
# How to Make Farming and Agricultural Extension More Nutrition-Sensitive: Evidence from a Randomized Controlled Trial in Kenya

*S. Ogutu[1]; A. Fongar[1]; T. Gödecke[1]; L. Jäckering[1]; H. Mwololo[2]; M. Njuguna[3]; M. Wollni[1]; M. Qaim[1]*

*1: University of Goettingen, Agricultural Economics and Rural Development, Germany, 2: University of Nairobi, Department of Agricultural Economics, Kenya, 3: Africa Harvest Biotechnology Foundation International, , Kenya*

*Corresponding author email: sylvester.ogutu@agr.uni-goettingen.de*

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*We analyze how agricultural extension can be made more effective in terms of increasing smallholder farmers' adoption of pro-nutrition technologies, such as biofortified crops. In a randomized controlled trial with farmers in Western Kenya, we implemented several extension treatments and evaluated their effects on the adoption of beans that were biofortified with iron and zinc. Difference-in-difference estimates show that intensive agricultural training tailored to local conditions can increase technology adoption considerably. Within less than one year, adoption of biofortified beans increased from almost zero to more than 20%. Providing additional nutrition training further increased adoption by another 10-12 percentage points, as this has helped farmers to better appreciate the technology's nutritional benefits. These results suggest that effective nutrition training through agricultural extension services is possible. Providing marketing training did not lead to additional adoption effects, although the study period may have been too short to measure these effects properly. This study is a first attempt to analyze how improved designs of agricultural extension can help to make smallholder farming more nutrition-sensitive. More research in this direction is needed. Key words: agricultural extension, technology adoption, biofortification, nutrition-sensitive agriculture, Kenya JEL codes: C93, O33, Q12, Q16, Q18*

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## Abstract

We analyze how agricultural extension can be made more effective in terms of increasing smallholder farmers' adoption of pro-nutrition technologies, such as biofortified crops. In a randomized controlled trial with farmers in Western Kenya, we implemented several extension treatments and evaluated their effects on the adoption of beans that were biofortified with iron and zinc. Difference-in-difference estimates show that intensive agricultural training tailored to local conditions can increase technology adoption considerably. Within less than one year, adoption of biofortified beans increased from almost zero to more than 20%. Providing additional nutrition training further increased adoption by another 10-12 percentage points, as this has helped farmers to better appreciate the technology's nutritional benefits. These results suggest that effective nutrition training through agricultural extension services is possible. Providing marketing training did not lead to additional adoption effects, although the study period may have been too short to measure these effects properly. This study is a first attempt to analyze how improved designs of agricultural extension can help to make smallholder farming more nutrition-sensitive. More research in this direction is needed.



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## Introduction

Hunger and micronutrient malnutrition remain widespread problems in many developing countries with serious negative health consequences (FAO 2017; IFPRI 2017). Many of the people affected live in smallholder farm households. Hence, the question as to how smallholder farming can be made more nutrition-sensitive is ranking high on the development policy agenda (Pingali and Sunder 2017). The important role of market access for improving food security in the small farm sector was highlighted in recent empirical work (Bellemare and Novak 2017; Koppmair; Kassie, and Qaim 2017; Ogutu, Gödecke, and Qaim 2017; Sibhatu and Qaim 2017). In addition, agricultural technologies specifically designed to improve nutrition can possibly play an important role. Prominent examples of such pro-nutrition technologies are biofortified crops, which were bred to contain higher amounts of micronutrients, such as orange-fleshed sweetpotatoes enhanced with provitamin A or high-iron rice and wheat (Bouis and Saltzman 2017; Jones and de Brauw 2015). Other examples of pro-nutrition technologies are certain species of vegetables or pulses that farmers may grow to increase household dietary diversity and address specific nutritional deficiencies (Fanzo 2017).

One problem with pro-nutrition technologies is that farmers' adoption incentives may sometimes be low (Gilligan 2012). Farmers tend to adopt new technologies rapidly when these contribute to gains in productivity and income. However, technologies that were specifically designed to improve nutrition do not necessarily increase productivity and income directly. With limited appreciation of the nutritional benefits, farmers are hesitant to adopt technologies that do not increase yield but may be associated with differences in crop taste and outward appearance. Farmers may also be concerned about not being able to market new types of crops with characteristics that are not yet widely known by traders and consumers. Even when farmers grow certain food crops primarily for home consumption, the potential to sell in the market is important when cash is needed.

Recent research showed that the adoption of pro-nutrition technologies is higher in settings where farmers have a good understanding of the technologies' agronomic and nutritional attributes (de Brauw, Eozenou, and Moursi 2015a; de Brauw et al. 2015b; de Groote et al. 2016). This implies that agricultural extension could and should probably play a prominent role for technology dissemination. Agricultural extension services have the mandate to facilitate technology transfer and improve innovation processes in the farming sector, but concrete experience with pro-nutrition technologies hardly exists. More generally, experience with the effectiveness of agricultural extension to promote innovation is rather mixed (Anderson and Feder 2004; Goodhue, Klonsky, and Mohapatra 2010; Läpple and Hennesy 2015). Hence, improvement in the design of agricultural extension is urgently needed. While previous studies have analyzed how agricultural training components could be improved to increase farmers' adoption of agronomic innovations (Davis et al. 2012; Läpple and Hennessy 2015), we are not aware of research that has developed and tested new extension approaches for the effective dissemination of pro-nutrition technologies. Here, we address this research gap with a randomized controlled trial (RCT) in Kenya. In particular, we evaluate how agricultural training can be combined with training in nutrition and marketing to increase farmers' adoption of a new bean variety biofortified with iron and zinc.

The name of the new bean variety is KK15. This variety was bred by the Kenya Agricultural and Livestock Research Organization (KALRO) using conventional breeding methods. Compared to other bean varieties commonly grown in Kenya, KK15 contains six times higher amounts of iron and about two times higher amounts of zinc, as a laboratory analysis that we commissioned showed. However, KK15 also differs from commonly-grown bean varieties in terms of other characteristics. According to KALRO, KK15 is high-yielding, resistant to root-rot disease, and matures earlier than most other varieties. Moreover, KK15 beans are black in color, whereas most popular bean varieties in Kenya are red. Probably because of the notable difference in outward appearance, widespread adoption of KK15 did not yet occur and may not be expected without specific extension efforts to promote this variety.

Our RCT includes three treatment arms, each with a different extension design. The first treatment only includes agricultural training. This involves explanations of the agronomic and nutritional attributes of KK15 to farmers, as well as the demonstration and training of suitable cultivation practices for this type of bean variety during different stages of the growing season. The second treatment adds specific nutritional training that goes beyond only explaining the nutritional attributes of KK15. In our study, nutrition training includes broader information about human nutritional requirements, balanced diets, and causes and consequences of nutrient deficiencies. The third treatment further adds marketing training, explaining simple mechanisms of market functioning, possible sales strategies, and linking up farmers with bean traders in the local setting. The three treatments are compared with a control group of farmers that did not receive any of these trainings, in order to evaluate the effects of the different extension designs on KK15 adoption.

The RCT was carried out in one region of Kenya and refers to one specific technology, so results cannot simply be extrapolated to other settings and technologies. Nevertheless, we expect that some broader lessons may also be learned, as evidence on the effects of combining agricultural and nutrition training is very limited. Nutrition training was shown to be an effective intervention to improve dietary quality in many situations (IFPRI 2017; Waswa et al. 2015), but such training is usually provided by nutrition and health workers, not by agricultural extension officers as in our RCT. Combining different training elements and piggybacking on existing networks of agricultural extension in rural areas could potentially be a cost-effective strategy to make smallholder farming more nutrition-sensitive.

The remainder of this article is organized as follows. The next section describes the empirical setting in Kenya and the sampling framework for the household survey and experiment. Subsequently, the experimental design and the strategy to estimate the effects of the different treatments are explained, before the estimation results are presented and discussed. The last section concludes.

## Empirical Setting

This study builds on an RCT carried out with smallholder farmers in Western Kenya. In Kenya, smallholder agriculture accounts for nearly 75% of total agricultural production (Olwande et al. 2015). Adoption of improved agricultural technologies is relatively low among smallholders, and poverty and malnutrition are widespread (Muthayya et al. 2013; KNBS 2015; Wainana, Tongruksawattana, and Qaim 2016). The performance of extension services is mixed (Muyanga and Jayne 2008). Our RCT focuses on the adoption of a biofortified variety of beans. Kenya ranks among the top ten producers of common beans in the world (USAID 2010). In Western Kenya, most farm households cultivate beans, which are usually intercropped with maize. Beans are frequently consumed by local farm households, often on a daily basis, so that they play an important role for food security.

### *Study Region*

For the study, we purposively selected two counties in Western Kenya, Kisii and Nyamira, primarily because our development partner, Africa Harvest Biotech Foundation International (Africa Harvest), had prior experience in these counties and several extension officers on the ground. Africa Harvest is a non-governmental organization and was in charge of carrying out the RCT extension treatments that we jointly designed. Given the high population density in

Western Kenya, farms in Kisii and Nyamira are very small, with an average farm size of less than two acres. Farms in this region are fairly diverse and typically produce a number of food crops, such as maize, beans, sweetpotatoes, bananas, and different vegetables. Many also produce cash crops such as tea and coffee and keep small herds of livestock, including chicken, sheep, goats, and sometimes cattle. Kisii and Nyamira have two agricultural seasons, the main season from March to July and a second season from September to January. However, due to favorable climatic conditions, seasonal boundaries in this part of Kenya are not very clear-cut. In terms of nutritional indicators, Kisii and Nyamira are similar to the national average. The prevalence of child stunting, the most common anthropometric measure of child undernutrition, is around 25% in both counties (KNBS 2015).

*Sampling Strategy*

Traditionally, agricultural extension was often implemented through extension officers visiting individual farmers for giving advice on specific topics (Anderson and Feder 2004). However, newer extension approaches often operate through farmer groups, which can not only increase cost-effectiveness but also facilitate mutual learning and sharing of experiences among farmers (Davis et al. 2012; Fischer and Qaim 2012). In fact, many farmers in Kisii and Nyamira county area are already organized in farmer groups registered with the Ministry of Gender, Children, and Social Development. We therefore decided to build on existing group structures and cluster the survey and the experimental treatments by farmer groups. We used a list of all existing farmer groups in Kisii and Nyamira counties, but excluded groups that had received specific development support during the previous two years to reduce possible contamination when estimating the effects of our experimental treatments. From the remaining groups on the list, we randomly selected 48 farmer groups for inclusion in the study. Of these 48 groups, 32 are located in Kisii and 16 in Nyamira county. Farmer groups in our sample have between 20 and 50 active members.

*Farm Household Survey*

In each of the 48 selected farmer groups we updated the membership lists together with the group leaders. From these membership lists, we randomly selected 20 member farmers for inclusion in the survey. However, some of the selected farmers were not available for interview, even after repeated visits. Especially in small groups it was also not always possible to replace unavailable farmers with other group members, so in some of the groups we have fewer than 20 farmers included in the survey. The survey was implemented in two rounds. The baseline round was conducted between October and December 2015, before the experimental treatments were started; it includes observations from 824 farm households. The follow-up survey was conducted between October and December 2016, after the experimental treatments were completed. Due to sample attrition, the follow-up round includes observations from 746 farm households.[1] For the evaluation, we use the balanced panel of 746 observations with complete data for both survey rounds, as this allows us to employ difference-in-difference techniques. Possible issues of attrition are addressed further below.

Data from sample households were collected through face-to-face interviews with the household head and or the spouse using a structured questionnaire. A team of agricultural students and recent graduates from the University of Nairobi assisted in carrying out the interviews in the local language after careful training. The questionnaire captured details of family demographics, agricultural production and marketing, other economic activities of the household, infrastructure and institutional conditions, and other contextual variables. Selected socioeconomic characteristics of the sample are shown in table 1.

*(Table 1 about here)*

**Experimental Design**

Our RCT includes three treatment groups and one control group. The 48 randomly selected farmer groups were randomly assigned to these four alternatives, 12 farmer groups each. Randomization at group level facilitates implementation of the experimental treatments and also reduces potential spillovers (Pamuk, Bulte, and Adekunle 2014).

*Treatment Arms*

Farmers in treatment group 1 received agricultural training, which included information about the agronomic and nutritional attributes of the KK15 bean variety and training on proper cultivation practices. Farmers in treatment group 2 received agricultural training and nutrition training. The aim of the nutrition training was to increase participants' nutrition knowledge through training on human nutritional requirements, food groups and their nutrient composition, eating balanced diets at different life stages, breast feeding practices, and health consequences of nutrient deficiencies. Farmers in treatment group 3 received agricultural training, nutrition training, and marketing training. The marketing training was aimed at enhancing participants' access to markets by increasing their knowledge on the functioning of markets and marketing strategies. It also linked farmers with bean traders through organized forums in which the characteristics of the KK15 varieties were jointly discussed. Farmers in the control group received none of these training elements during the RCT (for reasons of fairness we offered training to control group farmers in 2017, after the follow-up survey data had been collected).

*Treatment Implementation*

The trainings were administered by Africa Harvest agricultural extension officers, who are based in the study region. In order to ensure harmonized delivery of the training contents, we did the following. First, we developed detailed manuals for each of the training components and sessions together with the extension officers. Second, we organized a workshop in which the extension officers were trained to deliver the contents with standardized methods following the manuals. This workshop also involved actual training sessions with farmer groups other than those selected for the RCT and subsequent feedback discussions in the team. Third, for the RCT we assigned extension officers to farmer groups in such a way that each officer had groups in all three treatment arms. This was important to reduce the risk of extension officer bias in evaluating the treatment effects; in spite of standardized training manuals, differences in extension officer personalities may possibly affect farmers' technology adoption behavior.

All training sessions were conducted in the regular meeting places of the farmer groups, following a structured schedule to ensure timely delivery of information. The agricultural training involved a total of seven sessions, the nutrition training involved three sessions, and the marketing training involved three sessions as well. The main training sessions were offered between January and July 2016; a summary refresher session for each of the three training components was offered in August and September 2016. Each training session lasted for about two hours.

Farmers in the treatment groups were invited to the training sessions through the group leader, who was informed and reminded of the particular date and time by the extension officers through phone calls and text messages. For all sessions, farmers and their spouses were encouraged to participate, but the decision to participate was voluntary. Participation in each of the sessions was recorded by the extension officers. In the introductory sessions, farmers were informed about the training elements and time schedule relevant for their particular treatment arm. The first sessions of all three training components (agriculture, nutrition, marketing) were conducted between January and March 2016, so to be relevant already for the March planting season.

Farmers who decided to adopt KK15 could place seed orders through their group leaders. Table 1 shows that there were a few farmers who had adopted KK15 already before the RCT started, but the adoption rate in the total sample was below 1%. As the project timeline was limited, we offered a 30% seed price subsidy to expedite the adoption process.[2] This may mean that the treatment effects are larger than they would be without the subsidy. However, as farmers in all three treatment groups and also in the control group had access to the subsidy, differences in the treatment effects on adoption can be fully attributed to the trainings and not the subsidy.

*Covariate Balancing*

Table 2 presents the covariate balancing tests for assessing the effectiveness of the randomization procedure in terms of delivering comparable groups. For this test we use the baseline data of households in the balanced panel. Except for very few variables where significant differences occur, the baseline characteristics are balanced across the control and treatment groups. This means that randomization bias, which is common in small samples (Barrett and Carter 2010), is not of major concern in our case. Nevertheless, to reduce any possible randomization bias, we rely on difference-in-difference estimators for evaluating the treatment effects. Moreover, we control for baseline differences in the regression models. Details of the estimation procedures are explained further below.

*(Table 2 about here)*

*Attrition*

As mentioned above, the baseline survey included 824 farm household observations, while in the follow-up survey we were only able to revisit 746 of these households. The average attrition rate is about 9%, but there is some variation across treatment and control groups (table A1 in the online appendix). Non-random attrition might bias the randomized design and subsequently the results. Table 2, with data from the balanced panel, suggests that attrition did not introduce significant randomization bias. However, to be on the safe side, we test and control for attrition bias through a weighting procedure. Table A3 in the online appendix shows probit models to analyze the association between attrition and socioeconomic variables for the baseline sample. The full-sample model in the last column of table A3 is used to calculate for each observation the probability to also be included in the follow-up round. These probabilities are used for inverse probability weighting in the difference-in-difference models, relying on the ignorability assumption (Wooldridge 2002).

*Hawthorne and John Henry Effects*

Apart from the treatment effects, experimental designs in randomized evaluations may potentially induce unintended behavioral changes among study participants. Changes in the behavior of the treatment group are called Hawthorne effects, while changes in the behavior of the control group are called John Henry effects (Duflo, Glennerster, and Kremer 2007). For instance, some individuals in the treatment group may be aware that they are being evaluated and may work harder to impress the evaluator. In contrast, some individuals in the control group may feel disappointed that they are not part of the treatment and either start competing with individuals in the treatment group or slack off. Such endogenous behavioral changes may lead to design contamination and possibly affect internal and external validity of the impact estimates.

We employed the following strategy to reduce possible Hawthorne and John Henry effects. First, we used cluster randomization, reducing potential behavioral change across experimental groups by limiting the likelihood of farmer groups knowing the treatments administered in other groups (Duflo et al. 2007). Second, we ensured that the household survey and the experimental treatments were implemented by different persons from different organizations to reduce the possibility of farmers drawing direct linkages between the training sessions and the household interviews. There was also no explicit mention of an evaluation during the implementation of the treatments or the survey interviews.

While farm households in the treatment groups are more likely to see the connection between the treatments and the evaluation (surveys), we feel that the risk of significant Hawthorne effects is small. The reason is that we are interested in the treatment effects on technology adoption, which is associated with a financial cost to farmers, as the KK15 seeds had to be purchased. Farmers in our sample are relatively poor. Hence, even if farmers in the treatment groups realized that they are part of an experiment, they would probably not adopt simply to impress the evaluator. A possible change in behavior might be increased attendance of the training sessions, which could possibly bias the treatment effects downward if training attendees decide not to adopt KK15 seeds. Yet we expect that even the decision to attend the training sessions will probably be made only if the expected utility from attending the training is higher than the expected utility from alternative uses of the time.

## Estimation Strategy

We want to measure the effect of different extension treatments on farmers' adoption of the biofortified bean variety KK15. We use two indicators of technology adoption: (a) adoption of KK15 expressed as a dummy variable that takes a value of one if a household planted KK15 during the study period and zero otherwise; (b) intensity of adoption measured in terms of the percentage share of total cultivated land under KK15.

For both outcome variables, we estimate intent-to-treat (ITT) effects and treatment-on-the-treated (TOT) effects (the TOT effect is also known as local average treatment effect). The ITT effect measures the average effect of being randomly assigned into a treatment group (offer to attend certain training sessions), regardless of whether or not farmers actually attended the training sessions. The TOT effect measures the actual effect of training attendance. The ITT analysis yields precise impact estimates when there is perfect compliance, but when there is non-compliance, ITT effects get diluted and poorly predict average treatment effects (Angrist 2006). We do not observe perfect compliance in our RCT (table A2 in the online appendix), which is why we also estimate TOT effects. The ITT effects are generally more relevant for policymakers because monitoring compliance is difficult outside of experiments. On the other hand, TOT estimates are of interest to researchers to capture actual effects of the treatment itself rather than of the simple offer to be treated (Bloom 2006; Duflo et al. 2007).

For both the ITT and TOT effects, we estimate separate regression models for each of the three treatments, always with the control group observations as the reference. This allows us to compare each treatment group with the control group, while avoiding possible challenges that may arise from estimating a single regression model with multiple endogenous variables, especially in the TOT analysis.

*Estimating Intent-To-Treat Effects*

We estimate the ITT effects using the following difference-in-difference specification:

$$y_{it} = \beta_0 + \beta_1 Post_t + \beta_2 T_j + \beta_3 Post_t \times T_j + \varepsilon_{ijt}, \tag{1}$$

where $y_{it}$ is the outcome variable of interest (KK15 adoption), $Post_t$ is a year dummy variable that takes a value of one for the follow-up data (collected in 2016), and zero for the baseline data (collected in 2015), $T_j$ is a dummy variable that takes a value of one if the farmer group is treated, and zero otherwise (depending on the model, $T_j$ stands for treatment group 1, treatment group 2, or treatment group 3). $\varepsilon_{ijt}$ is the error term, clustered at farmer group level. Subscripts *i, t,* and *j* denote household level observation, time period, and group level observation, respectively.

The parameter of particular interest is $\beta_3$, which is the difference-in-difference estimator of the ITT effect. Under the assumption of parallel trends, which requires the difference between the control and the treatment group to remain constant over time, the difference-in-difference estimator overcomes possible selection bias from the absence of perfect balance in the baseline covariates. This estimator also accounts for time-invariant unobserved heterogeneity (Greene 2012). Equation (1) is estimated with ordinary least squares (OLS). For the binary adoption outcome we use a linear probability model (LPM). While the LPM may generate predicted probabilities outside the unit interval, its marginal effects are generally close to those from non-linear models (Angrist and Pischke 2009).

To control for differences in baseline covariates, we extend the model in equation (1) as follows:

(2) $$y_{it} = \beta_0 + \beta_1 Post_t + \beta_2 T_j + \beta_3 Post_t \times T_j + \delta \boldsymbol{x_{ij}} + \varepsilon_{ijt},$$

where $\boldsymbol{x_{ij}}$ is a vector of socioeconomic controls.

*Estimating Treatment-On-The-Treated Effects*

For estimating the TOT effects, we use actual training attendance as the treatment variable. Since several training sessions were offered and it is possible that farmers participated in some but not all of these sessions, we measure training attendance in two different ways: (a) a dummy variable that takes a value of one if a household attended at least one of the training sessions that were offered in his/her group, and zero otherwise; (b) intensity of training attendance, measured by the number of training sessions attended relative to all training sessions offered in the group (this share can take values between zero and one).

The decision to attend training sessions is endogenous. To avoid endogeneity bias we use an instrumental variables (IV) approach, relying on the random assignment into the treatment groups (offer to attend certain trainings) as a valid instrument for training attendance. Using the randomization status as an instrument is a common approach in the RCT literature (Ashraf, Giné, and Karlan 2009; Carter, Laajaj, and Yang 2013). The TOT effect estimates are unbiased under the following assumptions (Angrist, Imbens, and Rubin 1996; Angrist and Pischke 2009; Ashraf et al. 2009): First, the offer to participate in the treatment is random, which is fulfilled in our case due to random assignment of farmer groups to different treatments. Second, the offer to participate in the treatment is highly correlated with actual training attendance, which is also fulfilled in our case. Third, the offer to participate in the treatment is not correlated with the outcome variables, except through actual attendance of the training sessions. This third assumption is more challenging to test; it can be violated if there are within-group externalities, for instance, if the behavior of non-attendees in the training sessions is affected by the behavior of attendees. Farmer groups are usually designed to facilitate cooperation among members, so that within-group externalities may occur. We will therefore interpret the TOT effect estimates cautiously. However, it is important to note that within-group externalities – if existent – would lead to a downward bias, meaning that the true TOT effects could be larger than the ones estimated with the IV approach.

We estimate the TOT effects using the following IV difference-in-difference specification:

(3) $$y_{it} = \alpha_0 + \alpha_1 Post_t + \alpha_2 \hat{T}_i + \alpha_3 Post_t \times \hat{T}_i + \upsilon_{ijt},$$

where $\hat{T}_i$ is the fitted value of the treatment (actual training attendance) obtained from the first-stage regression with the instrument. $\alpha_3$ is the parameter of interest, and $\upsilon_{ijt}$ is the error term, clustered at farmer group level.

Again, to control for differences in baseline covariates, we extend the model in equation (3) as follows:

(4) $$y_{it} = \alpha_0 + \alpha_1 Post_t + \alpha_2 \hat{T}_i + \alpha_3 Post_t \times \hat{T}_i + \delta \mathbf{x}_{ij} + \upsilon_{ijt}.$$

For the estimation of the models in equations (3) and (4) we apply two-stage least squares (2SLS). Non-linear models, such as IV probit and Tobit could have been used, but these require the endogenous regressors to be continuous (StataCorp 2013). The 2SLS estimator works efficiently and produces estimates with a robust causal interpretation also with limited dependent variables (Angrist 2006).

## Estimation Results

We now present and discuss the results of our analysis following the estimation strategy explained in the previous section.

*Intent-To-Treat Effects on Technology Adoption*

In table 3, we present estimates of the ITT effects for the decision to adopt KK15 bean seeds, as well as for adoption intensity (share of land under KK15). We show models with and without attrition-weighting (later not shown for brevity). The results between both alternatives are similar. In the discussion, we focus on the attrition-weighted results. For each model, we also show estimates with and without baseline controls included: the ITT effects in both specifications are identical, suggesting that the difference-in-difference procedure controls for baseline differences very well.

*(Table 3 about here)*

The results in table 3 show positive and significant effects of all three treatments on the likelihood of KK15 adoption, and also on adoption intensity, suggesting that the extension approaches are effective in terms of increasing the uptake of this pro-nutrition technology. The attrition-weighted ITT estimates in panel (A) of table 3 imply that farmers who were offered agricultural training alone (treatment 1) are 22.5 percentage points more likely to plant KK15 seeds than their colleagues in the control group. The share of land under KK15 is 4.9 percentage points higher. For farmers who were offered agricultural training and nutrition training (treatment 2 shown in panel B of table 3), the likelihood of planting KK15 seeds is 26 percentage points higher than for farmers in the control group. That is, the nutrition training seems to further increase technology adoption over and above the effect of agricultural training alone. However, farmers in treatment 3 (panel C) have a slightly lower likelihood of KK15 adoption than farmers in treatments 1 and 2.

As explained, the ITT results measure the effects of the training offers, without looking at farmers' actual attendance of training sessions. The effects of actual attendance are analyzed in the following.

*Treatment-On-The-Treated Effects on Technology Adoption*

Tables 4 and 5 present the estimated TOT effects. These estimates are computed by comparing farmers who attended the training sessions offered with those who did not attend, including the control group. Table 4 shows results of models where the attendance dummy is used as the treatment variable. The attrition-weighted results suggest that farmers who attended agricultural training only (treatment 1 shown in panel A of table 4) are 22.5 percentage points more likely to adopt KK15 beans than their colleagues who did not attend any of the training sessions. This refers to the model with baseline controls (column 2). The adoption intensity is 4.9 percentage points higher (column 4).

*(Table 4 about here)*

Farmers who attended agricultural and nutrition training (treatment 2 shown in panel B of table 4) are 32.2 percentage points more likely to adopt KK15 than those who did not attend any of the trainings. Their adoption intensity is 6.1 percentage points higher. The comparison of the TOT effects between treatment 1 and treatment 2 suggests that attendance of nutrition training increases KK15 adoption by 9.7 percentage points and adoption intensity by 1.2 percentage points over and above attendance of agricultural training alone. The TOT effects in panel (C) of table 4 are very similar to those in panel (B), which may imply that attending marketing training may not have an additional effect on adoption over and above agricultural and nutrition training.

Table 5 shows results of the TOT effects models with intensity of training attendance as the treatment variable. As explained, intensity is measured in terms of the share of training sessions attended with values ranging between zero and one. The attrition-weighted estimates with baseline controls in panel (A) suggest that farmers who attended all of the agricultural training sessions offered in treatment 1 are 40.9 percentage points more likely to adopt KK15 beans than farmers who did not attend any of the training session. Full attendance of all agricultural training sessions increases the adoption intensity by 7.3 percentage points.

*(Table 5 about here)*

In panel (B) of table 5, we observe that farmers who attended all of the agricultural and nutrition training sessions offered in treatment 2 are 52.8 percentage points more likely to adopt KK15 beans than their colleagues who attended none of the sessions. This TOT effect on adoption is almost 12 percentage points higher than that in treatment 1, further supporting the hypothesis that nutrition training components can increase the effectiveness of extension for pro-nutrition technologies. In terms of the adoption intensity, this difference is not visible.

In panel (C) of table 5, the treatment effect on adoption is similar as in panel (B), suggesting that attending the additional marketing training does not make a major difference for the adoption decision. However, the TOT effect on adoption intensity in treatment 3 is somewhat higher than in the other two treatments. This makes sense, because marketing training is particularly relevant when a marketable surplus is produced, which is more likely when a larger share of the farm area is cultivated with KK15 beans.

As expected, most of the estimated TOT effects are larger than the ITT effects, which is especially true when using the intensity of training attendance as the treatment variable. This comparison implies that the ITT estimates were affected by non-compliance problems.

## Conclusion

In this article, we have analyzed how agricultural extension can be improved to increase the adoption of pro-nutrition technologies by smallholder farm households. In particular, we have studied how agricultural training can be combined with nutrition training and marketing training to increase the adoption of KK15, a new variety of beans biofortified with iron and zinc. Different extension treatments were implemented in an RCT with smallholder farm households in Western Kenya. Treatment effects were estimated with difference-in-difference models, using data from baseline and follow-up surveys.

Results show that intensive training offered by agricultural extension officers and tailored to local conditions can increase technology adoption considerably within a relatively short period of time. In all three treatments, the adoption of KK15 increased from less than 1% before the RCT started to more than 20% one year later. This rapid increase in adoption in the treatment groups suggests that farmers are willing to adopt pro-nutrition technologies, when they are well informed about the attributes and their implications, even when the technologies are not primarily designed to increase productivity and income. Even though farmers in the RCT received a 30% subsidy on the price of KK15 seeds, they had to pay for the seeds and therefore made a real adoption decision considering expected benefits and costs.

Comparison of the different treatments revealed interesting additional insights. Farmers who had received agricultural training and nutrition training were more likely to adopt KK15 than farmers who had only received agricultural training. Comparison of the TOT effects suggests that additional nutrition training further increased adoption rates by 10-12 percentage points over and above the effects of agricultural training alone. This additional effect of nutrition training may not surprise, because of the positive nutritional attributes of KK15. However, it should be noted that these attributes of KK15 were communicated to farmers in the agricultural training sessions. The nutrition training sessions covered broader aspects related to healthy nutrition, balanced diets, and the health consequences of nutrient deficiencies. It seems that knowledge about these broader nutrition aspects has helped farmers to better appreciate the nutrition attributes of KK15, thus resulting in higher adoption rates. The nutrition training may certainly have positive effects on household diets and health beyond KK15 adoption. Analysis of such wider effects is beyond the scope of this study.

Our findings have important policy implications. Nutrition education is usually not delivered through the agricultural extension service, but through specialized nutrition and health workers. Our results suggest that combining agricultural and nutrition training in agricultural extension approaches is feasible. Of course, the nutrition training should be designed together with nutrition experts, and the agricultural extension officers first need to be trained themselves before they can effectively deliver nutrition training to farm families. However, the high personnel and logistics cost of reaching out to families in rural areas is a major impediment for more widespread coverage of nutrition and health education campaigns. Based on our results we argue that closer cooperation between agricultural extension and nutrition and health organizations can be a cost-effective way to promote pro-nutrition innovations among smallholder farm households.

The additional marketing training provided in one of the treatment arms of our RCT did not contribute to higher KK15 adoption over and above the effects of agricultural and nutrition training. This is surprising because research has shown that improved market access can improve technology adoption in the small farm sector (Fischer and Qaim 2012). That the marketing training did not have an additional effect in our study may be due to the fact that we only considered adoption during the one year in which the training sessions were implemented. During this very early period of KK15 adoption, most of the adopting farmers planted small areas with the new variety, in order to test out the technology’s attributes. The small quantities harvested were primarily consumed at home and not marketed. It is possible that the marketing training will have larger effects when farmers consider increasing the area cultivated with KK15 at a later stage. Indeed, in some of the TOT models we found that the marketing treatment had a significantly positive additional effect on adoption intensity.

The study region in Western Kenya with very small farm sizes, diverse production systems, limited market access due to infrastructure constraints, and relatively high rates of malnutrition is typical for the African small farm sector. Hence, some of the general findings will also be relevant beyond this specific setting. However, the exact estimates of the treatment effects should not be generalized. There are particularly two factors in our RCT that may reduce the external validity of the empirical estimates. First, our extension treatments were fairly intense. Within a period of nine months, farmers in all treatment groups were offered seven agricultural training sessions. In some of the treatment groups, three nutrition training and three marketing training sessions were offered in addition. Outside of an experiment, the training frequency and intensity may be lower, meaning that the effects on technology adoption may be lower too. Second, we only analyzed the short-term adoption effects, as the follow-up survey was carried out less than one year after the treatments had started. Technology adoption is a process over time. Most farmers seemed to be satisfied with KK15 during the first year of adoption, so it is likely that adoption rates will further increase in the future, among both treated and untreated farmers. Further research is needed on how the design of agricultural extension approaches can be improved in order to increase the adoption of pro-nutrition technologies. Our study is only an initial step in this direction.

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**Table 1. Selected Socioeconomic Characteristics of Sample Households at Baseline**

| Variables | Full sample | Treatment [a] | Control |
|---|---|---|---|
| Age of household head (years) | 49.483 | 49.980 | 47.984 |
| | (12.440) | (12.697) | (11.538) |
| Male household head (dummy) | 0.765 | 0.730 | 0.871 |
| | (0.424) | (0.444) | (0.336) |
| Education of household head (years) | 8.924 | 8.750 | 9.446 |
| | (3.732) | (3.796) | (3.490) |
| Farm size (acres) | 1.600 | 1.623 | 1.532 |
| | (1.253) | (1.309) | (1.067) |
| Number of crop and livestock species produced | 12.805 | 12.968 | 12.314 |
| | (4.625) | (4.694) | (4.387) |
| KK15 adopter (dummy) | 0.008 | 0.011 | 0.000 |
| | (0.089) | (0.103) | (0.000) |
| Observations | 746 | 560 | 186 |

*Notes*: Mean values are shown with standard deviations in parentheses. [a] Treatment includes all farm households randomly assigned to one of the treatment groups.

**Table 2. Mean Differences between Treatment and Control Groups at Baseline**

| Variables | Control – Treatment 1 | Control – Treatment 2 | Control – Treatment 3 | Control – All Treatments |
|---|---|---|---|---|
| Age of household head (years) | -3.885* | -0.594 | -1.437 | -1.996 |
| | (1.885) | (2.265) | (2.190) | (1.736) |
| Male household head (dummy) | 0.113 | 0.193* | 0.118* | 0.141** |
| | (0.078) | (0.105) | (0.063) | (0.054) |
| Education of household head (years) | 1.015** | 0.280 | 0.773* | 0.696** |
| | (0.472) | (0.559) | (0.400) | (0.332) |
| Household size (count) | 0.473 | 0.379 | 0.536* | 0.464* |
| | (0.348) | (0.268) | (0.279) | (0.257) |
| Risk attitude (scale 0 to 10) | 0.136 | 0.062 | 0.510* | 0.239 |
| | (0.292) | (0.254) | (0.261) | (0.203) |
| Farm size (acres) | -0.088 | -0.127 | -0.060 | -0.091 |
| | (0.236) | (0.224) | (0.195) | (0.177) |
| Land title deed (dummy) | 0.012 | -0.044 | 0.017 | -0.004 |
| | (0.048) | (0.059) | (0.055) | (0.045) |
| Farm productive assets (1,000 Ksh) | 7.962 | 1.738 | 0.241 | 2.061 |
| | (9.629) | (12.655) | (13.114) | (9.730) |
| Own motorcycle (dummy) | -0.040 | -0.003 | 0.012 | -0.010 |
| | (0.030) | (0.027) | (0.030) | (0.022) |
| Access to credit (dummy) | -0.073 | 0.002 | 0.037 | -0.012 |
| | (0.055) | (0.057) | (0.058) | (0.049) |
| Distance to main market (km) | -0.410 | -0.841 | 0.633 | -0.195 |
| | (0.782) | (0.987) | (0.760) | (0.688) |
| Distance to extension office (km) | -0.312 | -0.072 | 0.398 | 0.006 |
| | (0.700) | (0.569) | (0.735) | (0.522) |
| Number of groups (count) | 0.044 | -0.012 | 0.081 | 0.039 |
| | (0.073) | (0.079) | (0.067) | (0.059) |
| Group official (dummy) | -0.019 | -0.051 | 0.065 | -0.000 |
| | (0.055) | (0.061) | (0.048) | (0.046) |
| Knows KK15 attributes (dummy) [a] | 0.006 | -0.013 | 0.011 | 0.002 |
| | (0.024) | (0.029) | (0.027) | (0.021) |
| Knows KK15 attributes (score) | 0.000 | -0.013 | 0.004 | -0.003 |
| | (0.011) | (0.015) | (0.012) | (0.010) |
| KK15 adopter (dummy) | -0.005 | -0.022 | 0.000 | -0.009 |
| | (0.005) | (0.016) | (0.000) | (0.006) |
| Land area under KK15 (acres) | -0.000 | -0.012 | 0.000 | -0.004 |
| | (0.000) | (0.008) | (0.000) | (0.003) |
| Share of land under KK15 (%) | -0.055 | -0.408 | 0.000 | -0.150 |
| | (0.054) | (0.297) | (0.000) | (0.102) |
| Seed expenditure (Ksh/acre) | 424.289 | -315.417 | 520.061 | 219.020 |
| | (487.950) | (572.126) | (408.549) | (408.916) |
| Fertilizer expenditure (Ksh/acre) | 547.114 | -794.912 | 652.372 | 151.461 |
| | (452.998) | (468.471) | (580.048) | (404.608) |
| Value of crop output per acre (1,000 Ksh) | 1.977 | -7.401 | -6.865 | -4.037 |
| | (8.949) | (8.825) | (7.586) | (6.507) |
| Household income (1,000 Ksh) | 14.548 | 3.321 | -15.556 | 0.725 |
| | (31.039) | (25.625) | (26.623) | (20.460) |
| Observations | 376 | 366 | 376 | 746 |

*Notes*: Treatment 1, agricultural training. Treatment 2, agricultural training plus nutrition training. Treatment 3, agricultural training plus nutrition training plus marketing training. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

**Table 3. Effects of Extension Treatments on Technology Adoption, Intent-To-Treat Estimates**

| Variables | Attrition-weighted results | | | |
|---|---|---|---|---|
| | Planted KK15 (dummy) | | Share of land under KK15 (%) | |
| | (1) | (2) | (3) | (4) |
| *Panel A: Treatment 1 (n=752)* | | | | |
| Post (dummy) | 0.004 | 0.004 | 0.037 | 0.037 |
| | (0.004) | (0.004) | (0.037) | (0.037) |
| Treatment 1 (dummy) | 0.006 | 0.011 | 0.058 | 0.142 |
| | (0.005) | (0.017) | (0.053) | (0.437) |
| Post × Treatment 1 | 0.225** | 0.225** | 4.929** | 4.929** |
| | (0.074) | (0.082) | (1.989) | (2.004) |
| Baseline controls | No | Yes | No | Yes |
| R-squared | 0.163 | 0.175 | 0.096 | 0.113 |
| *Panel B: Treatment 2 (n=732)* | | | | |
| Post (dummy) | 0.004 | 0.004 | 0.037 | 0.037 |
| | (0.004) | (0.004) | (0.037) | (0.037) |
| Treatment 2 (dummy) | 0.023 | 0.020 | 0.429 | 0.393 |
| | (0.017) | (0.013) | (0.300) | (0.245) |
| Post × Treatment 2 | 0.261*** | 0.261*** | 4.814*** | 4.814*** |
| | (0.075) | (0.075) | (1.318) | (1.328) |
| Baseline controls | No | Yes | No | Yes |
| R-squared | 0.207 | 0.227 | 0.118 | 0.129 |
| *Panel C: Treatment 3 (n=752)* | | | | |
| Post (dummy) | 0.004 | 0.004 | 0.037 | 0.037 |
| | (0.004) | (0.004) | (0.037) | (0.037) |
| Treatment 3 (dummy) | 0.005 | 0.004 | 0.042 | -0.002 |
| | (0.005) | (0.011) | (0.041) | (0.212) |
| Post × Treatment 3 | 0.214*** | 0.214*** | 4.443*** | 4.443*** |
| | (0.052) | (0.052) | (1.454) | (1.465) |
| Baseline controls | No | Yes | No | Yes |
| R-squared | 0.165 | 0.192 | 0.104 | 0.125 |

*Notes*: Coefficient estimates are shown with robust standard errors clustered at farmer group level in parentheses. Post, dummy variable which takes a value of 1 for follow-up round observations (after treatment), and zero for baseline observations. Treatment 1, agricultural training. Treatment 2, agricultural training plus nutrition training. Treatment 3, agricultural training plus nutrition training plus marketing training. Baseline controls include age, gender, education, risk attitude, household size, farm size, value of productive assets, access to credit, distance to market, group official, and county dummy.*, **, and *** denote significance at the 10%, 5%, and 1% level, respectively. We do not show results without attrition weighting for brevity.

**Table 4. Effects of Extension Treatments on Technology Adoption, Treatment-On-The-Treated Estimates (IV Results with Training Attendance Dummies as Treatment Variables)**

| Variables | Attrition-weighted results | | | |
|---|---|---|---|---|
| | Planted KK15 (dummy) | | Share of land under KK15 (%) | |
| | (1) | (2) | (3) | (4) |
| *Panel A: Treatment 1 (n=752)* | | | | |
| Post (dummy) | 0.025 | 0.027 | 0.454 | 0.577 |
| | (0.016) | (0.017) | (0.323) | (0.403) |
| Treatment 1 (dummy) | 0.026* | 0.035 | 0.464 | 0.698 |
| | (0.015) | (0.029) | (0.288) | (0.750) |
| Post × Treatment 1 | 0.229*** | 0.225** | 5.093** | 4.861** |
| | (0.085) | (0.088) | (2.122) | (2.155) |
| Baseline controls | No | Yes | No | Yes |
| R-squared | 0.190 | 0.200 | 0.113 | 0.127 |
| *Panel B: Treatment 2 (n=732)* | | | | |
| Post (dummy) | 0.015* | 0.013 | 0.173 | 0.127 |
| | (0.009) | (0.008) | (0.109) | (0.099) |
| Treatment 2 (dummy) | 0.047 | 0.043 | 0.771* | 0.738* |
| | (0.030) | (0.024) | (0.464) | (0.437) |
| Post × Treatment 2 | 0.316*** | 0.322*** | 6.014*** | 6.149*** |
| | (0.084) | (0.088) | (1.669) | (1.792) |
| Baseline controls | No | Yes | No | Yes |
| R-squared | 0.279 | 0.299 | 0.163 | 0.175 |
| *Panel C: Treatment 3 (n=752)* | | | | |
| Post (dummy) | 0.002 | 0.002 | 0.019 | 0.008 |
| | (0.002) | (0.004) | (0.019) | (0.097) |
| Treatment 3 (dummy) | 0.004 | 0.003 | 0.035 | -0.033 |
| | (0.008) | (0.020) | (0.064) | (0.421) |
| Post × Treatment 3 | 0.317*** | 0.317*** | 6.500*** | 6.533*** |
| | (0.066) | (0.069) | (1.842) | (1.905) |
| Baseline controls | No | Yes | No | Yes |
| R-squared | 0.268 | 0.293 | 0.167 | 0.187 |

*Notes*: Coefficient estimates are shown with robust standard errors clustered at farmer group level in parentheses. Post, dummy variable which takes a value of 1 for follow-up round observations (after treatment), and zero for baseline observations. Treatment 1, agricultural training. Treatment 2, agricultural training plus nutrition training. Treatment 3, agricultural training plus nutrition training plus marketing training. Baseline controls include age, gender, education, risk attitude, household size, farm size, value of productive assets, access to credit, distance to market, group official, and county dummy.*, **, and *** denote significance at the 10%, 5%, and 1% level, respectively. We do not show results without attrition weighting for brevity.

**Table 5. Effects of Extension Treatments on Technology Adoption, Treatment-On-The-Treated Estimates (IV Results with Intensity of Training Attendance as Treatment Variables)**

| Variables | Attrition-weighted results | | | |
|---|---|---|---|---|
| | Planted KK15 (dummy) | | Share of land under KK15 (%) | |
| | (1) | (2) | (3) | (4) |
| *Panel A: Treatment 1 (n=752)* | | | | |
| Post (dummy) | 0.009 | 0.012 | 0.678 | 0.749 |
| | (0.026) | (0.027) | (0.596) | (0.642) |
| Treatment 1 (share) | 0.018 | 0.036 | 1.089 | 1.447 |
| | (0.041) | (0.062) | (1.842) | (1.450) |
| Post × Treatment 1 | 0.417*** | 0.409*** | 7.527*** | 7.309** |
| | (0.136) | (0.148) | (2.792) | (3.017) |
| Baseline controls | No | Yes | No | Yes |
| R-squared | 0.261 | 0.272 | 0.135 | 0.150 |
| *Panel B: Treatment 2 (n=732)* | | | | |
| Post (dummy) | 0.023 | 0.021 | 1.043 | 0.988 |
| | (0.024) | (0.022) | (0.781) | (0.721) |
| Treatment 2 (share) | 0.103 | 0.108 | 3.615* | 3.734** |
| | (0.072) | (0.066) | (2.048) | (1.868) |
| Post × Treatment 2 | 0.518*** | 0.528*** | 6.109*** | 6.392*** |
| | (0.113) | (0.130) | (2.091) | (2.219) |
| Baseline controls | No | Yes | No | Yes |
| R-squared | 0.348 | 0.370 | 0.145 | 0.159 |
| *Panel C: Treatment 3 (n=752)* | | | | |
| Post (dummy) | 0.016 | 0.018 | 0.638 | 0.660 |
| | (0.020) | (0.019) | (0.563) | (0.594) |
| Treatment 3 (share) | 0.048 | 0.053 | 1.823 | 1.828 |
| | (0.052) | (0.055) | (1.544) | (1.806) |
| Post × Treatment 3 | 0.550*** | 0.540*** | 9.422** | 9.294** |
| | (0.124) | (0.131) | (3.809) | (4.127) |
| Baseline controls | No | Yes | No | Yes |
| R-squared | 0.354 | 0.370 | 0.190 | 0.205 |

*Notes*: Coefficient estimates are shown with robust standard errors clustered at farmer group level in parentheses. Post, dummy variable which takes a value of 1 for follow-up round observations (after treatment), and zero for baseline observations. Treatment 1, agricultural training. Treatment 2, agricultural training plus nutrition training. Treatment 3, agricultural training plus nutrition training plus marketing training. Baseline controls include age, gender, education, risk attitude, household size, farm size, value of productive assets, access to credit, distance to market, group official, and county dummy.*, **, and *** denote significance at the 10%, 5%, and 1% level, respectively. We do not show results without attrition weighting for brevity.

**NB: We apologize for not showing Online Appendix Tables A1-A3 for brevity**