

DISCUSSION PAPER SERIES

IZA DP No. 13400

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Randomized Experiment in Indonesia**

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ABSTRACT

Can Training Enhance Adoption, Knowledge and Perception of Organic Farming Practices? Evidence from a Randomized Experiment in Indonesia*

In many parts of the world, several decades of intensively applying Green Revolution technologies came at environmental costs, i.e. degraded water and soil quality as well as a loss of biodiversity. This has led to an increased interest in alternative farming systems such as organic farming, which is commonly perceived as more sustainable. Despite many initiatives to promote organic farming, it remains a marginal activity in many countries. Widespread uptake of organic farming requires a better understanding of the drivers for and barriers to its adoption. Previous studies highlighted information as an important driver of agricultural technology adoption. Yet, despite the variety of programs studied, little is known about the role of removing information constraints in the context of organic farming. In this paper, we focus on the role of information provision and training as one driver for the adoption of organic farming practices in Indonesia. We use a randomized controlled trial (RCT) to identify the impact of a three-day hands-on training in organic farming on smallholder farmers' adoption and knowledge of such practices as well as on their perception towards organic farming. We find that the training intervention had a positive and statistically significant effect on the use of organic inputs. We further find positive and statistically significant treatment effects with respect to knowledge about and perception of organic farming. Overall, our findings suggest that intense training is a promising instrument to increase the uptake of organic farming.

JEL Classification: C93, O12, O33, Q12, Q16

Keywords: organic farming, technology adoption, RCT, Indonesia

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1. Introduction

Over the past decades, farmers in Asia extensively adopted 'green revolution' type technologies and experienced rapid increases in productivity. However, several decades of intensively applying these technologies came at environmental costs, i.e. degraded water and soil quality as well as biodiversity loss (Hazell, 2009; IAASTD, 2009; IFAD, 2013). Furthermore, despite increases in productivity, farmers in Asia are still overrepresented in the lower-income segments and now additionally face the consequences of environmental degradation (IFAD, 2013). These concerns led to increased interest in more sustainable farming systems such as organic farming. By abstaining from using chemical inputs and by promoting practices such as crop rotation and vegetative buffer zones, organic farming offers the potential to regenerate agricultural land and to counteract biodiversity loss (Jouzi et al., 2017; Seufert, 2012). Furthermore, several studies suggest that organic farming can also have positive effects on farmers' livelihood, mostly due to lower input costs and price premiums for organic products (Jouzi et al., 2017). However, despite policy efforts to promote organic farming, it remains a marginal activity in most Asian countries as well as in other parts of the world. Widespread uptake of organic farming requires a better understanding of the drivers for and barriers to its adoption. Previous studies highlighted the role of information as driver for agricultural technology adoption (Foster & Rosenzweig, 1995; Jack, 2011; Magruder, 2018). Providing farmers with information can increase problem awareness and knowledge of techniques; prerequisites for subsequent adoption (Jones, 2002; Lee, 2005).

In this paper, we use a randomized experiment to identify the effect of an organic farming on farmers' uptake of organic inputs as well as their knowledge about and perception of organic farming. The experiment was implemented in two regions in Indonesia and encompassed 60 villages. Treatment and control groups were sampled from farmer groups that were invited to information sessions on organic farming. Subsequent to the baseline survey, half of the villages were randomly assigned to the treatment group while the other half formed the control group. The treatment group was further randomly split into three different treatment arms: (i) training only, (ii) the same training augmented by an awareness video on the negative impacts of chemical inputs on environmental and human health, and (iii) the same training augmented by a role-play to strengthen mutual support within farmer groups. After the baseline data collection, respondents in all treatment villages received an invitation to participate in the training. The Indonesian non-governmental organization (NGO) Aliansi Organisme Indonesia (AOI) designed and delivered the training. The training was designed to be largely participatory with practical exercises to produce fertiliser but also included some lecture parts on organic principles. AOI revisited all treatment villages around eight months after the training for a meeting to address questions that occurred since the training. Follow-up data was collected one year after the baseline survey and around eleven months after the training.

For decades, agricultural programs have been widely implemented to address information and skills constraints. Existing studies have covered numerous extension program types, agricultural technologies and geographical and cultural contexts. These studies found mixed results for a variety of outcomes such as farmers' adoption of new technologies, knowledge or improved chemical input use (e.g. Fafchamps, Malek, Islam, & Pakrashi, 2018; Feder, Murgai, & Quizon, 2004; Kondylis, Mueller, & Zhu, 2017; Waddington et al., 2014). Many empirical studies found evidence for positive effects of extension programs on the adoption of new technologies, yet, the magnitude of the effects varies widely and oftentimes farmers only adopt a subset of the promoted technologies (e.g. Fafchamps e.g. 2018; Folke & Bie, 2014; Hörner, Bouguen, Frölich, & Wollni, 2019; Y. Pan, Smith, & Sulaiman, 2018; Takahashi, Mano, & Otsuka, 2019). Studying the impact of an agricultural extension program targeted at female farmers in Uganda, Y. Pan et al. (2018), for instance, observed that the program increased the adoption of cultivation methods that require low upfront monetary investment but not the adoption of more capital intensive cultivation methods.

Apart from promoting the adoption of new technologies or improved input use, extension programs provide farmers with new information and skills, which may in turn increase farmers' knowledge. However, evidence regarding knowledge improvements is mixed. Hörner et al. (2019), for example, found that training positively affected knowledge about integrated soil fertility management among farmers in Ethiopia. Guo, Jia, Huang, Kumar and Burger (2015) observed that while farmer field schools improved knowledge among Chinese farmers, these knowledge improvements were heterogeneous with respect to knowledge area, age and gender. Yet, in other settings extension programs had no impact on knowledge among contact farmers in Mozambique who received direct training (Kondylis et al., 2017) or farmers in India who received access to mobile phone based advisory services (Cole & Fernando, 2014) despite increasing adoption of the promoted techniques in both settings.

Despite the variety of extension programs studied, little is known about the impact of information provision in the context of organic farming. While previous studies have emphasized the importance of information for the adoption of organic farming, existing studies largely rely on observational data in developed countries (Burton, Rigby, & Young, 2003; Genius & Pantzios, 2006). Our contribution is to explore the effectiveness of providing information and hands-on training with an experimental design which allows to circumvent problems of self-selection and potentially confounding factors.¹ Moreover, we provide a comprehensive approach for studying the effect of an information treatment because in addition to studying treatment effects for input uptake and knowledge, we also investigate how farmers' perception of organic farming changed in response to the intervention. So far only a few studies have studied the impact of information provision on farmers' perception.² Finally, Indonesia is an interesting case to study. Its exceptional agricultural growth in the second half of the twentieth century was largely based on the extensive use of chemical inputs strongly promoted by the government at the time. Yet, concerns about the adverse environmental impact of intensive chemical use are growing and over-applications of chemical inputs has, for instance, long been identified as one cause of agricultural land degradation in Indonesia (Simatupang & Timmer, 2008). Today, policy makers have expressed a strong interest to encourage organic farming, also in view of developing new export markets (David & Ardiansyah, 2016).

We find a statistically significant increase in organic input adoption, in particular with respect to self-produced non-manure organic fertiliser. Further, treated farmers were more likely to answer knowledge questions about organic farming correctly and to perceive organic farming as more profitable and modern. Overall, our findings suggest that information constraints are a barrier to the adoption of organic farming as information provision increased the uptake of organic farming practices. However, as expected we cannot yet observe complete conversion to organic farming, rather it seems that some farmers in the treatment group have started to experiment with methods taught in the training but simultaneously continue to use chemical inputs. Furthermore, we find no evidence for heterogeneous effects across the three treatment arms.

The rest of the paper is structured as follows. Section 2 explains the study context, sampling framework and experiment design. Section 3 details our estimation strategy and describes the sample. Section 4 presents our findings. Section 5 discusses potential mechanisms linking the exposure to training and organic farming uptake. Section 6 concludes.

¹ While earlier studies have been criticized to insufficiently address this selection bias (Feder et al., 2004; Folke & Bie, 2014), recent studies have increasingly exploited RCTs to address this concern (Fafchamps et al., 2018; Guo et al., 2015; Hörner et al., 2019; Islam, 2014; Kondylis et al., 2017; D. Pan et al., 2017; Takahashi et al., 2019).

² Romero, Wollni, Rudolf, Asnawi, and Irawan (2019), for example, investigate the impact of information about biodiversity enrichment in oil palm plantations on farmers' perception in Indonesia.

2. Context, Experimental Design and Data

Although agriculture's relative contribution to GDP is decreasing, the sector continues to provide an important source of employment and income for rural households in Indonesia. Dominated by small-scale farms, it constitutes the main source of employment for about half the country's rural labour force (BPS, 2018b). On Java Island, the geographical context of this study, cultivated land sizes are smallest with less than 0.5 hectares per household on average (BPS, 2014). Java is the most densely populated Indonesian island and the population pressure further adds to declining cultivated land sizes per household. High shares of self-consumption and income diversification towards non-farm income are common among agricultural households (BPS, 2014; OECD, 2012). Rice remains the most important staple food in Indonesia and Java is the centre of the country's rice production. Here, more than 50 percent of agricultural households plant rice at least once a year, the majority of them smallholders (BPS, 2018a). Indonesia's political actors have long stressed rice self-sufficiency as essential for achieving food security, a national goal that has remained on the top of government agendas. In the 1970s, rice yields increased significantly in response to the Green Revolution and the associated uptake of chemical inputs and high yielding varieties. Thereby, agricultural intensification made an important contribution to poverty reduction and food security in Indonesia (Schreer & Padmanabhan, 2020; Simatupang & Timmer, 2008).

Yet, while food security remains a central policy issue, concerns about the adverse environmental impact of intensive chemical input use are growing. Over-application of chemical inputs has, for instance, long been identified as one cause of agricultural land degradation in Indonesia (Simatupang & Timmer, 2008). According to the National Development Planning Agency BAPPENAS (2014), particularly the over-application of the nitrogen fertiliser 'urea' is causing widespread quality loss of agricultural land. Correspondingly, Osorio, Abriningrum, Armas and Firdaus (2011) found that smallholder rice farmers, who constitute the majority of rice farmers, apply on average twice as much urea per hectare than large scale farmers and more than officially recommended by the Indonesian Ministry of Agriculture. Next to land degradation, consequences of intensive and inappropriate chemical input use include biodiversity loss and water pollution.³

Organic farming, initially an activist and NGO movement, gained increasing popularity among Indonesian policy makers in the early 2000s as one alternative to chemical-intensive conventional farming. In 2001, the government issued the so-called 'GoOrganic 2010' campaign, which envisioned Indonesia to become the leading producer of organic food by 2010. Although this goal was not reached, the campaign marked the beginning of political interest in and institutionalisation of organic farming in Indonesia. In 2003, the government introduced organic standards and since 2008, government subsidies for fertiliser include a budget share for organic fertiliser. In 2010, the government launched a second large policy project named '1000 organic villages' which aims to introduce organic farming in 1000 villages across Indonesia. Furthermore, regional governments have started to launch their own programs to support organic agriculture locally. An example is an initiative by the agricultural ministry of the district Kulon Progo in Yogyakarta province, one of our study locations. The ministry is currently promoting a project together with a national certification body to establish 200 ha of certified organic rice by 2020 to promote organic rice among farmers (Katajogja, 2019). Next to the government, NGOs remain active promoters of organic farming and private businesses are becoming increasingly involved.

³ Early on, in the 1970s and 80s, Indonesia experienced the negative consequences that agrochemical inputs induced biodiversity loss can have for agriculture. Following the excessive use of chemical pesticides, which destroyed both planthoppers and their natural predators, planthoppers were able re-emerge in much larger quantities due to the absence of their natural predators. In response, a farmer-training program on Integrated Pest Management (IPM) was widely implemented across Indonesia to minimize pesticide use among rice farmers. The training was delivered in the form of so-called farmer field schools, which utilise a participatory rather than top-down approach. Although the success of the IPM training is debated (Feder & Savastano, 2006), the participatory farmer field school approach remains a popular extension service tool.

Yet, despite the political support, available statistics indicate that the share of land that is currently farmed organically is below one percent (Lernoud & Willer, 2018).

Our field partner AOI, is one of the largest NGOs in the organic farming context in Indonesia. Founded in 2002, AOI is today active in 18 Indonesian provinces to support local farmers and spread organic farming nationwide. AOI has been instrumental in the design of our intervention and ensured that it is tailored to the local context. A workshop held during the inception phase of the project in 2017 in Yogyakarta brought together a large variety of stakeholders including farmers, activists, government officials and academics and further allowed to integrate different perspectives and expectations. Finally, the research design was also intended to respond to broader knowledge gaps as formulated by the German Ministry of Education and Research (*Bioeconomy International Initiative*).

Sampling and Intervention

Within Java, the study is set in two regions, Tasikmalaya District, West Java Province and the three districts Sleman, Bantul and Kulon Progo, all Yogyakarta Province. These regions were selected based on the capacity of our field partner AOI as it has large members in both regions who were capable of providing training to 300 farmers each.

For both regions we first compiled a list of all farmer groups that were not registered as organic, focused on crops, were not located on the building ground of the new Yogyakarta airport and reported more than 20 but less than 200 members and at least 60 members across all existing farmer groups within a village (Tasikmalaya) or subvillage (Yogyakarta).⁴ We obtained a sample of 1,392 farmer groups in 205 villages for Yogyakarta and of 1,412 farmer groups in 291 villages for Tasikmalaya. In total, we had to drop around 64 percent of the farmer groups in Yogyakarta and around 13 percent of the groups in Tasikmalaya. The more detailed initial level of information in Yogyakarta, i.e. sub-village level and agricultural focus, led to a significantly higher share of dropped farmer groups in Yogyakarta. We then applied a three-stage random sampling design to select 1,200 respondents. In the first stage, we randomly picked 30 villages each from the 205 villages in Yogyakarta and the 291 villages in Tasikmalaya.⁵ In the second stage, we randomly drew a minimum of one and a maximum of three farmer groups in each village until the total number of farmers in the selected farmer groups was equal to or larger than 60.⁶ After identifying the farmer groups, the members of the selected farmer groups were invited to an information session on organic farming in their respective village. These information sessions served two purposes (1) allow for self-selection based on an initial interest in organic farming and the willingness to participate in farmer group events (2) collect contact details on prospective respondents. The information sessions were implemented by AOI. Enumerators and a field team from Germany accompanied the sessions to assist with the collection of attendants' contact data. In the third sampling stage, respondents were randomly sampled among the attendants of the information session. If there were too few attendants at the information session, we asked the farmer group head to provide us with contact details of further farmer group members who did not attend the information session. To avoid bias during the interview and biased nominations of additional respondents by the farmer group head, we only informed farmers about the upcoming training after the survey was

⁴ Small and very large farmer groups were dropped as it would have been difficult to implement the intervention for very small or very large farmer groups. In advance to the survey, we visited each village (control and treatment) for an information session. Considering that not every farmer in the selected farmer group is interested in such an event necessitated a minimum number of farmers invited. On the other side, logistical reasons limited the possible size of these information sessions.

⁵ Given that villages are, on average, larger in Yogyakarta than in Tasikmalaya and encompass more farmers we then further randomized at the village level in Yogyakarta, i.e. one sub-village was randomly selected in each village. Selecting from villages in Yogyakarta might have led to the selection of farmer groups that are located far apart. Thereby this could have led to longer travel distances for training, which we tried to avoid to increase training attendance.

⁶ If the first drawn farmer group had 60 members or more, we only selected this one farmer group. Otherwise, we drew again. If first and second together counted at least 60 members, we stopped drawing. Otherwise, we drew a third farmer group.

completed in their village. Table A1 in the Appendix provides descriptive statistics for non-attenders by treatment status. Overall, there seems to be no systematic difference between non-attenders in the treatment and in the control group in terms of socio-demographic characteristics such as age, gender and education. There are some differences regarding beliefs about agricultural pollution, present bias and cultivated land size. However, in particular for the first two variables, this information is difficult to assess for farmer group heads. Given that the unbalances occur for these variables and not for variables that are easier to assess, i.e. age, gender, education, we conclude that biased nominations by farmer group heads based on treatment status are unlikely to have occurred.

The treatment was randomized at the village level and consisted of a training on organic farming methods and principles. Control villages did not receive any training. As baseline data was not available at the time of the randomisation, we used publicly available regional data for the stratification. Specifically, we stratified the sample according to urban and rural status and the size of reported agricultural land area per village. In Tasikmalaya, we additionally used 'travel distance to the district capital' as stratification criterion as this region is characterized by less developed infrastructure and long travel times within the district. The treatment villages were further randomly assigned in equal shares to one of three treatment arms (i) training only, (ii) the same training augmented by a video on the negative impacts of chemical inputs to raise awareness for the potential health and environmental benefits of organic farming, and (iii) the same training augmented by a group exercise in form of a role play to strengthen mutual support within farmer groups. Figure 1 provides a map of the treatment and control villages.

After the baseline data collection, we invited the 20 interviewed farmers in each treatment village to participate in a three-day training on organic farming which lasted seven hours per day. The training provided farmers with an introduction to organic farming including information on organic farming principles and potential marketing channels. Particular emphasis was given to practical exercises such as testing the level of soil health and making different organic fertilisers and pesticides. In the video session, the farmers were exposed to a video that showed the negative effects of using chemical inputs on the environment and human health, particularly the health of farmers who work with chemical inputs every day. The trainers then discussed the video content with the farmers. During the role-play session, the farmers were invited to participate in a role-play addressing the accountability of organic farmers towards their consumers. The role-play focused on the importance of recording the process of organic cultivation and working as a group. The training was designed by AOI in cooperation with the research team from Germany. AOI, together with its members, delivered the training, which was held in the respective villages to minimize travel time for respondents. The farmers received IDR 50,000 (around USD 3.5) for each day of the training (only if they attended) to cover any transport costs and to compensate them for potentially forgone earnings on that day. After the training, the farmers received a printed manual on organic farming. The training was rolled out at the end of March 2018 and completed in May 2018. On average, 17 farmers of the 20 invited farmers attended the training per day.

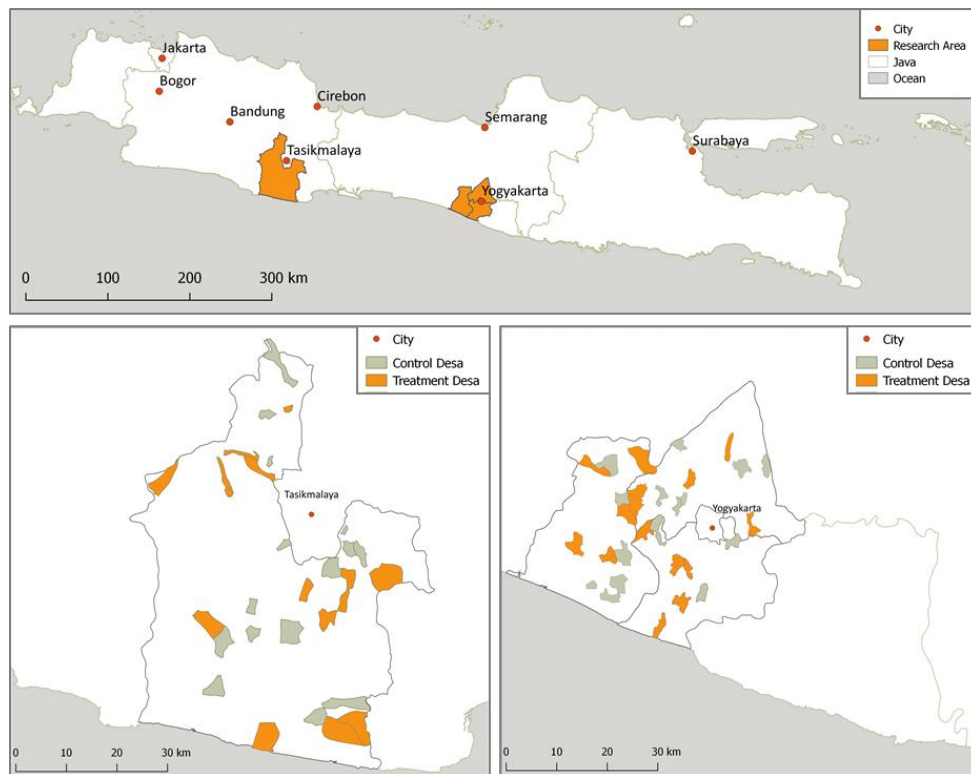


Figure 1: Research Locations

Data and descriptive statistics

The data used in this article comes from two waves of survey data. Baseline data was collected between February and April 2018 and directly followed by the training intervention. Regarding agricultural information, the baseline survey covered the most recently completed cropping cycle. Follow up data was collected between February and April 2019 and also covered the most recently completed cropping cycle.

From the 1,200 respondents interviewed at baseline, we successfully re-interviewed 1,149 respondents indicating an attrition rate of around 4%. The main causes of attrition were migration and health problems. The attrition rate was similar between the treatment and control group with 4.0% and 4.1% respectively. We further test for attrition bias by regressing a binary variable for attrition status on treatment status and the regressors that appear in the main estimation equation. Table A2 in the Appendix shows the regression results for the test for attrition bias. We find that treatment status is not statistically significantly different between attritted and returning respondents. For the other regressors we find no statistically significant relationship with the attrition rate except for the regional dummy for Tasikmalaya and the dummy for 'doing farming as main activity', statistically significant at the 1% level and 10% level respectively. Given the low attrition rate and the absence of systematic differences between attritted and returning respondents, we conclude that attrition is unlikely to be a concern for our analysis. The remainder of our analysis is based on the 1,149 respondents who were interviewed at baseline and follow-up.

Table 1 presents baseline summary statistics by treatment status, including demographic information and agricultural production variables. Most respondents are male with an average age of 54 years and eight years of completed education. While crop farming is the main economic activity for the majority of respondents, livelihood diversification is common. Around two-thirds of the sample engaged in a second economic activity next to crop farming. Ownership of assets is mixed. An asset index calculated by summing up the assets owned by respondents' households shows that households

owned, on average, 1.7 assets out of the following four assets: bike, motorcycle, car, fridge. However, there are around 12% of households which own none of the listed assets, most of them (94%) from Tasikmalaya. Respondents and their household members cultivate, on average, 0.3 hectares of land for crop production. This includes land for the production of staple crops such as rice, maize and cassava as well as vegetables. The definition excludes, however, land for plantations such as trees. The average share of land owned is at 60% and fairly equal across treatment and control group. Around half of respondents own all land. However, there is considerable regional heterogeneity and ownership of cultivated land is more restricted in Yogyakarta. Here, only 35% of respondents own all land while this share lies at 65% in Tasikmalaya. Rice is the dominant crop in our sample. Around 93% of respondents planted rice on at least one of their plots during the last cropping cycle. Furthermore, irrigation levels are high and most respondents (91%) cultivated at least one plot that was irrigated during the last cropping cycle.

Balance

We employ a joint orthogonality F-test to test for baseline balance between the control and the treatment group. Despite randomisation we obtain a p -value below 10% (p -value = 0.06). Imbalances in gender composition and cultivated land sizes appear to drive the relatively low p -value. In fact, excluding cultivated land size outliers, i.e. cultivated land sizes above 2 ha, results in a considerably higher p -value of the joint-orthogonality test (p -value = 0.49). Apart from the cultivated land size and gender composition, characteristics are balanced. Furthermore, other characteristics not used as controls in the main regression are well balanced and thereby provide no indication for structural differences between the control and the treatment group. This lets us conclude that there is no evidence for a strong structural difference in overall respondent characteristics between the treatment and the control group that would let us to abstain from comparing the two groups. To account for imbalance at baseline, our analyses control for baseline socioeconomic characteristics.

Table 1: Descriptive Statistics

	Control group mean	Treatment group mean	C-T
<i>Individual and household characteristics</i>			
Male	0.79	0.87	-0.08***
Age (in yrs.)	54.38	53.09	1.29
Muslim	0.96	0.97	-0.02
Schooling (in yrs.)	8.04	8.25	-0.21
Asset Index (bicycle, motorcycle, car, fridge)	1.69	1.77	-0.08
Farming is main activity	0.79	0.78	0.00
Farmers' decisions matter (perception)	0.59	0.56	0.02
Agr. env. Pollution is problematic (perception)	0.46	0.45	0.01
<i>Agricultural characteristics</i>			
Cultivated land (in ha)	0.30	0.41	-0.12***
Land ownership share	0.62	0.61	0.01
Rice (=1 if R planted rice)	0.94	0.91	0.03*
Irrigation (=1 if cultivated land is irrigated)	0.89	0.92	-0.03
p -value for joint orthogonality test		0.061	
p -value for joint orthogonality test (14 land outliers (>2ha) dropped)		0.493	

Outcomes

We collected data on three types of outcomes variables: *application* of organic farming inputs, *knowledge* about organic farming practices and *perception* about organic farming (see Table A3 in the Appendix).

Our first type of outcome variables, *application*, comprises three binary variables, namely (i) organic fertiliser other than manure, (ii) organic pesticide and (iii) processed manure. Variables are equal to one if respondents reported to have applied the respective input during the last cropping cycle. We differentiate between 'organic fertiliser other than manure' and 'processed manure' to better capture the impact of the training. While processed manure (in contrast to unprocessed manure) is considered an organic fertiliser by the implementing NGO, the training focused on the production of organic fertiliser other than manure. At baseline, around one-third of the respondents reported to have applied organic fertiliser or processed manure. Reported application rates for organic pesticide were much lower with around 8%. While there are differences between control and treatment group, these appear to be not systematic; control group farmers used less organic fertiliser but more organic pesticide. In our regression design, we will control for these imbalances at baseline.

To measure *knowledge*, we asked respondents five questions focusing on the training content. For each question, we constructed a binary variable equal to one if the respondent answered correctly. Specifically, we asked respondents about (i) the existence of organic labels, (ii) land burning, (iii) processing of manure, (iv) the conversion period from conventional to organic farming and (v) buffer zones between conventional and organic plots. At baseline, only a few respondents were aware of the existence of organic farming labels (22 %). Around two-thirds knew that organic farmers must not burn land and must process manure before use. Knowledge levels at baseline appear to be similar across treatment and control group. However, unfortunately, we do not have baseline values for the last two knowledge questions and treatment effects for these two questions are therefore interpreted with more caution.

To capture respondents' *perception* of organic farming, we asked respondents whether they perceive (i) organic farming as modern or old-fashioned, (ii) how they think the demand for organic products has developed over the past five years and (iii) how they think organic farming and conventional farming compare with respect to profitability. Again, we constructed binary variables for the three indicators equalling one if the respondent perceives organic farming as modern, to have experienced increasing demand and to be more profitable. Additionally, the analysis considers two measures of respondents' perception about the impacts of chemical inputs, namely whether they think it is true that chemical inputs have a negative effect on the environment and on farmers' health respectively. At baseline, around 40% of respondents perceived organic farming as modern, while only around 19% thought that demand for organic products had increased over the past five years. There are no notable differences between control and treatment group at baseline.

3. Empirical Estimation

To estimate the impact of the training intervention, we focus on intent-to-treat (ITT) estimates; that is, simple comparisons of averages in treatment and comparison areas, averaged over all those farmers that had been invited to the training whether they participated or not. On average, across the thirty treated villages, about 80% to 90% of all farmers invited to the training participated. Although the intervention was randomised and simple mean comparisons yield unbiased impact estimates, we include control variables observed at baseline to redress imbalances that showed up in the balance tests and to reduce the noise in the data to get more precise impact estimates. We estimate all specifications via a linear probability model (LPM).

Hence, the full regression model reads as follows:

$$Y_{iv} = \beta_0 + \beta_1 T_v + X'_{iv0} \gamma + (\epsilon Y_{iv0}) + \delta S' + u_{iv}, \quad (1)$$

where Y_{ij} stands for the various outcome measures we are interested in observed for individual i of village v , including the uptake of organic fertiliser and pesticide, chemical input use as well as knowledge about and perception of organic farming. The variable T_v takes the value one if individual i lives in a treatment village. Hence the key interest of our analysis is with the impact estimate β_1 . The vector X_{iv0} controls for baseline characteristics of the sampled individuals, S is a vector of randomisation strata. Y_{iv0} is the lagged outcome (at baseline). We include it for all outcomes for which it is contained in our data. u_{iv} stands for the error term, to account for within village correlations of our variables, we cluster standard errors at the village level.

For outcomes for which we have baseline data, we first estimated the treatment effects including the lagged outcome as a control (ANCOVA specification). We then subsequently estimated treatment effects without the lagged outcome as a control for all our outcomes (POST specification).

Controlling for the lagged outcome variables allows to account for minor imbalances in outcomes at baseline and to improve precision because lagged outcomes are often a strong predictor of outcomes at follow-up. We prefer the ANCOVA specification to a difference-in-difference specification because of the improvement in statistical power (Mckenzie, 2012). In the result section, we focus on the ANCOVA specification when possible and on the POST specification otherwise

Since we implemented the treatment in three versions: just training (T), training and exposure to an awareness video (V) and training and participation in a role play (R), we also estimate Equation (1) with interaction effects that account for the additional intervention to see whether these can further increase the impact of the simple treatment. The specification then becomes:

$$Y_{iv} = \beta_0 + \beta_1 T_v + \beta_2 (T \times V)_v + \beta_3 (T \times R)_v + X'_{iv0} \gamma + (\epsilon Y_{iv0}) + \delta S' + u_{iv}, \quad (2)$$

Given our sample size, we will obviously need relatively large treatment effects associated with the normal training and these additional interventions to be able to identify the effect associated with these additional interventions.

We will also explore whether impacts depend on socio-economic characteristics, i.e. age, years of schooling, perception of agricultural pollution as problematic, cultivated land size, location, risk aversion and present biasedness (all measured at baseline).

Finally, we also estimate the Local Average Treatment Effect (LATE) using the invitation to the training as an instrument for the actual participation, although we do not expect large differences with the ITT effects as uptake was relatively high. Moreover, even if farmers did not participate in the training, they may have learned about the practices later through the day-to-day interaction with farmers who attended the training. Hence, it would be a strong assumption to consider a farmer of a treated farmer group as untreated.

Multiple Hypothesis Testing

Given that we test for a large number of outcomes, there is a higher probability of falsely rejecting the null hypothesis in some cases. We control for the potentially false discovery of significant effects by correcting p-values for multiple hypothesis testing using the Benjamini-Hochberg method (Benjamini & Hochberg, 1995). Following Baird, McIntosh and Özler (2019), who used q-values as derived in Anderson (2008). We present the corresponding q-values in our main tables.

Randomisation Inference

We further report p-values from randomisation inference tests for our main results. The randomisation inference based p-values are derived by randomly assigning treatment status within the strata and estimating a distribution of the parameter of interest (based on 5,000 permutations). Randomisation inferences tests the robustness of our results, it relies entirely on randomisation without making any distributional assumptions about the test statistics (Heß, 2017).

4. Results

In this section, we first examine whether treatment assignment changed farming input-related behaviour, knowledge about organic farming and perceptions of organic farming. Comparing the coefficients from the ANCOVA and the POST models, we find that these specifications generate very similar results in terms of coefficient size, sign and significance level. This lends additional robustness to our results from the POST specifications. In the second step, we then explore whether treatment arms matter before looking at heterogeneity in treatment effects based on the observed characteristics. Lastly, we perform robustness checks for the main results.

Application

Increasing the uptake of organic practices was the main objective of the training. We thus begin by examining whether treatment assignment led to farming input-related behavioural change, i.e. an increase in the use of organic inputs and a decrease in the use of chemical inputs. Table 2 presents the results.

Cols. (1) and (4) show the intention to treat effect (ITT) on farmers' use of organic fertiliser and organic pesticide. We observe that farmers assigned to the treatment group are on average 13 percentage points more likely to use organic fertiliser and 8 percentage points more likely to use organic pesticide. In the ANCOVA model, both treatment effects are significant at the 1% level. Furthermore, treatment effects are robust to controlling for the number of false discoveries (FDR adjustment) and remain significant at the 1% and the 5% significance level, respectively.

In the follow-up survey, we further asked farmers whether they produced the respective organic input themselves or whether they bought it. Cols. (2) and (4) show the ITT effect on the probability that farmers *produced* at least part of it while Cols. (3) and (6) show the effect on the probability that farmers *purchased* at least part of it. We find that the estimated effects are considerably higher, and in the case of organic fertiliser, only statistically significant for self-produced organic inputs. This indicates that encouragement and instructions for own production seem to drive the positive effect on organic fertiliser and organic pesticide use. However, adjusting for the FDR, we can no longer reject the null hypothesis of no impact for the use of self-produced fertiliser. We further examine the impact on the use of processed manure. While the estimated coefficient is positive and suggests an increase in the probability to use processed manure by 5 percentage points, the effect is only significant at the 10% level in the ANCOVA model and not robust to FDR adjustment.

Given the positive impact of treatment assignment on the probability to use organic inputs, one might expect a decrease in the use of chemical inputs if organic inputs are perceived as a substitute. Considering the substantial effect on organic fertiliser use, this effect should be particularly strong for chemical fertiliser. However, we find no significant training impact on the decision of whether to use chemical fertiliser (Col. 8).⁷ Instead, chemical fertiliser use remains high in both the treatment and the

⁷ We also find no effect if we use 'money spent on fertiliser' as outcome variable (and similarly no effect for 'money spent on chemical pesticide').

control group. From the results using the ANCOVA model, we find that farmers assigned to the treatment group are 9 percentage points more likely to abstain from using chemical pesticide (Col. 9). However, results are not robust to the FDR adjustment. In summary, our results provide evidence for increased experimentation with and adoption of the methods taught during the training among farmers assigned to the treatment group.

Table 2: ITT effects on input application (OLS estimation)

	(1) Organic fertilizer	(2) Organic fertilizer <i>produced</i>	(3) Organic fertilizer <i>purchased</i>	(4) Organic pesticide	(5) Organic pesticide <i>produced</i>	(6) Organic pesticide <i>purchased</i>	(7) Processed manure	(8) No chemical fertilizer	(9) No chemical pesticide
ANCOVA	0.126*** (0.000)			0.091*** (0.010)			0.058* (0.094)	0.001 (0.933)	0.093* (0.057)
[q-value]	[0.001]			[0.030]			[0.123]	[0.933]	[0.103]
{p-value RI}	{0.000}			{0.005}			{0.140}	{0.943}	{0.082}
POST	0.166*** (0.000)	0.139*** (0.000)	0.051 (0.131)	0.075** (0.033)	0.062* (0.052)	0.027* (0.095)	0.051 (0.183)	0.005 (0.765)	0.102* (0.064)
[q-value]	[0.001]	[0.001]	[0.169]	[0.099]	[0.115]	[0.143]	[0.206]	[0.766]	[0.115]
{p-value RI}	{0.000}	{0.000}	{0.149}	{0.030}	{0.029}	{0.107}	{0.227}	{0.778}	{0.088}
Control mean (follow-up)	0.27	0.14	0.15	0.07	0.02	0.03	0.33	0.05	0.40
Strata	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N (ANCOVA)	1,146			1,142			1,146	1,146	1,146
N (POST)	1,149	1,107	1,107	1,149	1,145	1,145	1,149	1,150	1,150
R-squared (ANCOVA)	0.301			0.061			0.108	0.072	0.120
R-squared (POST)	0.078	0.022	0.077	0.069	0.056	0.120	0.046	0.034	0.056

Note: *** p<0.01, ** p<0.05, * p<0.1 . Naive p-values from the Huber-White robust standard errors, clustered at the village level in parentheses. Multiple hypothesis adjusted q-values in square brackets. For the ANCOVA model, q-values are based on the rank relative to the naive p-values from the POST model for the outcomes for which there is no lagged outcome. P-values from randomisation inference in curly brackets.

Knowledge

Measuring knowledge served two purposes. First, it allows assessing knowledge gains. Second, it allows measuring the training impact in a way that is not prone to a bias arising from strategic answers by respondents, i.e. if they just reported what they think interviewers want to hear without however understanding.

Table 3 shows the results. First, as shown in Col. (1), we observe a substantial increase in knowledge about the existence of organic labelling among farmers in the treatment group. Put differently, only 24% of farmers in the control group reported knowing about the existence of labelling compared to 46% in the treatment group. However, closer examination reveals that only few farmers, in both the treatment and the control group, could name specific labels (less than 1% in the control group and around 2% in the treatment group). Cols. (2-4) show the training effect on questions that test farmers' knowledge about prohibited and required practices in organic farming. Farmers assigned to the treatment group were around 14 percentage points more likely to know about the prohibition of land burning, around 7 percentage points more likely to know about the requirement to process manure before application and, finally, 20 percentage points more likely to know about the requirement for a buffer zone between organically farmed land and conventionally farmed land⁸.

⁸ Conditional on correctly answering the questions on land burning and manure processing, we further asked farmers open ended question requesting them to explain why land burning is seen as negative and how one can assess whether manure is ready. Enumerators were instructed on the right answer and to code respondents' answers as correct or wrong. We find that, conditional on answering the first

Furthermore, the training caused a 16 percentage point increase in the probability that respondents knew that the conversion from conventional to organic farming requires some time. This is important as realistic expectations about conversion times prevent disappointment after initial adoption and subsequent disadoption. Effects on all knowledge outcomes are robust to the FDR adjustment. Overall, farmers in the treatment group answered 0.76 more questions (out of five) correctly compared to farmers in the control group who answered, on average, only 2.7 questions correctly. Thus, the positive effects we find for knowledge further strengthen the robustness of our findings for the application of organic practices.

Table 3: ITT effects on knowledge (OLS estimation)

	(1) Labels	(2) No land burning	(3) Manure processing	(4) Conversion period	(5) Buffer zone	(6) Knowledge index
ANCOVA	0.200*** (0.000)	0.138*** (0.000)	0.067** (0.038)			
[q-value]	[0.001]	[0.001]	[0.038]			
{p-value RI}	{0.000}	{0.001}	{0.073}			
POST	0.195*** (0.000)	0.136*** (0.000)	0.065** (0.047)	0.161*** (0.000)	0.202*** (0.000)	0.759*** (0.000)
[q-value]	[0.001]	[0.001]	[0.048]	[0.001]	[0.001]	
{p-value RI}	{0.000}	{0.001}	{0.082}	{0.000}	{0.000}	
Control mean (follow up)	0.24	0.74	0.77	0.48	0.49	2.71
Strata	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
N (ANCOVA)	0					
N (POST)	1,150	1,150	1,150	1,150	1,150	1,150
R-squared (ANCOVA)						
R-squared (POST)	0.172	0.063	0.045	0.124	0.105	0.212

Note: *** p<0.01, ** p<0.05, * p<0.1 . Naive p-values from the Huber-White robust standard errors, clustered at the village level in parentheses. Multiple hypothesis adjusted q-values in square brackets. For the ANCOVA model, q-values are based on the rank relative to the naive p-values from the POST model for the outcomes for which there is no lagged outcome. P-values from randomisation inference in curly brackets.

Perception

Table 4 presents the impact of the training intervention on farmers' perceptions of organic farming. The results indicate that the training positively affected farmers' perceptions of organic farming and strengthened their awareness of the negative effects associated with conventional farming. The estimated coefficients are statistically significant and robust to FDR adjustment. Specifically, we find that farmers assigned to the treatment group are 8 percentage points more likely to perceive organic farming as a 'modern' rather than an 'old fashioned' technology. Further, farmers in the treatment group were 14 percentage points more likely to think (rightly) that within Indonesia, demand for organic products has increased in the past five years. The training also increased the probability that farmers perceive organic farming as more profitable compared to conventional farming. The probability that farmers think that chemical inputs negatively affect the environment and farmers' health increased by 6 and 7 percentage points, respectively.

question correctly, farmers assigned to the treatment group are not more or less likely to answer the follow up open ended question correctly. While this indicates that the training was not successful in teaching everyone a better understanding, it also provides suggestive against strategic answering (then the share of correct answers should be substantially lower in the treatment group).

Table 4: ITT effects on perceptions (OLS estimation)

	(1) Modern	(2) Demand increased	(3) More profitable	(4) Chemicals: neg. for env.	(5) Chemicals: neg. for health
ANCOVA	0.082** (0.015)	0.143*** (0.000)			
[q-value]	[0.020]	[0.001]			
{p-value RI}	{0.029}	{0.001}			
POST	0.079** (0.023)	0.143*** (0.000)	0.115*** (0.001)	0.064** (0.046)	0.067*** (0.005)
[q-value]	[0.030]	[0.001]	[0.000]	[0.047]	[0.009]
{p-value RI}	{0.042}	{0.001}	{0.003}	{0.079}	{0.011}
Control mean (follow up)	0.37	0.37	0.52	0.71	0.76
Strata	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
N (ANCOVA)	1,150	1,150	1,150	1,150	1,150
N (POST)	1,150	1,150	1,150	1,150	1,150
R-squared (ANCOVA)	0.117	0.113			
R-squared (POST)	0.111	0.113	0.094	0.105	0.087

Note: *** p<0.01, ** p<0.05, * p<0.1. Naive p-values from the Huber-White robust standard errors, clustered at the village level in parentheses. Multiple hypothesis adjusted q-values in square brackets. For the ANCOVA model, q-values are based on the rank relative to the naive p-values from the POST model for the outcomes for which there is no lagged outcome. P-values from randomisation inference in curly brackets.

Treatment Arms

So far, we have considered the treatment group as a single group. However, as described previously, the full design consists of three treatment arms: just training, training augmented by exposure to an awareness video (V) and training augmented by participation in a role-play (R). Given the similarity between the three treatment arms and the loss in statistical power from analysing the treatment group separately, we would not expect substantial differences between the impacts of the respective treatment arms. Yet, the awareness video and the role-play present very low-cost augmentations to the training and even small differences would thus be of interest to policy makers. Tables 7-9 report the results for the analysis of differential impacts across treatment arms. The interaction effects between *Training* and *Video* or *Role-play* indicate whether the respective augmentation was significantly different from the training only treatment. We further test for the equality of the two interaction coefficients.

Overall, our results indicate that the three training versions had generally similar impacts. For the adoption outcomes (Table A4), we cannot reject the null hypothesis of no differential impact of the two augmentations compared to the training only treatment. Furthermore, the differences between the estimated interaction coefficients are not statistically significant at the 10% significance level for all outcomes⁹. Among the knowledge outcomes (Table A5), there is only one outcome, i.e. knowledge about the requirement to process manure before application, for which one treatment arm is significantly different from both other treatment arms. The coefficient of the interaction term *Training*Role-play* is positive and statistically significant at the 5% significance level and the difference between the estimated interaction terms is statistically significant at the 10% level. For the perception

⁹ The absence of a statistically significant effect for *Training* in Col. (4) could be explained by the loss in power from splitting the treatment arms combined with the relatively low number of cases for organic pesticide use at follow up.

outcomes (Table A6), we find that farmers assigned to the video treatment were more likely to think that chemicals negatively affect the environment (Col. 4) and farmers' health (Col. 5) compared to farmers assigned to the training-only group. However, employing the Wald test for equality of coefficients, we cannot reject the null hypothesis of no difference between the video and the role-play treatment arm at the 10% significance level.

Summarising, we cannot find robust evidence that the training effect can be enhanced by no/low-cost augmentations such as a video treatment or a role-play. However, when interpreting these results, one should keep in mind the limited size of the subsamples and hence the limited power as well as that both augmentations only took around two additional hours within a three-day training. Hence, our results can say very little about what more exhaustive augmentations could achieve.

Heterogeneity in Treatment Impact

Policy makers and other stakeholders are often interested in the heterogeneity in treatment effects, i.e. whether some sub-populations are more responsive to the training than others are. In the following, we thus examine heterogeneity in treatment effects based on the observed characteristics we specified in the Pre-Analysis Plan¹⁰. Specifically, we examine heterogeneity in treatment effects for farmers' age, education, awareness for agricultural environmental pollution, risk attitude, land size, and the research location (Tasikmalaya district or Yogyakarta province) – all measured at baseline. Tables A7-A9 in the Appendix present the results.

Table A7 examines treatment heterogeneity in impact for the adoption outcomes. We find suggestive evidence for heterogeneity in impacts with respect to the research location. Yet, these results are not robust to the FDR adjustment, likely because of a loss in statistical power due to the large set of outcomes. Findings should, therefore, be interpreted with caution. Positive interactions between treatment assignment and the region dummy Tasikmalaya indicate that the training was more successful in raising adoption of organic fertiliser and decreasing chemical input use in Tasikmalaya compared to Yogyakarta.

Table A8 shows heterogeneity in impacts for the knowledge outcomes. After adjustment for the FDR, we find only one significant interaction, namely between treatment assignment and awareness for agricultural pollution; see Col. (2), which reports impacts on the knowledge about the prohibition to burn land. As land burning is associated with air pollution, this finding might be explained by the initially higher knowledge about the negative effects of land burning among those farmers who reported awareness for agricultural pollution at baseline.

Finally, Table A9 reports treatment heterogeneity for the perception outcomes with respect to farmers' age, education, awareness for agricultural environmental pollution and cultivated land size. We find FDR-robust heterogeneity in impact for two outcomes: the perception that chemicals can have negative environmental effects (Col. 4) and the perception that chemical can have negative health effects (Col. 5). For the first outcome, significant and negative interactions between treatment assignment and age as well as education suggest that the training impact decreases with farmers' age and education. However, we do not find similar effects for other perception outcomes. For both outcomes related to the perceptions regarding chemical inputs, we further find negative and significant interactions between treatment assignment and cultivated land size. These results suggest that the training had a lower impact on farmers who cultivated more land.

¹⁰ Registered in the AEA RCT registry: ID AEARCTR-0003840

Local Average Treatment Effect

Tables A10-A11 present the LATE estimates across the main outcomes when the invitation to the training is used as an IV for training attendance. Overall, the LATE estimates are slightly higher in magnitude than the ITT estimates. However, this difference is very small. For example, for the outcome ‘use of organic fertiliser’ we find an ITT effect of 12.6 percentage points compared to a LATE effect of 14 percentage points. The similarity between the LATE estimates and the ITT estimates can be explained by the high compliance, i.e. the high training attendance rate.

5. Discussion

In this section we summarize our main findings, relate them to the literature and elaborate on the underlying mechanisms linking the exposure to training and the uptake of organic farming practices. The main behavioural hypothesis we tested is whether the training led to an increased uptake of organic farming inputs. Eleven months after the training was implemented, we find indeed causal evidence that the training increased the uptake of self-produced organic fertiliser and organic pesticide. Our ITT effects are similar in magnitude to those found by Hörner et al. (2019) for the impact of an Integrated Soil Fertiliser Management program on the uptake of individual practices among smallholders in Ethiopia (9 – 22 percentage points). They are also comparable to those found by Kondylis et al. (2017) for the impact of an Sustainable Land Management training among (lead) farmers in Mozambique (17.6 percentage points).

We expect that the ‘success’ of the training was likely due to a combination of factors. Firstly, we expect that both, changes in knowledge and perception, have increased farmers’ propensity to experiment with and to adopt organic inputs in response to the training. This mechanism has also been suggested previously by other studies in the context of sustainable agriculture technologies (Hörner et al., 2019; Kondylis et al., 2017; Romero, Wollni, Rudolf, Asnawi, & Irawan, 2019). We have shown that assignment to the training raised knowledge levels considerably. The effects on knowledge levels are also in line with some previous studies (Fabregas, Kremer, & Schilbach, 2019; Hörner et al., 2019). However, other studies show that information provision may also fail to increase knowledge even if it increases the uptake of the promoted practice (Cole & Fernando, 2014; Kondylis et al., 2017). As outlined before, information in this context refers to information about how to use organic practices but also to why to use organic practices. Regarding the latter, we find that the training led to a more positive perception of the profitability of organic products and higher awareness for the negative effects that come with chemical inputs. So far, only a few studies have investigated the impact of information provision on perceptions and their role for agricultural technology adoption. A notable exception is a study by Romero et al. (2019) who investigate the impact of a randomized information treatment on biodiversity enrichment within smallholder oil palm plantations in Indonesia. The authors find that information provision significantly affected perceptions about the promoted biodiversity enrichment in the form of non-oil palm tree planting. Very similarly, we find that training had a positive impact on farmers’ perception of organic farming and their awareness for the potential negative effects of intensive chemical input use. While we expect that changes in knowledge and perceptions have contributed to the observed behavioural change, we fail to find conclusive evidence for this mechanism – at least in the short run. This is also due to the type of knowledge questions in our survey, which tend to test formal (e.g. regarding labels) rather than practical knowledge (e.g. how to produce organic fertiliser). Unfortunately, our data do not permit us to determine the temporal sequence between changes in knowledge, perceptions and actual experimentation and ultimately adoption.

A second possible mechanism through which the training may have operated is through an encouragement (or persuasion) effect. Endorsement of organic farming by an external trainer may have acted as a form of ‘expert validation’. This is in line with the evidence from qualitative interviews that we conducted with farmers who participated in our training. A farmer in Tasikmalaya, for example,

explained his renewed uptake of organic fertiliser made of banana trees as follows: *“So it has actually already been there for centuries. But after the training, I got reminded, I got confirmation in the sense that it is actually correct, that it has a good effect on my crops. Therefore, I reuse it again. But I already try it a long time ago actually. From my father, when I was a kid, I saw that he used that. But I wasn’t sure whether it was good or not”*. A similar mechanism might be beneath the observation made by some studies that an information treatment has increased the uptake of the promoted technology levels but has no effect on knowledge levels (Cole & Fernando, 2014; Kondylis et al., 2017).

Thirdly, the group setting of the training may have reinforced the training effect. We selected a maximum of three farmer groups per village to ensure that members from the same farmer group are jointly participating in the training. This may have benefitted farmers who prefer to experiment with new technologies in a group rather than by themselves. Experimenting in groups allows farmers to draw on their peers’ knowledge and moral support. Indeed, when asked at follow-up, 54% of new users of organic fertiliser and 74% of new users of organic pesticide reported having practiced training content with other training attendants. Furthermore, the participation of several members of one farmer group (including in most cases the farmer group head) may have had a more widespread effect on the perception of organic farming in the respective farmer groups. Studying the spatial patterns of organic farming in Honduras, Wollni and Andersson (2014) confirm the importance of social conformity. The authors find that perceived approval for organic farming by neighbours increased the likelihood to engage in organic farming.

While we find a positive impact of the exposure to the training on the experimentation with and adoption of organic inputs, farmers differ in their responsiveness to the training. We thus set out to investigate which factors may explain the heterogeneous training effect. Overall, we find only limited evidence for heterogeneous effects in relation to age, education or risk aversion. Evidence from previous studies is also mixed and there appears to be no conclusive pattern with respect to most factors such as age, gender or education in the context of agricultural conservation technologies (Knowler & Bradshaw, 2007). Some studies suggest that environmental concerns motivate farmers to convert to organic farming. This evidence comes for example from Ireland (Läpple & Rensburg, 2011) and Ghana (Kleemann, Abdulai, & Buss, 2014), but a study in Honduras cannot confirm this finding (Wollni & Andersson, 2014). Yet, in line with the study from Honduras, we find that awareness for agricultural pollution (measured at baseline) is not associated with a significant change in the effect of the training. We further find no evidence that the effect of the training was stronger among farmers who sell their products at the market. Instead, we find that farmers who produce only for their own consumption responded equally strong to the training. This is interesting given that price premiums and high profitability are frequently put forward as the main motivation for organic farming. In contrast, we find that the most frequently stated motivation for organic fertiliser use at follow-up is the improvement in soil quality.

Finally, the results raise the question why the training did not translate into a reduction in chemical fertiliser and only in a small reduction in pesticide use. An explanation may come from a study on pesticide use among smallholders in China (Liu and Huang, 2013). There the authors argue that pesticide use is correlated with risk aversion. Risk aversion is also a likely explanation in our case. Qualitative interviews with training participants further support this hypothesis as several farmers reported to experiment with organic farming only on a small piece of their land or experiment with it while still using chemical fertiliser.

6. Conclusion

In Indonesia, the uptake of organic farming practices remains very low despite efforts of the government and NGOs to promote it nationwide. Information provision in the form of training or agricultural extension is one potential policy instrument to further increase the uptake of organic farming. We used a randomized controlled trial to investigate the impact of a three-day-training on organic farming in two regions on Java.

While previous studies have shown for other agricultural practices that information provision and extension services can increase uptake (e.g. Hörner et al., 2019; Kondylis et al., 2017), few studies have examined this in the context of organic farming practices, even less with an experimental design. To our knowledge, this is one of the first experimental studies to evaluate technology adoption in the context of organic farming. Overall, our findings suggest that intense training is a promising policy tool to increase the uptake of organic farming. We find that training increased the uptake of organic farming practices, improved farmers' knowledge about organic farming and positively affected farmers' perception of organic farming. However, we do not observe complete conversion to organic farming. The most plausible reading of our finding is that the training has sparked interest and experimentation among training attendants but, as expected given the short observation window of just one year, has not yet led to full conversion and a full substitution of chemical inputs. Interestingly, we also find that farmers do not only respond to the price premium that is associated with organic products but also to the prospects of improved soil quality that is associated with organic farming practices.

One noteworthy caveat of our study is the reliance on self-reported organic input use and the lack of information about the quality of these inputs. While we cannot rule out the possibility of a desirability bias affecting our results, we expect this bias to be small, given the large number of follow up questions we asked if a farmer reported to use organic fertiliser or pesticide (e.g. on time of adoption, self-production, information source and motivation for use). We believe that without actually using the organic input, these follow-up questions are likely challenging to answer convincingly. Additionally, we gain confidence from the similarly positive training effects on knowledge levels. Furthermore, while we have identified a positive short term effect of training on the uptake of organic farming practices, future work should consider and evaluate how training can enhance long-term adoption. Studying long-term adoption will further allow shedding light on the socioeconomic impacts of organic farming adoption including farmer's profits and well-being.

Appendix

Table A1. Differences among info-session non-attendees by treatment group

	Control mean	Treatment mean	p-value
Tasikmalaya	0.56	0.58	-0.02
Male	0.82	0.85	-0.03
Age (yrs.)	55.47	53.19	2.29
Schooling (yrs.)	7.77	7.54	0.22
Asset index	1.64	1.50	0.14
Farming main activity	0.79	0.86	-0.07
Present bias	0.79	0.64	0.15*
Farmers' decisions matter (perc.)	0.54	0.63	-0.08
Agr. env. pollution problematic (perc.)	0.33	0.49	-0.16*
Cultivated land	0.29	0.46	-0.17**
Land ownership share	445.01	492.59	-47.58
Observations	129	112	241

Table A2. Testing for systematic attrition (OLS regression)

	(1) Attrition (=1)	(2) Attrition (=1)
Treatment	0.005 (0.013)	0.000 (0.013)
Tasikmalaya	0.058*** (0.018)	0.064*** (0.023)
Male		0.004 (0.019)
Age (yrs.)		0.001 (0.001)
Schooling (yrs.)		0.001 (0.003)
Asset index		0.004 (0.009)
Farming main activity		-0.026* (0.013)
Present bias		0.011 (0.009)
Farmers' decisions matter (perc.)		0.008 (0.016)
Agr. env. pollution problematic (perc.)		-0.013 (0.013)
Cultivated land		0.045 (0.028)
Land ownership share		-0.002 (0.017)
Constant		-0.026 (0.043)
Strata	Yes	Yes
Observations	1,201	1,201
R-squared	0.023	0

Notes: Robust standard errors clustered at village level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A3. Outcome variables at baseline and follow-up

		Baseline			Follow up		
		Control	Treatment	C-T	Control	Treatment	C-T
<i>Application</i>							
Organic fertilizer	1=self reported use of organic fertilizer (other than manure) during last cropping season; 0= otherwise	0.29	0.37	-0.08**	0.27	0.45	-0.18***
Organic pesticide	1=self reported use of organic pesticide during last cropping season; 0= otherwise	0.10	0.07	0.04*	0.07	0.14	-0.07***
Processed manure	1=self reported use of processed manure during last cropping season; 0= otherwise	0.40	0.37	0.02	0.33	0.40	-0.07**
<i>Knowledge</i>							
Labels	1=R knows there are specific labels for organic products; 0=otherwise	0.24	0.21	0.03	0.24	0.46	-0.22***
No land burning	1= R knows that as organic farmer it is not permitted to burn land; 0= otherwise	0.67	0.68	-0.02	0.74	0.89	-0.14***
Manure processing	1= R knows that as organic farmer, manure must be processed before application; 0=otherwise	0.67	0.65	0.02	0.77	0.84	-0.07***
Conversion period	1= R knows there is a conversion period, one cannot change from conventional to organic farming directly ; 0= otherwise				0.48	0.66	-0.18***
Buffer zone	1= R knows organic farmers must not plant directly next to conventional farmers, a buffer zone is required; 0=otherwise				0.49	0.71	-0.22***
<i>Perception</i>							
Modern	1= R classifies organic farming as a modern technology; 0= otherwise	0.41	0.39	0.02	0.37	0.46	-0.09***
Demand increased	1= R thinks that demand for organic products increased over the past five years; 0= otherwise	0.17	0.21	-0.04	0.37	0.53	-0.16***
Chemicals neg. env. effect	1= R thinks it is true that chemical inputs have a negative impact on the environment; 0= otherwise				0.52	0.64	0.12***
Chemicals neg. health effect	1= R thinks it is true that chemical inputs have a negative impact on farmers' health; 0= otherwise				0.71	0.78	-0.07***

Table A4. ITT effects on input application (OLS estimation for treatment arms)

	(1) Organic fertilizer	(2) Organic fertilizer <i>produced</i>	(3) Organic fertilizer <i>purchased</i>	(4) Organic pesticide	(5) Organic pesticide <i>produced</i>	(6) Organic pesticide <i>purchased</i>	(7) Processed manure	(8) No chemical fertilizer	(9) No chemical pesticide
Training	0.108*** (0.006)	0.128** (0.014)	0.047 (0.337)	0.051 (0.206)	0.037 (0.303)	0.011 (0.480)	0.046 (0.357)	-0.011 (0.514)	0.016 (0.794)
Training*Video (T*V)	0.046 (0.258)	0.031 (0.640)	0.035 (0.531)	0.026 (0.560)	0.012 (0.746)	0.002 (0.946)	0.040 (0.491)	0.007 (0.761)	0.100 (0.186)
Training*Role-play (T*R)	0.006 (0.861)	0.002 (0.969)	-0.025 (0.627)	0.095 (0.173)	0.063 (0.382)	0.045 (0.153)	-0.004 (0.950)	0.031 (0.237)	0.129* (0.088)
Wald test (p-value) T*V = T*R	0.521	0.587	0.212	0.205	0.451	0.204	0.636	0.377	0.902
Strata	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lagged Outcome	Yes	No	No	Yes	No	No	Yes	Yes	Yes
N	1,146	1,107	1,107	1,146	1,145	1,145	1,146	1,146	1,146

Note: Robust standard errors clustered at village level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A5. ITT effects on knowledge (OLS estimation for treatment arms)

	(1) Labels	(2) No land burning	(3) Manure processing	(4) Conversion period	(5) Buffer zone
Training	0.175*** (0.005)	0.120** (0.015)	0.017 (0.743)	0.138*** (0.007)	0.185*** (0.000)
Training*Video	0.012 (0.858)	-0.001 (0.976)	0.025 (0.656)	0.042 (0.442)	0.029 (0.422)
Training*Role-play	0.063 (0.373)	0.055 (0.259)	0.127** (0.031)	0.025 (0.706)	0.020 (0.615)
Wald test (p-value) T*V = T*R	0.409	0.159	0.0109	0.788	0.827
Strata	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Lagged Outcome	Yes	Yes	Yes	No	No
N	1,150	1,149	1,150	1,150	1,150

Note: Robust standard errors clustered at village level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A6. ITT effects on perception (OLS estimation for treatment arms)

	(1) Labels	(2) No land burning	(3) Manure processing	(4) Conversion period	(5) Buffer zone
Training	0.175*** (0.005)	0.120** (0.015)	0.017 (0.743)	0.138*** (0.007)	0.185*** (0.000)
Training*Video	0.012 (0.858)	-0.001 (0.976)	0.025 (0.656)	0.042 (0.442)	0.029 (0.422)
Training*Role-play	0.063 (0.373)	0.055 (0.259)	0.127** (0.031)	0.025 (0.706)	0.020 (0.615)
Wald test (p-value) T*V = T*R	0.409	0.159	0.0109	0.788	0.827
Strata	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Lagged Outcome	Yes	Yes	Yes	No	No
N	1,150	1,149	1,150	1,150	1,150

Note: Robust standard errors clustered at village level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A7. ITT effects on input application (OLS estimation for heterogeneity)

	(1) Organic fertilizer	(2) Organic fertilizer <i>produced</i>	(3) Organic fertilizer <i>purchased</i>	(4) Organic pesticide	(5) Organic pesticide <i>produced</i>	(6) Organic pesticide <i>purchased</i>	(7) Processed manure	(8) No chemical fertilizer	(9) No chemical pesticide
Training	0.175 (0.169)	0.143 (0.246)	0.071 (0.482)	0.211 (0.118)	0.277** (0.040)	-0.025 (0.612)	0.004 (0.978)	0.097 (0.170)	0.277* (0.085)
Training*Age	-0.001 (0.689)	-0.000 (0.973)	-0.000 (0.840)	-0.002 (0.279)	-0.004** (0.048)	0.001 (0.281)	0.001 (0.701)	-0.002 (0.136)	-0.003 (0.198)
Training	0.180** (0.013)	0.183*** (0.009)	0.042 (0.443)	0.081 (0.126)	0.066 (0.252)	0.011 (0.720)	0.050 (0.579)	0.009 (0.799)	0.248*** (0.006)
Training*Years of schooling	-0.007 (0.422)	-0.005 (0.503)	0.001 (0.872)	0.001 (0.789)	-0.001 (0.910)	0.002 (0.593)	0.001 (0.919)	-0.001 (0.818)	-0.019** (0.043)
Training	0.157*** (0.000)	0.152*** (0.000)	0.060* (0.090)	0.090** (0.049)	0.070 (0.121)	0.010 (0.521)	0.096** (0.021)	0.012 (0.544)	0.106* (0.056)
Training*Agr. pollution perceived as problem	-0.068 (0.165)	-0.028 (0.473)	-0.020 (0.674)	0.004 (0.934)	-0.018 (0.642)	0.037** (0.048)	-0.084 (0.120)	-0.024 (0.434)	-0.030 (0.663)
Training	0.149*** (0.000)	0.131*** (0.002)	0.056 (0.174)	0.087** (0.030)	0.060 (0.111)	0.028 (0.197)	0.058 (0.210)	0.027 (0.228)	0.091* (0.094)
Training*Cultivated land size	-0.071 (0.303)	0.024 (0.719)	-0.013 (0.858)	0.013 (0.815)	0.005 (0.912)	-0.005 (0.859)	-0.000 (0.999)	-0.078 (0.220)	0.006 (0.940)
Training	0.081* (0.059)	0.128*** (0.002)	0.038 (0.492)	0.063* (0.070)	0.021 (0.141)	0.045* (0.088)	0.052 (0.302)	-0.024** (0.044)	0.008 (0.910)
Training*Tasikmalaya	0.096* (0.064)	0.024 (0.694)	0.027 (0.644)	0.061 (0.371)	0.087 (0.172)	-0.038 (0.180)	0.014 (0.838)	0.054* (0.068)	0.181* (0.051)
Training	0.124*** (0.000)	0.141*** (0.000)	0.051 (0.158)	0.094*** (0.008)	0.058* (0.067)	0.026 (0.117)	0.062* (0.100)	-0.003 (0.798)	0.084* (0.099)
Training*Self-class. early adopter	0.012 (0.881)	-0.017 (0.835)	-0.003 (0.973)	-0.028 (0.623)	0.023 (0.603)	0.001 (0.985)	-0.032 (0.713)	0.038 (0.394)	0.073 (0.321)
Training	0.130*** (0.000)	0.134*** (0.000)	0.049 (0.137)	0.093** (0.011)	0.060* (0.075)	0.019 (0.219)	0.056 (0.123)	-0.003 (0.835)	0.090* (0.072)
Training*Self-class. high adoption willingness	-0.057 (0.482)	0.073 (0.439)	0.018 (0.864)	-0.042 (0.590)	0.020 (0.752)	0.105** (0.030)	0.035 (0.747)	0.060 (0.231)	0.042 (0.705)
Training	0.176** (0.015)	0.106 (0.192)	0.077 (0.317)	0.044 (0.451)	0.016 (0.735)	0.040 (0.354)	0.131 (0.167)	0.007 (0.170)	0.305*** (0.002)
Training*Present biased	-0.037 (0.525)	0.025 (0.676)	-0.019 (0.755)	0.035 (0.426)	0.034 (0.382)	-0.010 (0.718)	-0.054 (0.399)	-0.004 (0.888)	-0.158** (0.026)
Strata	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lagged Outcome	Yes	No	No	Yes	No	No	Yes	Yes	Yes
N	1,146	1,107	1,107	1,142	1,145	1,145	1,146	1,146	1,146

Note: Robust standard errors clustered at village level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A8. ITT effects on knowledge (OLS estimation for heterogeneity)

	(1) Labels	(2) No land burning	(3) Manure processing	(4) Conversion period	(5) Buffer zone
Training	0.387** (0.013)	0.039 (0.763)	-0.172 (0.203)	0.261* (0.096)	0.298** (0.040)
Training*Age	-0.003 (0.195)	0.002 (0.449)	0.004* (0.090)	-0.002 (0.521)	-0.002 (0.508)
Training	0.090 (0.208)	0.211*** (0.003)	0.083 (0.265)	0.094 (0.304)	0.186** (0.020)
Training*Years of schooling	0.013* (0.084)	-0.009 (0.203)	-0.002 (0.784)	0.008 (0.428)	0.002 (0.830)
Training	0.194*** (0.000)	0.206*** (0.000)	0.083** (0.044)	0.195*** (0.000)	0.203*** (0.000)
Training*Agr. pollution perceived as problem	0.012 (0.821)	-0.149*** (0.002)	-0.035 (0.455)	-0.074 (0.185)	-0.002 (0.974)
Training	0.205*** (0.000)	0.170*** (0.000)	0.096** (0.018)	0.205*** (0.000)	0.248*** (0.000)
Training*Years of schooling	-0.015 (0.857)	-0.098* (0.054)	-0.089 (0.234)	-0.134 (0.113)	-0.140 (0.102)
Training	0.206*** (0.000)	0.160*** (0.006)	0.096* (0.068)	0.186*** (0.002)	0.204*** (0.000)
Training*Tasikmalaya	-0.014 (0.851)	-0.046 (0.473)	-0.061 (0.327)	-0.054 (0.457)	-0.005 (0.929)
Strata	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Lagged Outcome	Yes	Yes	No	No	No
N	1,150	1,150	1,150	1,150	1,149

Note: Robust standard errors clustered at village level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A9. ITT effects on perceptions (OLS estimation for heterogeneity)

	(1) Modern	(2) Demand increased	(3) More profitable	(4) Chemicals: neg. for env.	(5) Chemicals: neg. for health
Training	0.228 (0.112)	0.219 (0.123)	0.136 (0.355)	0.304*** (0.010)	0.164 (0.143)
Training*Age	-0.003 (0.259)	-0.001 (0.563)	-0.000 (0.887)	-0.004* (0.057)	-0.002 (0.404)
Training	0.270*** (0.001)	0.240*** (0.002)	0.104 (0.187)	0.210*** (0.002)	0.097 (0.146)
Training*Years of schooling	-0.023** (0.018)	-0.012 (0.166)	0.001 (0.865)	-0.018** (0.010)	-0.004 (0.600)
Training	0.093** (0.039)	0.137*** (0.003)	0.130*** (0.006)	0.069* (0.088)	0.081** (0.015)
Training*Agr. pollution perceived as problem	-0.024 (0.699)	0.014 (0.824)	-0.031 (0.608)	-0.010 (0.868)	-0.029 -0.594
Training	0.099** (0.028)	0.141*** (0.008)	0.101** (0.032)	0.114*** (0.005)	0.102*** (0.001)
Training*Cultivated land size	-0.050 (0.524)	0.007 (0.930)	0.044 (0.603)	-0.153** (0.014)	-0.107** (0.017)
Training	0.082* (0.053)	0.099* (0.071)	0.107** (0.020)	0.084 (0.116)	0.066* (0.077)
Training*Tasikmalaya	-0.001 (0.992)	0.093 (0.198)	0.018 (0.796)	-0.042 (0.499)	0.002 (0.960)
Strata	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Lagged Outcome	Yes	Yes	No	No	No
N	1,150	1,150	1,150	1,150	1,150

Note: Robust standard errors clustered at village level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A10. LATE effects on input application (OLS estimation)

	(1) Organic fertilizer	(2) Organic fertilizer <i>produced</i>	(3) Organic fertilizer <i>purchased</i>	(4) Organic pesticide	(5) Organic pesticide <i>produced</i>	(6) Organic pesticide <i>purchased</i>	(7) Processed manure	(8) No chemical fertilizer	(9) No chemical pesticide
ITT	0.126*** (0.000)	0.139*** (0.000)	0.051 (0.131)	0.091*** (0.010)	0.062* (0.052)	0.027* (0.095)	0.058* (0.094)	0.001 (0.933)	0.093* (0.057)
LATE	0.140*** (0.000)	0.154*** (0.000)	0.057 (0.121)	0.101*** (0.007)	0.068** (0.043)	0.030* (0.087)	0.065* (0.084)	0.001 (0.932)	0.103** (0.047)
Strata	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lagged Outcome	Yes	No	No	Yes	No	No	Yes	Yes	Yes
N	1146	1107	1107	1142	1145	1145	1,146	1,146	1,146

Note: Robust standard errors clustered at village level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A11. LATE effects on knowledge (OLS estimation)

	(1) Labels	(2) No land burning	(3) Manure processing	(4) Conversion period	(5) Buffer zone	(6) Knowledge index
ITT	0.200*** (0.000)	0.138*** (0.000)	0.067** (0.038)	0.161*** (0.000)	0.202*** (0.000)	0.759*** (0.000)
LATE	0.222*** (0.000)	0.153*** (0.000)	0.074** (0.029)	0.179*** (0.000)	0.224*** (0.000)	0.842*** (0.000)
Strata	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Lagged Outcome	Yes	Yes	Yes	No	No	
N	1150	1149	1150	1150	1150	1150

Note: Robust standard errors clustered at village level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A12. LATE effects on perceptions (OLS estimation)

	(1) Modern	(2) Demand increased	(3) More profitable	(4) Chemicals: neg. for env.	(5) Chemicals: neg. for health
ITT	0.082** (0.015)	0.143*** (0.000)	0.115*** (0.001)	0.064** (0.046)	0.067*** (0.005)
LATE	0.091** (0.011)	0.159*** (0.000)	0.128*** (0.000)	0.071** (0.038)	0.075*** (0.003)
Strata	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Lagged Outcome	Yes	Yes	No	No	No
N	1150	1150	1150	1150	1150

Note: Robust standard errors clustered at village level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

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