European Review of Agricultural Economics pp. 1–24
doi:10.1093/erae/jby049

How to make farming and agricultural extension more nutrition-sensitive: evidence from a randomised controlled trial in Kenya

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Received June 2018; editorial decision November 2018; final version accepted November 2018

Review coordinated by Carl Johan Lagerkvist

Abstract

We analyse how agricultural extension can be made more effective in terms of increasing farmers' adoption of pro-nutrition technologies, such as biofortified crops. In a randomised controlled trial with farmers in Kenya, we implemented several extension treatments and evaluated their effects on the adoption of beans biofortified with iron and zinc. Difference-in-difference estimates show that intensive agricultural training can increase technology adoption considerably. Additional nutrition training helps farmers to better appreciate the technology's nutritional benefits and thus further increases adoption. This study is among the first to analyse how improved extension designs can help to make smallholder farming more nutrition-sensitive.

Keywords: agricultural extension, technology adoption, biofortification, nutrition-sensitive agriculture, Kenya

JEL classification: C93, O33, Q12, Q16, Q18

1. Introduction

Hunger and micronutrient malnutrition remain widespread public health problems in many developing countries (IFPRI, 2017). Many of the people affected live in smallholder farm households. Hence, the question of how smallholder farming can be made more nutrition-sensitive is ranking high on the development policy agenda (Pingali and Sunder, 2017; Ruel, Quisumbing and Balagamwala, 2018). The importance of market access for improving

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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). In addition, agricultural technologies specifically designed to improve nutrition could possibly play a critical role. Prominent examples of such pro-nutrition technologies are biofortified crops, which were bred to contain higher amounts of micronutrients, such as orange-fleshed sweet potatoes enhanced with provitamin A or high-iron rice and wheat (Jones and de Brauw, 2015; Bouis and Saltzman, 2017). 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 (Ruel, Quisumbing and Balagamwala, 2018).

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 they 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 the market potential of 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, 2015; de Groote *et al.*, 2016; de Brauw *et al.*, 2018). This implies that training and extension could play a prominent role in technology dissemination.[1] 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. In this article, we use data from a randomised controlled trial (RCT) in Kenya to analyse how agricultural extension can be designed to promote the uptake of pro-nutrition technologies. In particular, we evaluate how agricultural training can be combined with nutrition training and marketing training to increase farmers' adoption of a new bean variety biofortified with iron and zinc.

A few existing studies combined agricultural and nutrition training to promote the uptake of biofortified crops (Hotz *et al.*, 2012; de Brauw *et al.*, 2018). Our study adds to this existing research in two important ways. First, previous studies combined agricultural and nutrition training components in one single intervention, hence they were not able to evaluate the effects that the nutrition training component may have on top of the agricultural training. With our experimental design, we are able to do such kind of evaluation.

1 Besides extension, farmer-to-farmer exchange of knowledge can also influence technology adoption significantly (Conley and Udry, 2010; Foster and Rosenzweig, 2010).

Second, our RCT additionally includes a marketing training component, which previous studies did not. While the concrete results of our research are specific to the study area in Kenya, the lessons to be learned are broader, because such a type of research has not been carried out previously anywhere in the world. In a recent review, Ruel, Quisumbing and Balagamwala (2018) pointed out that more research is needed to understand the cost-effectiveness of nutrition-sensitive agriculture interventions. Comparing the effectiveness of different intervention components, as we do here, is an important first step to design cost-effective intervention packages for upscaling.

The name of the biofortified bean variety used in our RCT is KK15. This variety was bred by the Kenya Agricultural and Livestock Research Organization (KALRO) with conventional breeding methods. Like most bean varieties commonly grown in Kenya, KK15 is a bush bean variety. Its main difference from other common beans is that KK15 contains six times higher amounts of iron and about twice higher amounts of zinc, as a laboratory analysis that we commissioned confirmed. According to KALRO, KK15 is high yielding, resistant to root-rot disease and matures earlier than most other varieties. This means that – on top of the nutritional advantages – there may also be direct economic benefits for adopting farmers. However, KK15 beans are black in colour, whereas most popular bean varieties in Kenya are red. Probably because of this notable difference in outward appearance, widespread adoption of KK15 has not yet occurred 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 includes agricultural training as well, but 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 includes agricultural and nutrition training, but further adds marketing training. In the marketing training, simple mechanisms of market functioning and possible sales strategies were explained. In addition, farmers were linked with bean traders to facilitate the marketing of KK15. All treatments (trainings) were randomly assigned to farmers without targeting particular types of households (see the following for further details). The three treatments are compared with a control group of farmers that did not receive any of these trainings during the RCT, to evaluate the effects of the different extension designs on KK15 adoption.[2]

The remainder of this article is organised as follows. Section 2 describes the empirical setting and the sampling framework for the household survey

2 For ethical reasons, we offered training to control group farmers after the data for the evaluation had been collected.

and experiment. Sections 3 and 4 describe the experimental design and the strategy to estimate the treatment effects. Estimation results are presented and discussed in Section 5. Section 6 concludes.

2. Empirical setting

This study builds on an RCT carried out with smallholder farmers in Western Kenya. Smallholder agriculture accounts for nearly 75 per cent of total agricultural production in Kenya (Olwande *et al.*, 2015). Adoption of improved technologies is relatively low; poverty and malnutrition are widespread in the small farm sector (KNBS, 2015; Wainaina, Tongruksawattana and Qaim, 2016). 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.

2.1. Study region

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 organisation 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, typically producing a number of food crops, such as maize, beans, bananas and different vegetables. Many also produce cash crops such as tea and coffee and keep small herds of livestock. 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 favourable climatic conditions, seasonal boundaries are not very clearcut. In terms of nutritional indicators, Kisii and Nyamira are similar to the national average (KNBS, 2015).

2.2. Sampling strategy

Traditionally, agricultural extension was often implemented through extension officers who visited individual farmers to provide advice on specific topics (Anderson and Feder, 2004). 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). Many farmers in Kisii and Nyamira are organised 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, but excluded groups that had received specific development support during the previous two years. From the remaining groups, we randomly selected 48 farmer groups for inclusion in the study. Of these 48 groups, 32 are located in Kisii and 16 in Nyamira. Farmer groups in our sample have between 20 and 50 active members.

2.3. Farm household survey

In each of the 48 selected farmer groups, we randomly selected 20 members 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 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. For the evaluation, we use a 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 or the spouse using a structured questionnaire. The questionnaire captured details of family demographics, agricultural production and marketing, other economic activities of the household, institutional conditions and contextual variables.

3. Experimental design

Our RCT includes three treatment arms and one control. As mentioned, the first treatment arm (T1) only includes agricultural training. The second treatment arm (T2) includes agricultural plus nutrition training, while the third (T3) includes agricultural plus nutrition plus marketing training. The 48 selected farmer groups were randomly assigned to the treatment arms and control, hence 12 farmer groups each. Randomisation at group level facilitates implementation of the treatments and reduces potential spillovers (Duflo, Glennerster and Kremer, 2007).

One caveat that should be mentioned is that 12 farmer groups per treatment arm is a relatively small number to identify treatment effects on a binary outcome variable, such as technology adoption. Power calculations showed that a minimum of 40 farmer groups per treatment would have been required to identify a 10 percentage point treatment effect (at a 0.05 significance level) with a power of 80 per cent. This would have implied a total number of 160 groups for the three treatment arms and the control. All of our treatments involve multiple training sessions. Moreover, farmer groups are located in quite some distance from each other with poor road conditions. Hence, doing the RCT with 160 groups would have been well beyond the available project budget. Fortunately, the treatment effects are all sufficiently large, so the estimates are statistically significant even with our smaller number of groups.

However, statistical power turns out to be a limitation when comparing the effects between the different treatment arms. While we find sizeable differences between some of the treatments, not all of these differences are statistically significant. Probably some of these differences would be significant with a larger number of groups included. Against this background, some of the results are only suggestive. Nevertheless, even this suggestive evidence can add to the research direction and encourage follow-up work, as – to our knowledge – an RCT design that compares different extension approaches has not been implemented before.

3.1. Treatment implementation

The trainings were administered by Africa Harvest's agricultural extension officers. In order to ensure harmonised delivery of the training contents, we did the following. First, we developed detailed manuals for each of the training components together with the extension officers. Second, we organised a workshop in which the extension officers were trained to deliver the contents with standardised 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.

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 2 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 and marketing) were conducted between January and March 2016, so as to be relevant for the March planting season.

Farmers who decided to adopt KK15 could place seed orders through their group leader, who subsequently informed the extension officers.[3] The extension officers arranged delivery of the ordered seeds to the group leader's house, from where ordering farmers could pick up the seeds against cash payment. Very few farmers had adopted KK15 before the RCT started: the adoption rate in the total sample was below 1 per cent (Table A1 in the Appendix in supplementary data at *ERAE* online). As the project timeline was limited, we offered a 30 per cent seed price subsidy to expedite the adoption process.[4] This may mean that the treatment effects are larger than they would be without the subsidy. However, all farmers had access to the subsidy, including those in the control group. Hence, the treatment effects on adoption will mostly be due to the trainings (perhaps in combination with the subsidy), not the subsidy alone.

3.2. Covariate balancing

Table 1 shows the descriptive statistics and presents covariate balancing tests for assessing the effectiveness of the randomisation 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, the baseline characteristics are balanced across the control and treatment groups. This means that randomisation bias, which is common in small samples (Barrett and Carter, 2010), is not a major concern here. Nevertheless, to reduce any possible randomisation 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 next.

3 Participation in the training sessions was not a precondition for placing seed orders so that farmers who learned about the technology from other group members could also adopt.

4 As ordered seeds were delivered to the group leader's house, transportation costs for farmers were lower than when buying seeds from a seed dealer in the next town. Hence, the effective subsidy was even somewhat higher than 30 per cent. However, for common varieties of beans, farmers mostly use farm-saved seeds or seeds obtained from neighbours and friends. Against this background, even with the 30 per cent subsidy, adoption of KK15 seeds was more expensive for farmers than using other varieties of beans. The widespread use of farm-saved seeds for beans is also an important reason why establishing input value chains with private sector suppliers is challenging. In our project, the seeds were supplied by KALRO, a public sector institute. The nutritional traits (high iron and high zinc) are hereditary, so they will also be present if farm-saved KK15 seeds are used in subsequent seasons.

Table 1. Mean differences between treatment and control groups at baseline

Variables	Mean (full sample)	Control – Treatment 1	Control – Treatment 2	Control – Treatment 3	Control – All Treatments
Age of household head (years)	49.483	−3.885*	−0.594	−1.437	−1.996
	(12.440)	(1.885)	(2.265)	(2.190)	(1.736)
Male household head (dummy)	0.765	0.113	0.193*	0.118*	0.141**
	(0.424)	(0.078)	(0.105)	(0.063)	(0.054)
Education of household head (years)	8.924	1.015**	0.280	0.773*	0.696**
	(3.732)	(0.472)	(0.559)	(0.400)	(0.332)
Household size (count)	5.625	0.473	0.379	0.536*	0.464*
	(2.062)	(0.348)	(0.268)	(0.279)	(0.257)
Risk attitude (scale 0 to 10)	6.788	0.136	0.062	0.510*	0.239
	(2.528)	(0.292)	(0.254)	(0.261)	(0.203)
Farm size (acres)	1.600	−0.088	−0.127	−0.060	−0.091
	(1.253)	(0.236)	(0.224)	(0.195)	(0.177)
Land title deed (dummy)	0.310	0.012	−0.044	0.017	−0.004
	0.463	(0.048)	(0.059)	(0.055)	(0.045)
Farm productive assets (1,000 Ksh)	35.238	7.962	1.738	0.241	2.061
	(108.571)	(9.629)	(12.655)	(13.114)	(9.730)
Own motorcycle (dummy)	0.088	−0.040	−0.003	0.012	−0.010
	(0.284)	(0.030)	(0.027)	(0.030)	(0.022)
Access to credit (dummy)	0.783	−0.073	0.002	0.037	−0.012
	(0.413)	(0.055)	(0.057)	(0.058)	(0.049)
Distance to main market (km)	4.744	−0.410	−0.841	0.633	−0.195
	(5.974)	(0.782)	(0.987)	(0.760)	(0.688)
Distance to extension office (km)	4.408	−0.312	−0.072	0.398	0.006
	(4.976)	(0.700)	(0.569)	(0.735)	(0.522)
Number of groups (count)	1.320	0.044	−0.012	0.081	0.039
	(0.602)	(0.073)	(0.079)	(0.067)	(0.059)
Group official (dummy)	0.361	−0.019	−0.051	0.065	−0.000
	(0.480)	(0.055)	(0.061)	(0.048)	(0.046)
Knows KK15 attributes (dummy)[a]	0.042	0.006	−0.013	0.011	0.002
	(0.200)	(0.024)	(0.029)	(0.027)	(0.021)
Knows KK15 attributes (score)	0.017	0.000	−0.013	0.004	−0.003
	(0.100)	(0.011)	(0.015)	(0.012)	(0.010)
KK15 adopter (dummy)	0.008	−0.005	−0.022	0.005	−0.011
	(0.089)	(0.005)	(0.016)	(0.005)	(0.006)
Land area under KK15 (acres)	0.003	−0.000	−0.012	0.000	−0.004
	(0.045)	(0.000)	(0.008)	(0.000)	(0.003)
Share of land under KK15 (%)	0.124	−0.055	−0.408	0.043	−0.165
	(1.436)	(0.054)	(0.297)	(0.043)	(0.102)
Seed expenditure (Ksh/acre)	3173.393	424.289	−315.417	520.061	219.020
	(3925.614)	(487.950)	(572.126)	(408.549)	(408.916)

(*continued*)

Table 1. (*continued*)

Variables	Mean (full sample)	Control – Treatment 1	Control – Treatment 2	Control – Treatment 3	Control – All Treatments
Fertiliser expenditure	6186.846	547.114	−794.912	652.372	151.461
(Ksh/acre)	(5374.244)	(452.998)	(468.471)	(580.048)	(404.608)
Value of crop output	75.515	1.977	−7.401	−6.865	−4.037
(1,000 Ksh/acre)	(78.791)	(8.949)	(8.825)	(7.586)	(6.507)
Household income	179.716	14.548	3.321	−15.556	0.725
(1,000 Ksh)	(214.738)	(31.039)	(25.625)	(26.623)	(20.460)
Observations	746	376	366	376	746

Note: 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 per cent, 5 per cent and 1 per cent levels, respectively.

3.3. Attrition

The average attrition rate between baseline and follow-up survey was 9 per cent, with some variation across treatment and control groups (Table A2 in the Appendix in supplementary data at *ERAE* online). Non-random attrition might possibly bias the results. Table 1, with data from the balanced panel, suggests that attrition did not introduce significant randomisation bias. However, to be on the safe side, we test and control for attrition bias through a weighting procedure. Table A3 (in the Appendix in supplementary data at *ERAE* online) shows probit models to analyse 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 (Wooldridge, 2002).[5]

3.4. Possible unintended effects and spillovers

Apart from the treatment effects, experimental designs in randomised evaluations may potentially induce unintended behavioural changes among study participants. Changes in the behaviour of the treatment group are called Hawthorne effects, while changes in the behaviour 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

5 In the results section, we concentrate on the treatment effects with attrition weighting. However, for comparison we also show the treatment effects without attrition weighting in Table A11 (in the Appendix in supplementary data at *ERAE* online). Both sets of estimates are very similar, which also underlines that the attrition weighting procedure itself did not introduce any new bias.

part of the treatment and either start competing with individuals in the treatment group or slack off. Also, knowledge spillovers from treatment groups to the control may occur in principle. Such endogenous behavioural changes may lead to design contamination and possibly affect internal and external validity of the impact estimates.

We employed the following strategy to reduce such unintended effects. First, we used cluster randomisation, thus reducing potential spillovers and behavioural changes across experimental groups by limiting the likelihood of farmer groups knowing the treatments administered in other groups (Duflo, Glennerster and Kremer, 2007).[6] Second, we ensured that the household survey and the experimental treatments were implemented by different persons from different organisations 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.

4. 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: (i) 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; (ii) intensity of adoption measured in terms of the percentage share of total cultivated land under KK15.

A simple way to estimate the treatment effects would be a model of the following type that only includes observations from the follow-up survey:

$$y_i = \beta_0 + \beta_1 T_1 + \beta_2 T_2 + \beta_3 T_3 + \varepsilon_i \tag{1}$$

where y_i is KK15 adoption of farm household i, T_1, T_2 and T_3 are treatment dummies for the three treatment arms, β_1, β_2 and β_3 are the estimated treatment effects, and ε_i is a random error term clustered at farmer group level. Equation (1) is estimated with ordinary least squares (OLS).

However, the cross-section model in equation (1) has several drawbacks, as it does not account for possible unobserved heterogeneity and non-zero adoption status at baseline. Therefore, we also develop and estimate difference-in-difference estimators using the baseline and follow-up data, as explained further below.

Another relevant question is how to exactly define the treatment variables. When defining treatment simply as being a member of a farmer group that was randomly assigned to a treatment arm, we result in the intent-to-treat (ITT) effect. The ITT effect does not account for possible non-compliance,

6 As mentioned, most of the farmer groups were located in some distance from each other, so that the likelihood of knowledge spillovers was small. This is supported by the fact that KK15 adoption rates in the control group remained below 1 per cent also in the follow-up survey (Table A1 in the Appendix in supplementary data at *ERAE* online).

meaning that not all farmers that were offered certain training sessions also attended these sessions (Angrist, 2006). Non-compliance is better accounted for by the treatment-on-the-treatment (TOT) effect, which is also known as the local average treatment effect. TOT measures the actual effect of training attendance.

We do not observe perfect compliance in our RCT (Table A4 in the Appendix in supplementary data at *ERAE* online) shows attendance rates in the different training sessions, which means that estimating TOT effects is important (Bloom, 2006; Duflo, Glennerster and Kremer, 2007). However, the ITT effects are still relevant for policymakers, because most development programmes offer training or other types of services without the ability to enforce full compliance. The ITT effect shows how the development impact may look like without full compliance. Hence, we estimate both ITT and TOT effects.

4.1. Estimating intent-to-treat effects

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

$$y_{it} = \beta_0 + \beta_1 Post_t + \beta_2 T_j + \beta_3 Post_t \times T_j + \varepsilon_i \tag{2}$$

where y_{it} is KK15 adoption of household i in year t, $Post_t$ 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), and T_j is a dummy variable that takes a value of one if a farmer group is treated, and zero otherwise. We estimate this model separately for T_1, T_2 and T_3, each time only including the observations from the respective treatment group and the control. Hence, each of the treatment effects is only compared against the control.

The parameter of particular interest in equation (2) is β_3, which is the difference-in-difference estimator of the ITT effect. Under the assumption of parallel trends, 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 and non-zero adoption status at baseline (Greene, 2012; Pamuk *et al.*, 2015).

To control for differences in baseline covariates, we extend the model in equation (2) as follows:

$$y_{it} = \beta_0 + \beta_1 Post_t + \beta_2 T_j + \beta_3 Post_t \times T_j + \delta \boldsymbol{x_i} + \varepsilon_i \tag{3}$$

where $\boldsymbol{x_i}$ is a vector of baseline socioeconomic controls.

4.2. Estimating treatment-on-the-treated effects

For estimating the TOT effects, we use actual training attendance as treatment variables. In a first specification (TOT basic), we measure training attendance as a dummy variable that takes a value of one if a household attended at least one of the training sessions offered in its group, and zero otherwise. However, in T_2 and T_3 different training components were offered, which is accounted for in the second specification. In this second specification (TOT improved), the treatment variable for T_2 is defined as a dummy that takes a value of one if a household attended at least one agricultural and one nutrition training session, and zero otherwise. The treatment variable for T_3 is defined as a dummy that takes a value of one if a household attended at least one agricultural, one nutrition, and one marketing training session, and zero otherwise. In a third specification (TOT intensity), we look at the intensity of training attendance, measured by the number of training sessions attended relative to all training sessions offered in the group (this treatment variable can take values between zero and one).

The decision to attend training sessions is endogenous. To avoid endogeneity bias we use an instrumental variable (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 randomisation 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; Ashraf, Giné and Karlan, 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. This is also fulfilled in our case, as the first-stage regressions in Tables A5 and A6 in the Appendix (in supplementary data at *ERAE* online) with corresponding tests of weak instruments demonstrate. 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 behaviour of non-attendees in the training sessions is affected by the behaviour 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:

$$y_{it} = \beta_0 + \beta_1 Post_t + \beta_2 \widehat{T_j} + \beta_3 Post_t \times \widehat{T_j} + \varepsilon_i \tag{4}$$

where $\widehat{T_j}$ is the fitted value of the treatment (actual training attendance) obtained from the first-stage regression with the instrument. Again, to control for differences in baseline covariates, we extend the model in equation (4) as follows:

$$y_{it} = \beta_0 + \beta_1 Post_t + \beta_2 \widehat{T_j} + \beta_3 Post_t \times \widehat{T_j} + \delta \boldsymbol{x_i} + \varepsilon_i \tag{5}$$

For the estimation of the models in equations (4) and (5) 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. The 2SLS estimator works efficiently and produces estimates with a robust causal interpretation also with non-continuous treatment variables (Angrist, 2006).

5. Estimation results

5.1. Intent-to-treat effects

The estimation results for the cross-section OLS models explained in equation (1), with observations for all treatment arms and the control group included, are shown in Table A7 (in the Appendix in supplementary data at *ERAE* online). However, as discussed in the previous section, we prefer the difference-in-difference estimator to better control for potential confounding factors. In Table 2, 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) estimated with the difference-in-difference models explained in equations (2) and (3). Note that in these models each treatment is compared only with the control group; observations for the other treatments are excluded. We 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.

The results in Table 2 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 estimates in panel (A) imply that farmers that 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.[7] The share of land under KK15 is 4.9 percentage points higher.

For farmers that were offered agricultural training and nutrition training (treatment 2 shown in panel B of Table 2), the likelihood of planting KK15 seeds is 26 percentage points higher than for farmers in the control group. That is, the nutrition training may have an additional effect on adoption over and above the effect of agricultural training alone. However, the differences in the ITT effects between treatments 1 and 2 are not statistically significant. As explained, our RCT is underpowered to establish differences of only a few percentage points with statistical significance. The difference might turn significant with a larger sample, especially a larger number of farmer groups in each of the treatment arms. Farmers in treatment 3 (panel C) have a

7 This is near to the actual adoption rate of 24.7 per cent for treatment group 1 in the follow-up survey. The similarity between ITT effects and actual adoption rates is unsurprising given the randomised design, the almost zero adoption rate in all groups at baseline, and the almost zero adoption rate in the control group also in the follow-up survey (see Table A1 in the Appendix in supplementary data at *ERAE* online).

Table 2. Effects of extension treatments on technology adoption, ITT estimates

	Planted KK15 (dummy)		Share of land under KK15 (percentage)	
	(1)	(2)	(3)	(4)
Panel A: Treatment 1 ($n = 752$)				
Post × Treatment 1 (T1)	0.225**	0.225**	4.929**	4.929**
	(0.082)	(0.082)	(1.989)	(2.004)
	[0.009]	[0.011]	[0.014]	[0.017]
Baseline controls	No	Yes	No	Yes
R^2	0.163	0.175	0.096	0.113
Panel B: Treatment 2 ($n = 732$)				
Post × Treatment 2 (T2)	0.261***	0.261***	4.814***	4.814***
	(0.075)	(0.075)	(1.318)	(1.328)
	[0.002]	[0.002]	[0.004]	[0.001]
Baseline controls	No	Yes	No	Yes
R^2	0.207	0.227	0.118	0.129
Panel C: Treatment 3 ($n = 752$)				
Post × Treatment 3 (T3)	0.214***	0.214***	4.444***	4.444***
	(0.052)	(0.052)	(1.454)	(1.465)
	[0.001]	[0.000]	[0.005]	[0.004]
Baseline controls	No	Yes	No	Yes
R^2	0.165	0.192	0.104	0.125
Test H_0 : T1 = T2 (p-value)	0.742	0.743	0.961	0.962
Test H_0 : T1 = T3 (p-value)	0.911	0.912	0.843	0.844
Test H_0 : T2 = T3 (p-value)	0.604	0.605	0.850	0.850

Note: Coefficient estimates are shown with robust standard errors clustered at farmer group level in parentheses. p-values adjusted for the low number of clusters, calculated with the wild cluster bootstrapping technique (Cameron, Gelbach and Miller, 2008; Roodman *et al.*, 2018), are provided in square brackets. Inverse probability weighting was used to control for attrition. Not all variables are shown for brevity. Control group adoption rates and intensities are almost zero (Table A1 in the Appendix in supplementary data at *ERAE* online). Post, dummy variable that takes a value of 1 for follow-up round observations (after treatment) and zero for baseline observations. Treatment 1 (T1), agricultural training. Treatment 2 (T2), agricultural training plus nutrition training. Treatment 3 (T3), 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 per cent, 5 per cent and 1 per cent levels, respectively.

slightly lower likelihood of KK15 adoption (difference also not significant), which is somewhat surprising. Lower adoption in treatment group 3 might be explained by lower compliance rates (training attendance) in comparison to treatment groups 1 and 2 (see Table A4 in the Appendix in supplementary data at *ERAE* online).

5.2. Treatment-on-the-treated effects

Tables 3, 4 and A8 (in the Appendix in supplementary data at *ERAE* online) present the estimated TOT effects that account for actual training attendance.

Table 3 shows the results of models where the treatment variable is an attendance dummy that takes a value of one if at least one of the sessions offered in the respective group was attended, and zero otherwise. Farmers that attended agricultural training (treatment 1 shown in panel A of Table 3) are 22.5 percentage points more likely to adopt KK15 beans than their colleagues that 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).

Farmers that attended agricultural and nutrition training (treatment 2 shown in panel B of Table 3) are 32.2 percentage points more likely to adopt KK15 than those that did not attend any of the trainings. Their adoption intensity is 6.1 percentage points higher. Comparison of the TOT estimates between treatments 1 and 2 suggests that attendance of nutrition training increases KK15 adoption by almost 10 percentage points over and above attendance of agricultural training alone. But again, this difference is not statistically significant due to the relatively small number of farmer groups in each of the treatment arms. The TOT effects in panel (C) of Table 3 are very similar to those in panel (B), which could imply that attending marketing training does not have an additional effect on adoption over and above agricultural and nutrition training.

Another interesting comparison is the one between the ITT effects in Table 2 and the TOT effects in Table 3. For treatment 1, the ITT and TOT effects are almost identical, which implies that the adoption behaviour of individual farmers is affected also when they do not attend any of the training sessions themselves, as long as they belong to a group where other members attended. This is plausible and underlines the importance of farmer-to-farmer exchange of agricultural information within groups and social networks (Conley and Udry, 2010).[8] For treatment 2, the difference between the ITT and TOT effects is more visible, meaning that own training attendance plays a bigger role. This is also plausible, because nutrition information probably spreads less effectively through the social networks of farmer groups than agricultural information (Jäckering, Gödecke and Wollni, 2018).

Improved TOT estimates are shown in Table 4. As explained in Section 4, in these 'improved' TOT models, the treatment variable is as a dummy that takes a value of one only if the farm household attended at least one session in each of the training components (agriculture, nutrition, marketing) offered in their group, and zero otherwise. Farmers that attended agricultural training (treatment 1 shown in panel A of Table 4) are 24.8 percentage points more likely to adopt KK15 beans than their colleagues that did not attend any of the agricultural training sessions. Farmers that attended agricultural and nutrition training (treatment 2 shown in panel B of Table 4) are 50.0 percentage points more likely to adopt KK15 than those that did not attend any of the

8 Without such farmer-to-farmer exchange within the treatment groups, the difference between the TOT and the ITT would be larger, namely $\text{TOT} = \text{ITT}/\alpha$, where α is the share of farmers that actually attended any of the training sessions.

Table 3. Effects of extension treatments on technology adoption, TOT basic estimates

	Planted KK15 (dummy)		Share of land under KK15 (percentage)	
	(1)	(2)	(3)	(4)
Panel A: Treatment 1 ($n = 752$)				
Post × Treatment 1 (T1)	0.229***	0.225**	5.093**	4.861**
	(0.085)	(0.088)	(2.122)	(2.155)
	[0.015]	[0.024]	[0.019]	[0.022]
Baseline controls	No	Yes	No	Yes
R^2	0.190	0.200	0.113	0.127
Panel B: Treatment 2 ($n = 732$)				
Post × Treatment 2 (T2)	0.316***	0.322***	6.014***	6.149***
	(0.084)	(0.088)	(1.669)	(1.792)
	[0.003]	[0.006]	[0.003]	[0.006]
Baseline controls	No	Yes	No	Yes
R^2	0.279	0.299	0.163	0.175
Panel C: Treatment 3 ($n = 752$)				
Post × Treatment 3 (T3)	0.317***	0.317***	6.500***	6.533***
	(0.066)	(0.069)	(1.842)	(1.905)
	[0.000]	[0.003]	[0.000]	[0.002]
Baseline controls	No	Yes	No	Yes
R^2	0.268	0.293	0.167	0.187
Test H_0 : T1 = T2 (p-value)	0.210	0.160	0.629	0.495
Test H_0 : T1 = T3 (p-value)	0.168	0.146	0.455	0.369
Test H_0 : T2 = T3 (p-value)	0.988	0.944	0.790	0.835

Note: Coefficient estimates are shown with robust standard errors clustered at farmer group level in parentheses. *p*-values adjusted for the low number of clusters, calculated with the wild cluster bootstrapping technique (Cameron, Gelbach and Miller, 2008; Roodman *et al*., 2018), are provided in square brackets. The treatment variable is a dummy that takes a value of one if the household attended at least one of the training sessions offered in the respective group. Inverse probability weighting was used to control for attrition. Not all variables are shown for brevity. Control group adoption rates and intensities are almost zero (Table A1 in the Appendix in supplementary data at *ERAE* online). Post, dummy variable that takes a value of 1 for follow-up round observations (after treatment) and zero for baseline observations. Treatment 1 (T1), agricultural training. Treatment 2 (T2), agricultural training plus nutrition training. Treatment 3 (T3), 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 per cent, 5 per cent and 1 per cent levels, respectively.

trainings. Comparing the TOT effects between treatments 1 and 2 suggests that nutrition training attendance on top of agricultural training attendance doubles the adoption rate, and this difference is statistically significant at the 0.01 level. The TOT effect of 55 percentage points for treatment 3 (panel C in Table 4) is higher than the one for treatments 1 and 2, but only the difference between T1 and T3 is statistically significant.

Results of TOT models with the continuous treatment variable that measures the share of training sessions attended are shown in Table A8 (in the Appendix in supplementary data at *ERAE* online). These TOT estimates are

Table 4. Effects of extension treatments on technology adoption, TOT improved estimates

	Planted KK15 (dummy)		Share of land under KK15 (percentage)	
	(1)	(2)	(3)	(4)
Panel A: Treatment 1 (n = 752)				
Post × Treatment 1	0.248***	0.248**	5.488**	5.488**
	(0.089)	(0.090)	(2.208)	(2.224)
	[0.015]	[0.023]	[0.016]	[0.022]
Baseline controls	No	Yes	No	Yes
R^2	0.190	0.201	0.113	0.128
Panel B: Treatment 2 (n = 516)				
Post × Treatment 2	0.500***	0.500***	7.422***	7.422***
	(0.098)	(0.099)	(1.351)	(1.366)
	[0.000]	[0.000]	[0.000]	[0.000]
Baseline controls	No	Yes	No	Yes
R^2	0.455	0.467	0.284	0.293
Panel C: Treatment 3 (n = 472)				
Post × Treatment 3	0.550***	0.550***	9.803***	9.803***
	(0.115)	(0.116)	(3.199)	(3.237)
	[0.001]	[0.000]	[0.005]	[0.002]
Baseline controls	No	Yes	No	Yes
R^2	0.509	0.524	0.295	0.315
Test H_0 : T1 = T2 (p-value)	0.001	0.001	0.311	0.311
Test H_0 : T1 = T3 (p-value)	0.000	0.000	0.081	0.081
Test H_0 : T2 = T3 (p-value)	0.619	0.610	0.346	0.346

Note: Coefficient estimates are shown with robust standard errors clustered at farmer group level in parentheses. *p*-values adjusted for the low number of clusters, calculated with the wild cluster bootstrapping technique (Cameron, Gelbach and Miller, 2008; Roodman *et al.*, 2018), are provided in square brackets. The treatment variable is a dummy that takes a value of one if the household attended at least one of the training sessions from each component offered in the respective group. Inverse probability weighting was used to control for attrition. Not all variables are shown for brevity. Control group adoption rates and intensities are almost zero (Table A1 in the Appendix in supplementary data at *ERAE* online). Post, dummy variable that takes a value of 1 for follow-up round observations (after treatment) and zero for baseline observations. Treatment 1 (T1), agricultural training. Treatment 2 (T2), agricultural training plus nutrition training. Treatment 3 (T3), 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 per cent, 5 per cent and 1 per cent levels, respectively.

mostly larger than the ones in Tables 3 and 4, implying that full compliance (attendance of all training sessions offered) has larger effects on KK15 adoption than partial compliance.

5.3. Heterogeneous treatment effects

We examine heterogeneous treatment effects by gender and education of the farmer (household head). Gender and education were shown to be important variables in analyses of technology adoption and the effectiveness of agricultural extension in the African small farm sector (Anderson and Feder, 2004; Kabunga, Dubois and Qaim, 2012; Lambrecht, Vanlauwe and Maertens, 2016). For education, we create a dummy variable that takes a value of one if the farmer had at least 8 years of school education, and zero otherwise. Our sample size does not allow us to carry out the analysis with more than two education categories. Table A9 (in the Appendix in supplementary data at *ERAE* online) shows ITT models with additional interaction terms between the treatment and education dummies. For treatments 1 and 2, all interaction terms are statistically insignificant, suggesting that the treatment effects do not differ by education status.

Many previous studies showed that farmers with higher levels of education are better able to absorb new information and are more likely to be early adopters of new technologies (Foster and Rosenzweig, 2010; Fisher and Kandiwa, 2014; Wainaina, Tongruksawattana and Qaim, 2016). Hence, our result of homogenous treatment effects by education may surprise. However, it should be noted that KK15 is a technology that is not very difficult to understand and implement, as most farmers were already familiar with growing beans. Also, the training sessions offered in our RCT were tailored to farmers with relatively low levels of education: the extension officers used local dialects to explain concepts, employed visual aids such as posters and flipcharts, provided practical demonstrations and moderated interactive question and answer sessions. These methods facilitated understanding also for farmers with low levels of education. The only treatment where positive interactions between the treatment and the education dummy are observed in Table A9 (in the Appendix in supplementary data at *ERAE* online) is treatment 3, suggesting that the marketing training sessions may have been a bit more difficult to understand for farmers with low levels of education.

Table A10 (Appendix in supplementary data at *ERAE* online) analyses the heterogeneous treatment effects by gender. None of the interactions between the treatment and gender dummies is statistically significant, which suggests that the treatment effects do not differ between male and female farmers. Previous studies showed that women farmers are often slower or less likely to adopt new agricultural technologies due to various constraints (Fisher and Kandiwa, 2014; Peterman, Behrman and Quisumbing, 2014). However, a major constraint for women in technology adoption is access to proper information and extension (Kabunga, Dubois and Qaim, 2012). This was not an issue in our RCT. Around one-quarter of the farmers in our sample were female, and these female farmers were as likely as their male colleagues to attend the training sessions offered in the treatment groups.[9]

9 We also estimated heterogeneous treatment effects by education and gender using the TOT models. The interaction terms were not statistically significant in any of these models.

6. Discussion and conclusion

In this article, we have analysed 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. 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 per cent before the RCT started to more than 20 per cent 1 year later. This clearly shows that farmers are willing to adopt pro-nutrition technologies, if they are well informed about the technologies' attributes and implications.

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. Participation in nutrition training on top of the agricultural training doubled adoption rates from 25 per cent to 50 per cent. That nutrition training combined with agricultural training can further increase the adoption of pro-nutrition technologies is plausible but had not been shown explicitly in previous work.

It should be stressed that the positive nutritional 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. Knowledge about such broader nutrition aspects can be very important for farmers to better appreciate the nutrition attributes of KK15, thus resulting in higher adoption rates. The much higher iron and zinc contents in these beans can improve nutrition in farm households. Iron and zinc are particularly important for physical and cognitive development in children (IFPRI, 2017). Of course, the nutrition training may also have behavioural change effects that could positively influence household diets and nutrition well beyond KK15 adoption. We did not analyse such broader behavioural change effects of the nutrition training here. This could be an interesting topic for follow-up research.

The additional marketing training provided in one of the treatment arms of our RCT did not lead 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 (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 1 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.

6.1. Policy implications

Beyond this concrete example of KK15 adoption, combining agricultural training and nutrition training is worth further exploration. Nutrition education is usually not delivered through the agricultural extension service, but through specialised nutrition and health workers. Based on our results, we cautiously argue that combining agricultural and nutrition training in agricultural extension approaches may be a feasible approach. Of course, nutrition trainings should be designed together with nutrition experts, and the agricultural extension officers need proper training themselves before they can effectively deliver nutrition training to farm families. In our project, agricultural extension officers received nutrition training during an intensive two-day workshop and were accompanied by a nutritionist during the first few sessions that they gave to farmer groups. However, this is still much cheaper than running a completely separate nutrition outreach programme. Experience shows that high personnel and logistics costs of reaching out to farm households in rural areas are a major impediment for more widespread coverage of nutrition and health education campaigns (Ruel, Quisumbing and Balagamwala, 2018). We argue that closer cooperation between agricultural extension and nutrition and health organisations may be a cost-effective way to promote pro-nutrition innovations in the small farm sector.

6.2. Limitations and research implications

One challenge for every RCT carried out under specific conditions is that of external validity. While the study setting in Western Kenya is quite typical for the African small farm sector, the numerical estimates of the treatment effects should not simply be generalised. Our extension treatments were fairly intense and also involved a seed price subsidy. Outside an experiment, the training frequency and intensity may be lower, meaning that the short-term effects on technology adoption may be lower too. However, we would not expect that lower short-term adoption rates would alter our finding that agricultural training combined with nutrition training can increase the uptake of pro-nutrition technologies.

A second limitation of our study is the relatively small sample size and the limited number of farmer groups. Our study only included 12 groups per treatment arm (48 groups for three treatment arms and the control), which provides insufficient statistical power to establish small treatment effects at reasonable levels of significance. Fortunately, the treatment effects themselves were all quite large and therefore also statistically significant. However, low statistical power was a constraint for establishing significant differences between the treatment arms. While some of the TOT differences were statistically significant, others were not, so the results should be interpreted cautiously. Nevertheless, we feel that the study adds to the research direction, both conceptually and empirically, as we are not aware of any previous RCT that has compared different extension approaches.

Follow-up research with a larger sample and a larger number of farmer groups would be very useful, but we stress that an RCT that compares different extension approaches is also very costly. The reason for the high cost is that the research budget needs to cover not only the data collection and evaluation but also the cost of actual implementation of the extension treatments. The typical RCT procedure of ‘simply’ randomising and evaluating development treatments that are carried out anyway by other organisations is hardly possible when the focus is on comparing different extension approaches, because extension organisations typically use one approach in one setting instead of running various approaches in parallel. Our research project covered the cost of the extension treatments, which gave us the flexibility to design, implement and compare various approaches that were considered suitable. But running a number of consecutive training sessions in a large number of groups that are scattered in remote rural areas is an expensive exercise, which is probably also the reason why we were the first to carry out such research.

A final point that deserves discussion is the question of farmer attendance rates in the training sessions. In our RCT, more than two-thirds of the farmers in the treatment groups attended at least one of the training sessions offered. But less than 20 per cent of the farmers attended all of the sessions offered, and this in spite of our efforts to schedule sessions well in advance and to repeatedly remind group leaders and individual farmers. This effort was certainly greater than in extension situations outside an RCT, and we are confident that this led to training attendance rates that were higher than they would otherwise have been. Unfortunately, comparable data from other projects do not exist. Outside an RCT, extension organisations often prepare participants lists for trainings offered in order to document the total number of farmers reached. But such lists are rarely used to evaluate how many sessions individual farmers actually attended. More generally, research on farmer groups shows that the intensity of participation in group sessions and other collective activities is often low due to high opportunity costs of time and several other constraints (Weinberger and Jutting, 2001; Markelova *et al.*, 2009; Fischer and Qaim, 2014). The positive news is that our results clearly showed that training at the group level has technology adoption effects also for farmers that did not attend training sessions themselves, due to farmer-to-farmer exchange of information and mutual learning effects within the group.

We hope that our study will encourage follow-up research on agricultural extension with improved research designs. If the development community is serious about promoting sustainable and nutrition-sensitive innovation in the small farm sector, more research on how to improve existing extension approaches and make them more cost-effective will definitely be needed.

Supplementary data

Supplementary data are available at *European Review of Agricultural Economics* online.

Acknowledgements

The authors thank Jonathan Nzuma from the University of Nairobi for his research cooperation. Very valuable comments by three anonymous reviewers and the editors of this journal are also gratefully acknowledged.

Funding

This research was financially supported by the German Federal Ministry of Food and Agriculture (BMEL) based on a decision of the Parliament of the Federal Republic of Germany (grant number 2813FSNu01).

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