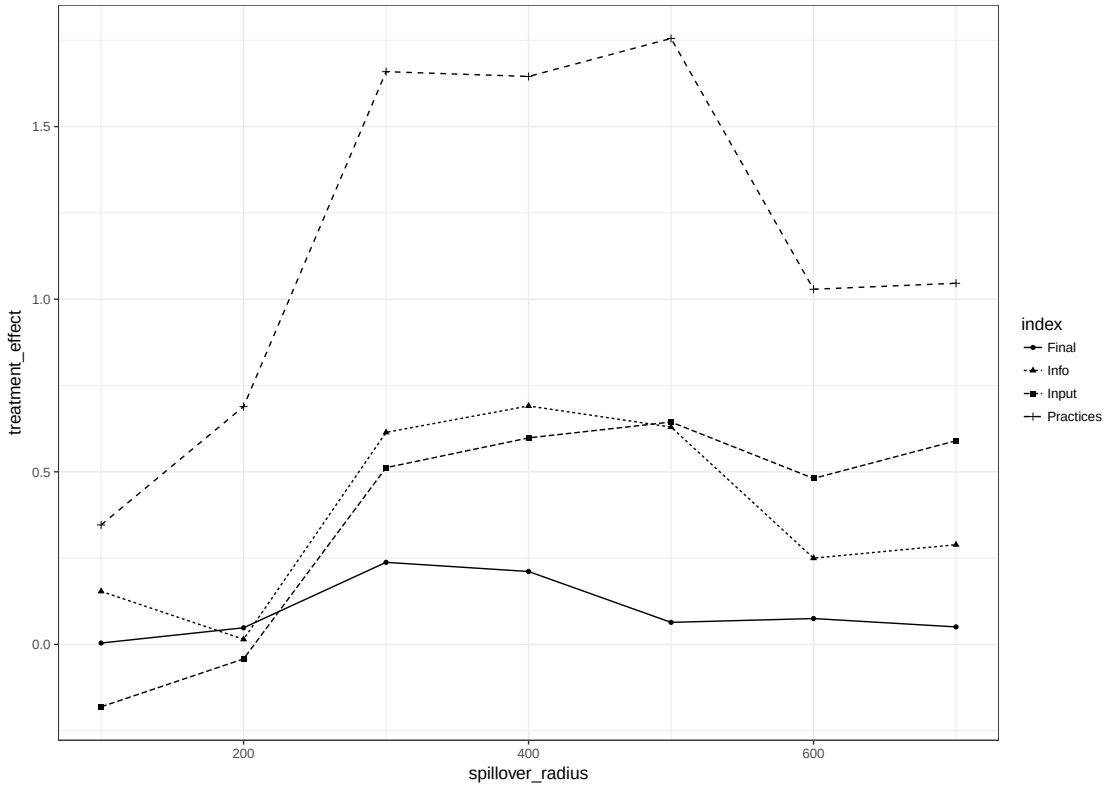


Figure 5: Variation in spillover radius (in meters) - PSS on main outcomes

Implementing a similar robustness check for the network spillover specification is not as straightforward given the nature of the data. For example, even though the information whether or not farmers know each other could theoretically be used as source of data to define spillovers, this was not possible practically as nearly all respondents report knowing the other respondents in their respective villages. This would lead to too many observations being classified in a spillover condition for any meaningful comparisons. As already mentioned in Table 2, however, most farmers who watched any of the videos report having only discussed the video with people within their immediate surroundings (friends, family, neighbors or other farmers), confirming the local nature of spillovers in our sample.

5 Discussion and conclusion

In this paper, we investigate if short, stand-alone video messages targeted to individual farmers can reduce potato seed degeneration among seed-recycling Ugandan smallholder potato farmers. In particular, we examine the impact of providing information on positive seed selection on the one hand, and proper storage and handling of seed on the other hand over the course of one year after the videos are shown. We judge the effectiveness in terms of the awareness of information, of practices adopted and inputs used. We also check if the interventions have an impact on outcomes further down the line, such as agricultural productivity, market participation and welfare.

We suspect that spillovers (or, put differently, interference between treatment and control) are important in our experiment. While spillover effects seriously impede the evaluation of an experiment, it should be considered a desirable outcome of an intervention that aims to disseminate agricultural extension information

among smallholder farmers. When these spillovers are not taken into account, we do not find an impact on learning, practices adopted, inputs used or on final outcomes. To formally account for spillovers, we implement a randomization-based framework developed by Aronow and Samii (2017). When we account for interference between treatment and control farmers on the basis of distance between farmers and survey data indicating whether or not they discussed the video with each other, we find that both videos had important beneficial outcomes. The video on seed selection (PSS) has led to increased awareness and subsequent adoption of practices shown, as well as a higher probability of using hired labor and a higher probability of buying potato seed on the market. These improvements have not (yet) resulted into robust progress in terms of yield, commercialization or welfare, but they did lead to increased consumption. The video on seed storage and handling (PSSH), on the other hand, did not engender the same increases in the awareness and adoption of storage-related practices, nor did it lead to a higher probability of using improved inputs (fertilizer being the notable exception). At the same time, the PSSH video did result in robust improvements in terms of yield and commercialization. We further argue that the relative success of the PSS treatment compared to the PSSH treatment is due to differences in the cost of implementation of some of the practices. Indeed, when we further unpack the results and look at the adoption of different practices individually, we notice that the practices where impact is highest were those that did not require a monetary investment. A higher share of the practices promoted in the PSSH video demanded an investment compared to the practices recommended in the PSS video, which could explain the lack of adoption.

The results in this paper have implications for agricultural policy and research in three distinct ways. First, we draw further attention to the issue of interference or spillovers in the design and the analysis of experiments. Often, due to time and budget constraints, randomization happens at the individual or household level, which provides more statistical power, but is more susceptible to interference than experiments where randomization takes place at a more aggregate level (for instance villages or sub-counties). We show how randomization-based inference can be used to take care of spillover effects ex-post. The only requirement of this methodology is that you need to specify how spillovers occur in your sample. But this is something that can easily be tested and varied in the design phase using qualitative methods. In other words, we demonstrate it is possible to compensate in the analysis phase for otherwise costly interventions or provisions in the design phase of an experiment. Further, exhaustive delivery of information, i.e., to every single farmer in an area as is the case in most traditional settings of agricultural extension, is probably not strictly necessary. Our results confirm once more (see, for instance, Beaman and Dillon, 2018) that information delivered to a specific set of farmers will spread to those who live in their close vicinity. While it is not clear if this would hold for every type of information or every mode of information delivery, these results could potentially lead to cost and time savings in agricultural extension.

Second, our results can also contribute to the discussion on the use of information technologies in agricultural extension. Our study demonstrates that farmers are open to new ways of learning about agricultural techniques. The videos were hailed as an interesting and engaging way to receive information and they were heavily discussed. While the set-up of our experiment does not allow us to compare the use of videos to alternative extension strategies, we can speculate about the type of agricultural information that is suitable to be spread through such a channel. It is clear that videos are best suited to spread information on simple agricultural techniques that have not yet been widely adopted but at the same time do not require substantial investments. Almost by definition, field- or farmer-specific information is not something that can be effectively conveyed through videos. In that sense, we consider the role of videos (or of information technologies more broadly) in agricultural extension to be complementary, whereby videos provide information on gen-

eralizable and cost efficient practices, while extension officers or experts provide farmers with tailored and more specific advice, constantly keeping an eye on other constraints (besides informational) farmers face.

Finally, our study also confirms the prevalence of seed recycling as an agricultural practice. It demonstrates there is a demand for interventions (information or otherwise) that help farmers improve practices related to seed quality. While it is too early to tell whether or not the adoption of some of the practices in our intervention will actually lead to any meaningful improvements in seed quality, it is clear there is room for similar interventions (using videos or other information technologies) to be implemented wherever institutional constraints impede the development of formal seed systems.

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Appendix

Appendix I: Multiple-choice questions

Code	Question	Answering possibilities (correct one in bold)		
sel1	Which plants should you peg for seed potato selection?	**The largest plants in the field**	The smallest plants in the field	Average sized plants in the field
sel2	What size should a seed potato tuber be?	The larger the better	**Size of an egg**	The smallest ones you find
sel3	Which plants should you peg for positive seed selection?	Random plants	Diseased plants	**Healthy plants**
store1	In which lighting conditions should you store seed potatoes?	In direct sunlight	In a dark place	**In indirect light**
store2	How should you store your seed potatoes?	In bags that have been thoroughly cleaned with JIK	**Spread out on racks or on dried grass on the floor**	In airtight containers or buckets with a closing lid
store3	Where should you ideally store your seed potatoes?	Together with other crops	Somewhere in the house	**In a separate store**
gen1	Which of the following statements is correct?	Immediately after harvest, you should thoroughly wash seed potatoes before putting them in storage using JIK	Immediately after harvest, you should thoroughly wash seed potatoes before putting them in storage using clean water	**You should never wash seed potatoes before putting them in storage**
gen2	When picking a field for Irish potato farming...	**Pick a garden that is in highlands and in isolated areas**	Pick lowlands with plenty of water	Pick a garden close to your house or in densely populated areas
gen3	How can you spot bacterial wilt in a potato tuber?	Malformations	**Puss in the eyes**	Cuts

Appendix II: PSS and PSSH practices

Code	PSS	PSSH
(1)	Pegged healthy plants for seed potatoes	Stored seed potatoes in separate room/store
(2)	Removed pegs from diseased plants	Stored seed potatoes on racks or on dried grass on the floor
(3)	Harvested pegged plants first	Applied malathion dust in storage for seed potatoes
(4)	Threw away rotten tubers or tubers with malformations after harvest	Stored seed potatoes in diffuse lighting conditions
(5)	Kept only seed potatoes with more than four eyes	Stored seed potatoes in a ventilated room
(6)	Used egg-sized potatoes for seed	Cleaned material (shovels and bags) using JIK