Chapter 3
The Case of Cote D'Ivoire: Learning from Experts of Rice Farming Management and Peer Farmers About Rice Production

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Abstract Technological innovation is vital to economic growth and food security in sub-Saharan Africa where agricultural productivity has been stagnant for a long time. Extension services and learning from peer farmers are two common approaches to facilitate the diffusion of new technologies, but little is known about their relative effectiveness. Selection bias, whereby well-motivated training participants would perform better even without extension services, as well as knowledge spillovers, where non-participants can indirectly benefit from extension services, are among the major threats to causal inference. Using a unique sequential randomized experiment on agricultural training, this chapter attempts to meet the dual objectives of executing rigorous impact evaluation of extension services and subsequent spillovers on rice production in Cote d'Ivoire. Specifically, to reduce selection bias, we randomly assigned eligibility for training participation; and to satisfy the stable unit treatment value assumption, control-group farmers were initially restricted from exchanging information with treated-group farmers who had received rice management training. Once the positive impacts were confirmed one year after the training, information exchange between the treated and control farmers was encouraged. We found that the initial performance gaps created by the randomized assignment disappeared over time, due presumably to social learning from peer farmers. A detailed analysis concerning the information network and peer effects provided suggestive evidence

This chapter draws heavily on Takahashi et al. (2019b).

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that there were information and technology spillovers from treated to control farmers after removing the information exchange restriction. Overall, our study demonstrates that information dissemination by farmers can be as effective in improving practices as the initial training provided by extension services.

3.1 Introduction

There has been increasing interest in replicating the Asian Green Revolution in sub-Saharan Africa (SSA), where stagnant agricultural productivity has long been an impediment to economic growth and food security (Evenson and Gollin 2003; Dawson et al. 2016; Bachewe et al. 2018). Emerging studies have identified lowland rice as the most promising staple crop among major cereals in the SSA, because of the high transferability of improved technology from Asia (Otsuka and Larson 2013, 2016). While the application of modern seeds and increased use of chemical fertilizer have been emphasized, Asian-type yield-enhancing rice-growing technologies also include improved management practices, such as bunding, leveling, and straight-row transplanting.[1] Since these management practices are knowledge-intensive and require deep understanding and careful execution to fully exploit yield potentials, the dissemination of those technologies may be difficult without credible and persuasive sources of information. How to reduce information friction in the diffusion process remains an important area of research.

Agricultural extension services are designed to play such a role by delivering advanced knowledge from lab and experimental fields to farmers. Yet, directly training millions of small farmers may be prohibitively costly. An alternative method is farmer-to-farmer training, in which selected progressive (contact) farmers participate in training organized by extension workers and then are encouraged to share skills learned with others in their network. While this contact farmer approach is common in many developing countries, evidence of its efficacy is mixed. For example, Feder et al. (2004), Tripp et al. (2005), and Kondylis et al. (2017) found that there is limited impact from the performance of contact farmers who have adopted new agricultural technologies on other farmers. On the other hand, Krishnan and Patnam (2014) and Nakano et al. (2018a, b) show that contact farmers increasingly adopt new technologies immediately after training, and demonstrated improvements spill over to other farmers later through farmer-to-farmer training.

An apparent empirical challenge to the examination of the effectiveness of the farmer-to-farmer extension system is selection bias, which might be one of the potential reasons for mixed empirical results: since progressive, contact farmers tend to be more motivated than ordinary farmers, they would perform better even without training. If this is the case, simply mimicking contact farmers' observed practices

[1] The existing studies show that with those technological packages rice yield can be more than 4 tons per hectare in SSA, which is comparable to or even higher than that in Asian countries like India (Otsuka and Larson 2013, 2016; Ragasa and Chapoto 2017; Nakano et al. 2018a, b).

may not necessarily improve fellow farmers' performance because there are differences in innate characteristics between the contact farmers and fellow farmers. Thus, it is of great importance to determine whether new technologies taught in the training period per se would have the intended positive impact. Rigorously implementing an impact evaluation of this kind, however, involves another empirical challenge in the presence of knowledge spillovers, because non-trained fellow farmers can indirectly benefit by imitation and learning from their trained peers, attenuating the true impact of training. This is a violation of the stable unit treatment value assumption (SUTVA) in social science, according to which the observation of one unit should not be affected by the treatment of other units.

To execute a rigorous impact evaluation of both extension services and the subsequent social learning in a unified framework, we implemented sequential field experiments related to rice production management training for this study, in collaboration with rice-growing farmers in Cote d'Ivoire. To mitigate selection bias, we conducted a randomized controlled trial (RCT), where a subset of randomly selected farmers from each sample site received training in the form of a short course in rice production management. To fulfill the SUTVA, farmers from the treated group were initially asked not to transmit information about their training to the control farmers and the latter were requested to refrain from asking treated farmers for agricultural advice. After one year of observation, we examined whether training had the intended positive impacts for trained farmers, such as the adoption of recommended agronomic practices, improved rice yields, and improved profit. Once the positive impacts were confirmed, we relaxed the restriction and started promoting spillovers by encouraging farmers to exchange information without any monetary incentive given to the trained farmers.

We conducted household surveys three times: at the baseline before the training (January 2015 to May 2015), at the midline one year after the start of training (March 2016 to May 2016), and at the endline two years after the end of the training (March 2017 to May 2017). Using these three-year panel data, we identified the evolution of both the intention-to-treat (ITT) effect and the treatment-on-the-treated (TOT) effect of the training. Furthermore, to explore the dynamics of information exchange between treated and control farmers in the first and second year of observation, we conducted a detailed analysis of information flows and the existence of peer effects using social network data collected in the mid- and endline surveys.

Our main findings are summarized as follows. We found that while the adoption rates of most improved rice management practices were unexpectedly high even at the baseline, the treated farmers were more likely to adopt improved practices by a year after training (i.e., the midline), such as transplanting in rows, canal/drainage construction, and field leveling after training. Higher adoption rates of those recommended agronomic practices lead to improved rice yield as well as increased income per hectare among treated farmers at the midline. Once all farmers were encouraged to exchange information later, the productivity gaps between treated and control farmers narrowed sharply by two years after training (i.e., the endline). It may seem reasonable to interpret this convergence as the sign of short-lived impacts of training where trained farmers dis-adopt new practices and return to traditional ones. However, we observed that trained farmers continued to adopt improved agronomic practices at the endline, and control farmers followed them. Our detailed network analysis based on a dyadic regression further revealed that information flows from treated to control farmers were less active than between control farmers (the reference group) at the midline but become more active at the endline. These results together suggest that farmers largely followed our guidance not to exchange information on rice production within the initial experimental phase, which mitigates estimation bias, if any, in our impact evaluation of training. Yet, once such a restriction is abolished and information exchange is encouraged, control farmers can successfully catch up with treated farmers through social learning. These results imply the importance of not only extension services to trigger the adoption of improved agricultural technologies, but also of social learning for their wider diffusion.

This study contributes to the literature on the role of social learning in the diffusion of agricultural technology (Foster and Rosenzweig 1995; Munshi 2004; Bandiera and Rasul 2006; Conley and Udry 2010). Most previous studies agree with the high potential of social learning, but there is little consensus as to the relative effectiveness of direct training by extension workers and learning from peer farmers. Moreover, a growing number of studies focus on determining who should be targeted to increase the initial adoption rate and to facilitate social learning (Beaman and Dillon 2018; BenYishay and Mobarak 2018; Macours 2019; Shikuku 2019). Our study demonstrates that information dissemination by farmers can be as effective at improving practices as the initial training provided by extension services, which is in line with the study of Nakano et al. (2018a, b). Unlike their study, however, our results show that the entry points to disseminate information are not necessarily the progressive, contact farmers, but can be the ordinary farmers who compose the majority of the rural community. Moreover, unlike BenYishay and Mobarak (2018), who revealed the importance of financial incentives for trained farmers to promote technology diffusion, our results also suggest that social learning can be facilitated by encouragement without any incentive.

The rest of this chapter is organized as follows. Section 3.2 explains the study setting, sampling framework, and experiment design, and examines the summary statistics of our sample. Section 3.3 explains our estimation strategy on the dynamic impact of training and discusses estimation results. Section 3.4 conducts a detailed analysis of the information network and explains the estimation results. Section 3.5 discusses the potentials and limitations of our study while referring to external validity and ethical concerns, and Sect. 3.6 concludes this chapter.

3.2 Survey and Experimental Design

3.2.1 The Study Setting

The study took place in the Bellier and Gbeke regions of Cote d'Ivoire, near the capital city of Yamoussoukro. Like other West African countries, rice is one of the major staple foods in this country and its consumption has exceeded domestic production (Fig. 3.1). The government has tried to increase rice yields to sustain food security and save foreign exchange reserves.

The adoption rate of modern inputs, such as improved seeds and chemical fertilizer, is higher in Cote d'Ivoire than in other rice-growing countries in the SSA. However, several recommended agronomic practices, including straight-row transplanting, that have been proven to boost rice yield in tropical Asia as well as other SSA countries, have not been adopted widely (David and Otsuka 1994; Otsuka and Larson 2013, 2016). There is thus room for management training to improve the performance of rice production.

The two regions, Bellier and Gbeke, were selected to improve domestic rice production and increase the quantity of marketed rice under a bilateral official development assistance (ODA) program between the Ivoirian and Japanese governments. Japanese technical experts were dispatched from 2014 to 2018 through an ODA scheme organized by the Japan International Cooperation Agency (JICA). There are a total of 107 production sites suitable for rice production within those two regions,

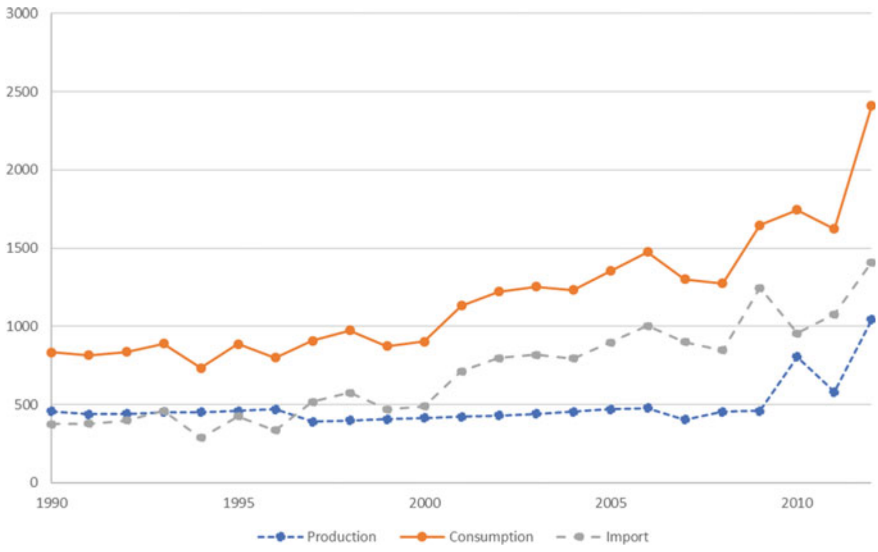

Fig. 3.1 Rice Production, Consumption, and Imports, Cote d'Ivoire (unit 1000 tons). *Note* The authors' calculation from FAOSTAT data. Rice consumption is measured by apparent consumption, that is production plus imports minus exports

which are all located in the lowlands. Some production sites have sufficient access to irrigation water and are able to cultivate rice twice in a good year. Others are in low-humidity zones (called bas-fonds) and are dependent on rainfall. The main rice cultivation season is roughly from July to December. If irrigated, a second cycle starts around January/February. When water is insufficient, farmers produce other crops, such as yams and peanuts, or leave the paddy field to fallow. Since these two regions are agro-climatically more favorable for rice production than other areas of the country, farmers have received various rice cultivation training packages provided by international donors, including JICA, World Bank, and AfricaRice (formerly known as WARDA [West Africa Rice Development Association], whose headquarters is located within Cote d'Ivoire), as well as local extension agencies, including the Agence Nationale d'Appui au Développement Rural (ANADER).

Out of 107 production sites, two were initially selected for the JICA project in 2014. Thereafter, the target area was expanded every year until 2018 to cover a total of 26 sites. This study relies on the data from the eight production sites selected in 2015. To choose our study sites, we closely collaborated with technical experts. Admittedly, the selection of study sites was not completely random because technical experts have a target to cover 1,500 hectares of land within the five-year project period. Thus, the study sites are relatively larger in operational size than the remaining sites in the Bellier and Gbeke regions. Since the impact of training may potentially vary by agro-ecological and institutional conditions, we classified all potential production sites in terms of access to irrigation and the existence of prior rice training. We selected two sites from each combination of with and without irrigation and prior training, generating a sample of eight production sites in total.[2]

3.2.2 Sampling Structure and Experimental Design

Prior to the experiment, we had meetings with farmers belonging to agricultural cooperatives at each selected site.[3] The objective of the meeting was to explain our implementation plan and obtain consent from farmers (see Fig. 3.2 for the timeline, data type, and sample size at each implementation period).

Based on an agreement with technical experts, we outlined our plan to the farmers as follows: (1) We would like to conduct a social experiment to assess the impact of training and ask farmers to cooperate with us; (2) We would group farmers randomly into two, with one eligible to receiving the training offered by JICA experts while the other was expected to apply the best management practice they had access to;

[2] Since the facility was generally weak and its capacity was small, heterogeneity was observed in accessibility of irrigation water within the same production site. Furthermore, even if some farmers at a production site received past training, this does not imply that all farmers in that community were eligible and received the same training.

[3] Virtually all rice-growing farmers belong to agricultural cooperatives, which manage the allocation of machinery and current inputs (i.e., seeds and chemical fertilizer) exclusive for cooperative members.

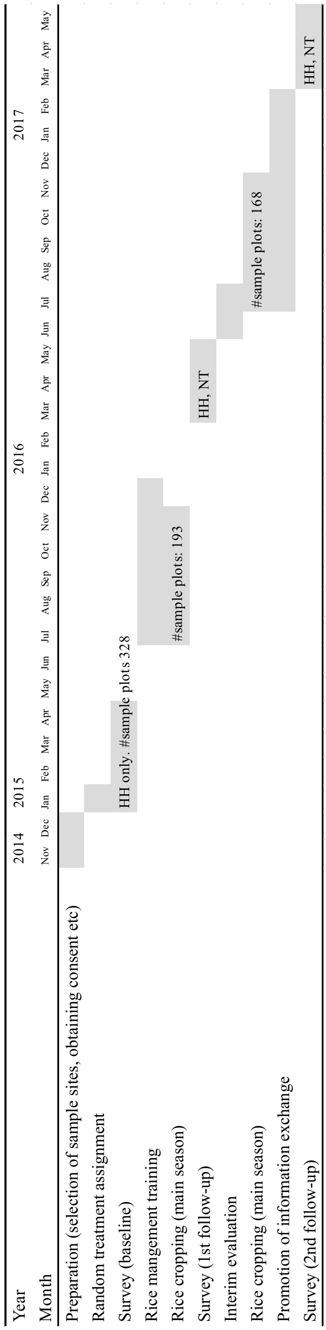

Fig. 3.2 Timeline of Implementation. *Note* HH refers to the household survey, while NT refers to the network survey

(3) All farmers including control farmers were to be provided with the necessary inputs, such as improved seeds, chemical fertilizer, and herbicide, on credit[4]; (4) The experimental phase was to last one year, during which farmers belonging to different groups would be expected not to exchange information about techniques and management practices taught in the training. Specifically, we requested treated farmers not to transmit such information and asked control farmers to refrain from asking treated farmers for agricultural advice; (5) Before and after the experiment, we would conduct household surveys for impact evaluation; (6) If farmers followed our guidance and treated farmers did not transmit information on rice production management, we would acquire precise and valuable knowledge regarding the effectiveness of the technological package taught in the training in their settings; (7) After the impact assessment, we would share which technology (i.e., conventional practice vs. one taught in training) was found to be superior; and (8) After the experimental phase, farmers would be encouraged to share information to facilitate knowledge diffusion.

While unequal treatment during the experimental phase could be a source of tension between treated and control farmers, we attempted to make them feel neither lucky nor unlucky in their treatment status. Rather, we emphasized that once we know which technology is better, everyone can benefit from such knowledge and that the success of this social experiment depends crucially on whether farmers exchange information or not within one year after the training, and also on whether they provide accurate information in the surveys. This sort of explanation seemed to ease tensions. Moreover, despite being uncommon, restrictions on information exchange were accepted by farmers without any revealed complaints once they understood the objectives of the research. Indeed, farmers showed a strong willingness to engage in this experiment.

After obtaining consent, we collected individual member lists from each agricultural cooperative. Out of 414 farmers on the shortlist, 275 households were found to be active rice producers who had cultivated rice at least once in the preceding year. These 275 households constitute the primary sample in this study to whom we assigned eligibility to participate in the training.[5] One half of the sample households were randomly selected as a treatment group and the other half selected as a control group at each site. Randomization was implemented at the farmer level within each site. Everyone in the sample knew not only own treatment status, but also that of their peers. We then conducted the baseline survey with those households from January 2015 to April 2015. The data obtained covered household demographic characteristics, details of rice production on all plots, other household income-generating activities, and household asset holdings.

[4] Interest of 3% per season was charged on these inputs, and farmers had to repay the loan to their agricultural cooperation after harvest.

[5] More precisely, 295 farmers were active rice producers in that they have at some time cultivated rice. 16 farmers were dropped from the analysis because of the lack of baseline data about their rice cultivation, although they were also candidates for random assignment and eligible to participate in training if they were in the treatment group. Out of the remaining 279, 4 households were dropped due to a lack of treatment information.

Technical experts provided short classroom training sessions to extension agents of ANADER and three key farmers who were selected from each site.[6] Those extension agents and key farmers in turn offered on-site training to eligible farmers under the supervision of technical experts. This training consisted of (a) land preparation, including bunding and land leveling, which is crucial to reduce the amount of water wasted and to promote the even growth of rice plants, (b) water control, including canal construction and maintenance, which is important in the management of water levels in rice fields during the growth period, (c) seed selection and incubation, (d) fertilizer and herbicide application, (e) transplanting in rows, which can be adopted to facilitate other complementary management practices such as hand or rotary weeding and even the application of fertilizers, herbicides, or insecticides, and (f) harvest and post-harvest management. To mitigate noncompliance, particularly in the participation of the control farmers, local counterparts visited every session of the training and recorded who participated in it. The on-site training proceeded gradually to meet the actual rice cultivation cycle, and, in total, it was held at least six times from June to November 2015 to cover the key practices.[7]

We conducted follow-up surveys twice, the first soon after the training period (March 2016 to May 2016) and, the second two years after training (March 2017 to May 2017), in which detailed data on information exchange across network members were also collected. Because there was a severe lack of rainfall during the 2015–2016 growing seasons, rice cultivation was difficult in those years. Most households could not cultivate rice in the sub-season. Even focusing on the main season, the number of sample plots cultivated for rice dropped sharply to 193 in the midline and further to 168 in the endline survey because of insufficient water. Due to the resulting lack of sufficient observations, we focused on the main season crop in the subsequent analysis and took attrition into consideration where relevant.

3.2.3 Descriptive Statistics and Balancing Test

Table 3.1, Columns (1)–(3) show the balance test on baseline characteristics for the sample household and plots in the main season. 275 households cultivated 328 rice fields in the baseline, of which 135 households and 160 plots belong to the treatment group.

About 72% of treated farmers had attended training at least once, while almost no (only two cases) control farmers had done so, indicating that control farmers largely adhered to our request and did not participate in the training. The attendance rate at

[6] These two-step approaches were proposed by JICA experts. Three key farmers selected for direct training by JICA experts were not included in our sample.

[7] A team of technical experts visited each production site frequently to organize the carefully designed on-site training in appropriate and timely agronomic techniques following the agricultural calendar.

Table 3.1 Baseline balance of sample plots by treatment and attrition status

	Treated (1)	Control (2)	Mean difference (3)	Attrition (4)	Non-attrition (5)	Mean difference (6)
Household Charactersitics						
Treatment (=1)	1.000 [0.000]	0.000 [0.000]	1.000	0.478 [0.047]	0.503 [0.040]	−0.025
Ever attend the training (=1)	0.719 [0.039]	0.014 [0.010]]	0.704***			
Attendance rate	0.383 [0.028]	0.002 [0.002]	0.380***			
Head's age (years)	44.014 [1.043]	44.341 [1.159]	0.326	43.611 [1.079]	44.543 [1.080]	−0.933
Head's education (years)	2.892 [0.340]	2.910 [0.323]	0.018	2.496 [0.345]	3.199 [0.314]	−0.703
Head is male (=1)	0.849 [0.030]	0.844 [0.031]	−0.004	0.788 [0.039]	0.889 [0.025]	−0.101**
HH size	8.712 [0.395]	9.000 [0.382]	0.288	2.892 2.892	8.383 [0.337]	1.157**
Number of plots	1.201 [0.034]	1.185 [0.034]	−0.016	1.096 [0.028]	1.262 [0.034]	−0.167***
Log asset value	4.532 [0.100]	4.470 [0.125]	−0.063	4.142 [0.100]	4.755 [0.111]	−0.613***
F-test of joint significance			0.090			5.829***
Number of household observations	135	140		113	162	
Plot Characteristics Plot size (ha)	0.571 [0.046]	0.472 [0.023]	0.099*	0.360 [0.026]	0.618 [0.036]	−0.258***
Owner (=1)	0.756 [0.034]	0.774 [0.032]	−0.018	0.847 [0.032]	0.712 [0.032]	0.135***
Leaseholder (=1)	0.188 [0.031]	0.161 [0.028]	0.027	0.089 [0.026]	0.229 [0.029]	−0.141***
Sharecropper (=1)	0.025 [0.012]	0.018 [0.010]	0.007	0.016 [0.011]	0.024 [0.011]	−0.008
Others (=1)	0.025 [0.012]	0.048 [0.016]	−0.023	0.040 [0.018]	0.034 [0.013]	0.006
F-test of joint significance			1.323			8.363***
Number of plot observations	160	168		124	204	

Standard deviations in brackets. *** p<0.01, ** p<0.05, * p<0.1

the training, a percentage representing how many times out of six key training opportunities each farmer attended, was about 38% among treated farmers. On average, households were large (about nine persons) and headed by a male in their mid-40 s with minimal or no formal education.

We conducted a joint significant test except for the treatment and attendance variables in Column (3), demonstrating that we failed to reject the zero-null hypothesis. This suggests that our randomization was mostly successful.

We present the remaining data using plots rather than households as the unit of observation because some characteristics vary at the plot level and because the main regression analysis is conducted at this level. The data include all rice plots cultivated by sample farmers. The average plot size was relatively small, approximately 0.5 hectare. Although the treatment status was randomized, the difference in the plot size between treatment and control farmers was found to be statistically significant. Most land was operated under owner cultivation. If rented, this was generally a fixed-rent contract. The joint significant test in Column (3) again shows that these plot-level variables are jointly statistically not different by the treatment status.

Columns (4)–(6) of Table 3.1 compare the baseline characteristics of attrition and non-attrition samples with a t-test of the equality of the mean between the two and the associated joint significance F-test. Out of 328 plots, 204 continued rice cultivation in either the midline or the endline year, or both.

While attrition is not sensitive to treatment status, many observable characteristics notably differ between attrition and non-attrition samples[8]: on average, attrition samples were more likely to be female-headed with less education, larger in household size, smaller in the number and size of cultivation plots, and were more likely to own rice plots. The joint significance test shows that the zero-null hypothesis is strictly rejected, implying that attrition is non-random. This non-random sample attrition is a potential threat to causal inference, and should be addressed in the econometric analysis.

Table 3.2 presents the changes in outcome variables of interest regarding the rice management practices and productivity of non-attrition samples over time. We again show the results of t- and F-tests for treated and control plots. In addition, columns (10) and (11) present an unconditional difference-in-differences (DID) regression estimate of the treatment effect (i.e., the difference in the time trend between treated and control plots).

The adoption of recommended management practices was generally quite high even in the baseline (Panel A). Because of its proximity to AfricaRice, adoption of the modern variety of rice was complete and uptake rates had reached 100%.[9] The use of chemical fertilizers was also remarkably high by SSA standards: on average, more than 200 kg/ha of fertilizer, such as NPK and UREA, were applied. In addition

[8] Farmers at the irrigation sites were more likely to continue rice cultivation, and therefore, in non-attritors. Attrition was caused partly because a household did not cultivate rice either at the midline or endline, and partly because a household used different plots over time.

[9] The vast majority of farmers use WITA-9, a high-yielding variety that is tolerant to rice yellow mottle virus and iron toxicity and has a maturity period of about 110 days.

Table 3.2 Changes in outcome variables by treatment status: baseline, midline, and endline

	Year 1			Year 2			Year 3			Unconditional DID	
	Treated (1)	Control (2)	Mean difference (3)	Treated (4)	Control (5)	Mean difference (6)	Treated (7)	Control (8)	Mean difference (9)	Year 2–Year 1	Year 3–Year 2
Panel A											
Fertilizer (kg/ha)	214.071 [19.979]	254.340 [32.869]	−40.269	248.822 [15.937]	261.288 [17.609]	−12.466	232.750 [21.745]	255.110 [17.994]	−22.360	27.803 [46.061]	−9.894 [36.572]
Seed selection (=1)	0.906 [0.029]	0.864 [0.034]	0.042	0.929 [0.026]	0.978 [0.015]	−0.050	0.976 [0.017]	0.976 [0.017]	−0.000	−0.092* [0.055]	0.050 [0.040]
Levelling (=1)	0.772 [0.040]	0.791 [0.039]	−0.019	0.857 [0.036]	0.677 [0.049]	0.180***	0.867 [0.037]	0.810 [0.043]	0.058	0.199** [0.081]	−0.122 [0.083]
Canal/drainage construction/repairing (=1)	0.906 [0.028]	0.879 [0.032]	0.027	0.867 [0.034]	0.731 [0.046]	0.136**	0.855 [0.039]	0.929 [0.028]	−0.073	0.109 [0.071]	−0.209*** [0.076]
Transplanting in row (=1)	0.054 [0.021]	0.019 [0.014]	0.035	0.378 [0.049]	0.108 [0.032]	0.270***	0.349 [0.053]	0.179 [0.042]	0.171**	0.235*** [0.063]	−0.099 [0.090]
Panel B											
Rice Yield (ton/ha)	3.440 [0.164]	3.940 [0.174]	−0.499**	4.052 [0.238]	3.671 [0.192]	0.382	3.416 [0.203]	3.724 [0.202]	−0.307	0.881** [0.387]	−0.689 [0.424]
Gross output value (000 CFAF/ha)	603.159 [32.452]	669.393 [31.737]	−66.233	645.433 [37.660]	582.479 [31.545]	62.954	536.090 [31.675]	597.471 [31.760]	−61.380	129.187* [66.960]	−124.334* [67.446]
Rice income (000 CFAF/ha)	405.308 [31.311]	405.091 [32.544]	0.217	413.726 [36.667]	353.458 [28.192]	60.268	232.174 [38.128]	292.198 [32.294]	−60.024	60.051 [64.909]	−120.292* [68.277]
Rice profits (000 CFAF/ha)	331.539 [29.863]	320.196 [34.177]	11.343	243.209 [51.905]	230.344 [32.074]	12.864	108.260 [40.102]	155.150 [31.341]	−46.890	1.522 [76.013]	−59.754 [81.382]

(continued)

Table 3.2 (continued)

	Year 1			Year 2			Year 3			Unconditional DID	
	Treated (1)	Control (2)	Mean difference (3)	Treated (4)	Control (5)	Mean difference (6)	Treated (7)	Control (8)	Mean difference (9)	Year 2–Year 1	Year 3–Year 2
F-test of joint significance			1.516			4.615***			1.777*		
Number of Household Observations	81	81		78	73		65	61		313	288
Number of Plot Observations	101	103		98	93		83	84		395	358

Standard deviations in brackets for mean values, and standard errors in brackets for unconditional difference-in-difference
*** p<0.01, ** p<0.05, * p<0.1

to these external inputs, the adoption rate of improved agronomic practices was mostly high. In our sample, about 90% of plots had selected better seeds by water or winnowing and constructed/repaired water canal/drainage systems in the baseline. It seems that the initial adoption rates were relatively low for leveling fields and greatly so for transplanting in a row, which might be technologies with some room for further improvement from training.

Panel B shows the rice productivity and profitability of sample plots. Gross production value per hectare is computed by multiplying the rice yield (1000 kg/ha) with the price received (CFAF/kg).[10] Rice income per hectare is equal to the gross production value minus paid-out costs, including land rent, irrigation fees, costs of purchased chemicals, and machinery rental, divided by the plot size. Profits per hectare are equal to rice income minus imputed family labor costs, divided by the plot size. To impute family labor costs, we used the typical prevailing hired wage rate for transplanting in each village.[11] The average yield exceeds 3.4 tons/ha which is significantly higher than the average of other countries in SSA of just above 2 tons/ha (Otsuka and Larson 2016). The average gross output value, rice income, and profits per hectare were about 600 thousand CFAF (or approximately 1,065 USD), 405 thousand CFAF (or 719 USD), and 320 thousand CFAF (or 568 USD), respectively.

The table also shows that while there were no statistically significant differences in the baseline adoption rate of recommended practices, treated farmers were more likely to adopt leveling, canal/drainage construction/repairs, and transplanting in rows at the time of the midline survey (Column (6)). Looking at each practice in detail, the increased adoption rate among treated farmers relative to control farmers stems partly from the fact that the control farmers did not continue several management practices, while the treated farmers did. This applies to leveling and canal/drainage construction/repairs. It is likely that insufficient rainfall, which tends to attenuate the impact of those improved practices on productivity and profitability, significantly reduced their adoption among control farmers, holding other things constant.

Meanwhile, control farmers increasingly adopted seed selection and transplanting in rows by the midline survey, and once information sharing was encouraged, the incremental adoption rate of most management practices between the mid- and endline was higher for control farmers (Column (11)). These observations suggest the existence of spillovers not only in the endline, but also the midline despite the restriction of information exchange during that period: indeed, there is room for seed selection and transplanting in rows to spill over because seed selection is relatively easy to imitate, and because transplanting depends largely on hired laborers who can assist both treatment and control farmers. Whether such technology dissemination, if any, significantly alters our view of the impact of training is one of the major issues addressed in later sections of this chapter.

[10] 1 USD is equivalent to 563 CFAF as of January 2015.

[11] One can use the different wage rate, such as for land preparation. However, since the land preparation contract is often made simultaneously with rental contract of a machine with an operator, it is difficult to extract only labor costs. On the other hand, our field observations revealed that wage rates for other activities, such as weeding, and harvesting are very close to those for transplanting.

On the other hand, all outcome variables except rice yield were not significantly different between the treated and control samples in the baseline, and no outcomes were significantly different in the midline and endline surveys. The unconditional DID estimates show that treatment plots increase rice yield and revenue between the baseline and midline more than control plots, while the reverse was true between the midline and endline surveys.

These results suggest that while the initial adoption rates of most management practices were already high, the treated farmers further improved or continued their rice management practices and performed better than control farmers in the first year after training. However, control farmers caught up with treated farmers in the following year. While these observations support the existence of positive training impacts in the first year and social learning in the following year, there are some reservations about the descriptive statistics. The following sections examine in more detail whether training has real impact and whether spillovers exist especially between the midline and endline surveys.

3.3 Dynamic Impacts of Training

3.3.1 Estimation Strategy

To identify the causal relationships between the provision of training and outcomes of interest, we estimate intention-to-treat (ITT) and treatment-on-the-treated (TOT) effects. We examine the average impacts of all production sites, allowing the impacts to vary across time. We are particularly interested in whether the training brings intended positive impacts in the first year after the training and whether the gap generated by the experiment decreases over time through spillovers in the next year. Following McKenzie (2012), we employ an analysis of covariance (ANCOVA) model in the form of:

$$Y_{ijt} = \beta_0 + \gamma Y_{ij0} + \beta_1 T_t + \beta_2 D_{ij} + \beta_3 (T_t \times D_{ij}) + X_{ij0}\delta + \mu_j + \varepsilon_{ijt} \quad (3.1)$$

where Y_{ijt} and Y_{ij0} are the post- and pre-treatment outcome variables of plot i in production site j at time t (i.e., either midline or endline) and time 0 (i.e., baseline), respectively; T_t is a dummy variable for the endline data; D_{ij} is a dummy variable equal to one if a household is eligible to participate in the training (ITT estimate) or a continuous variable for the attendance rate of training, instrumented by the treatment status (TOT estimate)[12]; X_{ij0} is a set of baseline control variables; μ_j is the time-invariant fixed effect at the production site; and ε_{ijt} is the unobserved error term. The parameters of interest are β_2 and β_3. The former is expected to capture the

[12] Strictly speaking, this is the local average treatment effect (LATE). However, because almost no control farmers attended on-site training, our estimate can be considered to be TOT (Angrist and Pische 2008).

short-term impacts of training under the imposition of the SUTVA, while the latter represents the mixture of the longer-term training impacts and spillover effects when the SUTVA is relaxed. We note that the pure training impact can be estimable only in the short term.

As outcome variables, we focus on the use of chemical fertilizer (kg/ha), the adoption of seed selection by water or winnowing (=1), leveling (=1), canal/drainage construction/repairing (=1), and transplanting in row (=1) as well as rice yield (ton/ha), gross output value (‘000CFAF), rice income (‘000CFAF), and rice profit (‘000CFAF) per hectare. When the outcome is binary, we apply a linear probability model. As baseline control variables, we include household size, household head’s characteristics (including age, gender, and years of education), plot characteristics (including parcel size and tenure status dummies), and the logged value of household assets at the baseline survey. We cluster all standard errors within production sites.[13]

While the random assignment of treatment status should make the treatment and control groups similar in all dimensions, the estimated parameters may be biased due to non-random sample attrition. To adjust for that, we use the inverse-probability weighting method suggested by Wooldridge (2010). Specifically, we ran a probit regression to compute the predicted probability of non-attrition at the plot level and used the inverse of this as weights in the main equation. This first-stage probit regression result is presented in Appendix 1.

3.3.2 Estimation Results

Table 3.3 shows the estimation results for the dynamic impacts of management training on rice productivity and profitability with coefficients on control variables suppressed for the sake of brevity. Note that the sample size here is 353, smaller by 8 from the sum of 193 (midline) and 168 (endline) observations due to missing explanatory variables in these few plots.

It is clear that training had positive and significant impacts on rice productivity by the midline, with the rice yield increasing by 0.75 ton/ha, the gross output value per hectare by 140 thousand CFAF, and rice income per hectare by 103 thousand CFAF. These improvements correspond to 20%, 24%, and 29% of the control means, respectively, suggesting that management training was effective in our context.[14] This improvement in productivity, however, did not lead to an increase in profits. As we will see, this might be because trained farmers test a larger number of improved management practices than control farmers, which require more family labor inputs. Qualitatively similar results were observed for TOT estimates. The fact that we

[13] Due to the small number of clusters, we also used unadjusted standard errors, assuming no serial correlations and heteroscedasticity. Statistical inference remains robust.

[14] According to experienced agricultural experts, impacts of recommended management practices on rice productivity are generally larger when there is sufficient water. Thus, our estimates could be considered the lower bound of the impacts that would be realized in a year with normal rainfall.

Table 3.3 Estimated results on the dynamic impacts of training: rice productivity

	Rice yield (ton/ha) (1)	Gross output value (000 CFAF/ha) (2)	Rice income (000 CFAF/ha) (3)	Rice profits (000 CFAF/ha) (4)
ITT				
Treatment (=1)	0.748* (0.335)	140.105** (51.348)	102.768* (45.640)	−8.362 (50.222)
×endline	−0.642* (0.279)	−126.704** (44.488)	−52.889 (61.507)	69.781 (70.156)
Endline (=1)	0.179 (0.231)	50.584 (47.283)	−48.217 (53.346)	−106.857** (42.896)
Wald test (Ho: total effect of treatment and its interaction is zero)	0.56	0.26	0.89	2.18
R-squared	0.465	0.418	0.678	0.709
TOT				
Attendance rate (instrumented)	1.453** (0.629)	270.438*** (94.168)	203.150** (81.749)	−15.249 (85.727)
×endline (instrumented)	−1.254** (0.527)	−245.705*** (80.283)	−114.245 (96.329)	123.358 (116.879)
Endline (=1)	0.187 (0.209)	51.485 (43.335)	−47.329 (48.608)	−107.506*** (39.799)
Wald test (Ho: total effect of treatment and its interaction is zero)	0.78	0.37	1.13	2.71*
R-squared	0.462	0.416	0.681	0.710

Sample size is 353. Clustered standard errors at the production site level in parentheses
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$
Note Control variables included but not reported here were Year dummy, household size, head's characteristics (age and its square, years of education and the male dummy), plot characteristics (cultivation size, tenure dummies for owner, leaseholder, sharecroppers), log household asset value, and local fixed effect. Attendance rate was instrumented by the treatment dummy, while attendance × endline was instrumented by treatment × endline dummy

found quantitatively larger magnitudes of impacts in TOT than in the ITT estimates suggests that actual training participation rather than simple eligibility is important in the improvement of production performance.

Notably, the coefficient estimates on the interaction term were negative and significant for rice yield and gross output value per hectare. This indicates that the improvement of performance among treatment groups from the mid- to endline was lower than for the control groups. The Wald test shows that we cannot reject the null hypothesis that the total training effect is zero in most specifications, implying that treated farmers are no better than control farmers by the endline. We could interpret this negative interaction term, β_3, as reflecting either the short-lived training effects or the existence of spillover effects. If training impacts do not last long, however, we would have observed some signals, such as a declining adoption rate of improved management practices among treated farmers from the mid- to endline surveys. We did not observe clear dis-adoption patterns among the data presented in Table 3.2. Thus, this finding seems consistent with the operation of a mechanism wherein control farmers improve their performance by learning from treated farmers after the SUTVA is relaxed.

Table 3.4, which shows the estimated impacts of training on the adoption of improved agronomic practices, also provides supportive evidence for the existence of spillovers. When the information exchange between treated and control farmers was restricted during the year after the training, a positive training impact on the adoption of improved management practices, such as leveling, canal/drainage construction/repairing, and straight-row transplanting is observed among treated farmers (ITT estimate) and training participants (TOT estimate). These results are in line with the expectation drawn from Table 3.2 that leveling and straight-row transplanting have a relatively large opportunity to assist improvement.

On the other hand, we did not observe the same positive effects on fertilizer use and seed selection, presumably because there is little room for improvement due to the high initial adoption rates of those practices at the baseline. Also, given that the same amount of fertilizer was provided to both treated and control groups in the experimental phase, and that it is easy for farmers to imitate seed selection techniques, it seems reasonable to observe negligible effects on these practices.

Once the restriction was lifted two years after the training, control farmers caught up with treated farmers in the adoption of recommended practices, as reflected in the negative and significant coefficients on the interaction term, β_3. The Wald tests also revealed that in most outcomes we failed to reject the hypothesis of zero training impact in the longer term.

Note also that, if spillovers exist, the average performance of control groups would improve over time, which should be reflected in β_1 (the endline dummy) > 0. β_1 is positive for most outcome variables and statistically significant for the adoption of canal/drainage construction/repairing and straight-row planting for TOT estimation, further supporting our interpretation in favor of the existence of spillovers.[15]

Taken together, we confirm that, after removing selection bias using the randomized experiment, training has positive impacts in the short term not only on the adoption of improved rice management practices, but also on rice productivity. Spillovers do not exist or at least do not matter much to completely cancel out positive training impact in the initial phase of the experiment. Our further intervention encouraging farmers to spread information, however, improved control farmers'

[15] A similar explanation can be offered for outcomes in Table 3.3, although we could not find any statistically positive and significant effects there, presumably because of other time-fixed confounders.

Table 3.4 Estimated results on the dynamic impacts of training: agronomic practice

	Fertilzer (kg/ha) (1)	Seed Selection (2)	Levelling (3)	Canal/drainage (4)	Straight-row planting (5)
ITT					
Treatment (=1)	24.736 (17.358)	−0.033 (0.031)	0.178*** (0.050)	0.119* (0.062)	0.218** (0.067)
× endline	−27.566 (24.593)	0.041** (0.017)	−0.202 (0.134)	−0.236** (0.089)	−0.227* (0.099)
Endline (=1)	0.396 (11.579)	0.002 (0.016)	0.144 (0.104)	0.176 (0.113)	0.162 (0.096)
Wald test (Ho: total effect of treatment and its interaction is zero)	0.03	0.07	0.06	2.28	0.02
R-squared	0.416	0.133	0.313	0.427	0.627
TOT					
Attendance rate (instrumented)	47.437 (30.788)	−0.063 (0.056)	0.340*** (0.084)	0.227* (0.118)	0.417*** (0.095)
× endline (instrumented)	−52.111 (41.586)	0.076*** (0.030)	−0.380* (0.215)	−0.432*** (0.154)	−0.430*** (0.155)
Endline (=1)	0.273 (10.307)	0.002 (0.016)	0.143 (0.095)	0.177* (0.102)	0.161* (0.086)
Wald test (Ho: total effect of treatment and its interaction is zero)	0.04	0.08	0.06	2.81*	0.02
R-squared	0.415	0.129	0.295	0.418	0.628

Sample size is 353. Clustered standard errors at the production site level in parentheses
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$
Note Control variables included but not reported here were Year dummy, household size, head's characteristics (age and its square, years of education, and the male dummy), plot characteristics (cultivation size, tenure dummies for owner, leaseholder, sharecroppers), log household asset value, and local fixed effect. Attendance rate was instrumented by the treatment dummy, while attendance endline was instrumented by treatment × endline dummy

practices and contributed to helping them catch up with treated farmers, presumably through spillovers.[16]

[16] Although the number of our outcome variables is not so large, one may wonder if we find false positives because we are testing multiple hypotheses. To address this concern, we computed false discovery rate sharpened q-values corrected multiple testing, following the Benjamini-Kreieger-Yekutieli method (Benjamini et al. 2006). All outcome variables that show statistically significant effects in Tables 3.6 and 3.7 remained significant at 10% or lower.

3.4 Spillover Effects

3.4.1 Information Network Analysis

3.4.1.1 Learning Link Data

Having outlined the treatment effects across time, we now examine whether social networks actually mediate information spillovers from treated to control farmers, using the detailed learning link data.

An empirical challenge on this topic is how to correctly specify social networks. Asking respondents about their social networks by arbitrarily setting a cap on the number of links may result in truncation bias, while asking an open-ended question tends to capture only the strong links, ignoring the weaker ones (see, for example, Maertens and Barrett (2013) for a thorough discussion of potential bias in empirically eliciting the true social network structure). To address this concern, we exploited a "random matching within sample" technique to elicit social networks, following, among others, Conley and Udry (2010), Maertens and Barrett (2013), and Mekonnen et al. (2018). More specifically, we matched each sample respondent with six other survey respondents randomly drawn from the sample at the same production sites and asked for details of the (non)existence of information exchange about agronomic practices between samples of farmers. We considered that a learning link between a respondent farmer i and a matched farmer j is established if i has *ever* asked j for advice at some time before interviews. This includes learning even before the training. We did not limit our interviews to only the post-training period, because respondents who violated the no-information-sharing rule, but were eager to satisfy the researchers' expectations, would likely manipulate their answers if they were asked about their behavior only after our intervention. If the reference period includes the non-intervention phase, they are more likely to freely provide the actual answer.[17]

To examine the differential roles played by treatment and control peers, we selected three matches from treated farmers and another three from control farmers. To capture changes in the network of interactions over time, we collected the learning link data in both the mid- and endline surveys.

Table 3.5 presents summary statistics of the learning link data for each year, separately for the probability of sample farmers knowing their match and the probability of sample farmers asking for agricultural information on the match, conditional on the

[17] One potential concern from this exercise is that respondents' self-reporting about information exchange patterns may be biased, reflecting their reluctance to tell the truth. Although we could not directly address such concerns, we reduced potential bias by asking about their past experiences rather than those of just the preceding year. We also observed that some cooperatives voluntarily created their own rules to keep control farmers from learning the management practices taught in the training program during the first year. Given that our experiments were executed by close collaboration with farmers to better understand suitable rice management practices in their contexts, we expected that such reporting errors may not be so serious in our study.

Table 3.5 Summary statistics of network data

	Year2 (mid-line)		Year3 (end-line)	
	Pr(Know family)	Pr(Ever ask agricultural advice | Know family)	Pr(Know family)	Pr(Ever ask agricultural advice | Know family)
Both control [Control, Control]	0.761 (0.427)	0.858 (0.349)	0.840 (0.367)	0.923 (0.268)
Both treat [Treat, Treat]	0.723 (0.448)	0.897 (0.304)	0.819 (0.385)	0.947 (0.224)
Own treat, pair control [Treat, Control]	0.769 (0.422)	0.874 (0.332)	0.821 (0.384)	0.975 (0.157)
Own control, pair treat [Control, Treat]	0.696 (0.460)	0.793 (0.406)	0.838 (0.369)	0.955 (0.208)
Number of observations	1664	1015	1607	1081

Standard deviations in parentheses

former knowing the latter. Because of the attrition of own and paired sample households, we had a total of 1,664 and 1,607 observations in the mid- and endline surveys, respectively. These samples included farmers who do not have rice production data in the baseline.

Conditional on sample farmers knowing their matches, about 80 to 90% of farmers asked their match for advice on agronomic practice, such as land preparation, transplanting, and fertilizer application in the midline. These results look high at first glance but seem to be reasonable because they reflect the probability that a sample farmer has *ever* asked matched farmers for agricultural advice. Thus, it may be more useful to focus on the differential probability of information exchange rather than the absolute level.

Compared with Control-Control pairs, Control-Treatment pairs are less likely to ask for advice in the midline, but more likely to do so in the endline. All other pairs have similar trends: they are more eager to ask advice by the endline surveys.

3.4.1.2 Dyadic Regression

We then ran a dyadic regression for those who know their matches to characterize the flow of information about management practices between farmers over time.[18] Formally, let L_{ijt} be equal to one if a respondent farmer i has ever asked a farmer j (conditional on i knows j) for advice by time t. We explore the correlates of learning links by including the attributes of a household i and j as:

[18] Using the full sample, including nonacquaintance pairs, did not alter our main findings.

$$\begin{aligned}L_{ijt} = &\delta + \gamma T_{\mathrm{t}} + \alpha_1 D_{ij}^1 + \alpha_2 D_{ij}^2 + \alpha_3 D_{ij}^3 \\ &+\beta_1\left(D_{ij}^1 \times \mathrm{T_t}\right) + \beta_2(D_{ij}^2 \times \mathrm{T_t}) + \beta_3\left(D_{ij}^3 \times \mathrm{T_t}\right) \\ &+ \left(X_i + X_j\right)\rho + \left(X_i - X_j\right)\tau + W_{ij}\pi + \varphi + u_{ijt}\end{aligned} \tag{3.2}$$

where D_{ij}^1, D_{ij}^2, and D_{ij}^3 are a combination of the treatment status of households i and j with [treated, treated], [treated, control], and [control, treated]. The remaining combination [control, control] is a reference group; T is a binary indicator for the endline survey; X_i and X_j denote a vector of baseline controls for farmers i and j characteristics, respectively[19]; W_{ij} describes a dummy equal to one if the gender of both farmers is the same; φ is the production site fixed effect; and u_{ijt} is a random disturbance. Following Attanasio et al. (2012) and Takahashi et al. (2019a, b), standard errors are clustered at the production site level to allow for possible correlations not only within dyadic pairs but also across all dyads in the same location.

Table 3.6 presents estimated results using a linear probability model. The coefficient estimate on the [treated, treated] dummy, α_1, is positive and statistically significant, but its interaction term with the endline data dummy, β_1, is statistically insignificant. This indicates that information exchange between treated farmers is more active than between control counterparts at the same production site, and this tendency does not systematically change over time. It might be that treated farmers are more likely to exchange information with each other to reinforce agricultural skills taught in the training, although we cannot completely deny the possibility that there was some baseline imbalance in the randomized matching process.

On the other hand, and consistent with our expectations, we observed a negative and significant coefficient on the [control, treated] dummy, α_3, and a positive and significant coefficient on the interaction term with the endline data dummy, β_3. The results suggest that controlled farmers refrained from asking agricultural advice from treated farmers or the latter refrained from disclosing management information to the former in the first year after training, but that they were eager and active in doing so in the second year after training. This indicates that impact evaluation in the initial phase was less likely to be undermined by spillovers, supporting our claim that the recommended practices are more productive. It also supports our main finding that there were information spillovers after the relaxation of the SUTVA in the two years after training, which would facilitate control farmers to improve their rice management practices and performance through social learning.

The insignificant effects of the [treated, control] dummy, α_2, and its interaction with the endline dummy, β_2, are also broadly consistent with our expectations because treatment farmers may have no more incentive to ask control farmers for advice than control farmers do. Considering that our network data were intended to capture the one directional flow of information, non-symmetric results of α_2 and α_3 seem reasonable.

[19] If L_{ij} is bidirectional (i.e., $L_{ij} = L_{ji}$), $\beta X_{ij} = \beta X_{ji}$ should be imposed: In such a case, $|X_i - X_j|$ instead of $\left(X_i - X_j\right)$ is more relevant as regressors (Fafchamps and Gubert 2007).

Table 3.6 Estimated results on the dyadic regression

	Ask agriculture advice = 1
Both treat [Treat, Treat]	0.045* (0.020)
×endline	−0.018 (0.031)
Own treat, pair control [Treat, Control]	0.019 (0.030)
×endline	0.036 (0.040)
Own control, pair treat [Control, Treat]	−0.063** (0.027)
×endline	0.091** (0.033)
N	2096
R-squared	0.063

Clustered standard errors at the production site level are in parentheses
*** p < 0.01, ** p < 0.05, * p < 0.1
Note Control variables included but not reported here were: The sum and differences of household size, heads' age, head's years of education, cultivation land size, asset values, a dummy equal to one if the household heads are same gender, and local fixed effects

3.4.2 *Extension to the Linear-In-Mean Model*

While our analysis so far supports the existence of social learning, one may wonder if social learning actually plays a significant role. If this were so, we might observe the influences of peer behavior and performance on one's own behavior. As a final robustness check to verify this possibility, we employed an extended linear-in-mean model.

We restricted the observation of this analysis to the endline year as this reflects the normal condition without the prohibition of information exchange in which spillovers are more likely to take place. We also restricted the outcome variables to rice yield, gross output value per hectare, the adoption of field leveling, canal/drainage construction/repairing, and straight-row planting, for which strong information spillovers from treated to control farmers seem to exist as observed in Tables 3.3 and 3.4.

To disentangle social effects from other confounders, we modify Eq. (1) as follows:

$$Y_i = \gamma_0 + X_{i0}\delta_1 + \overline{X}^N_{-i0}\delta_2 + \gamma_1 Y_{i0} + \gamma_2 \overline{Y}^N_{-i,t-1} + \text{Networ}\,k_i + \frac{\#\text{Treatment}^N_i}{\#\text{Network}_i} + \mu + \epsilon_i \tag{3.3}$$

where $\overline{X}_{-i0}^{N}$ denotes the average values of baseline observable characteristics in i's information network excluding i's own value, regardless of whether network peers are treated or control farmers. For simplicity, we omit the subscript j to denote a production site. As in the previous sub-section, we define network peers as those persons i had asked for agricultural advice by the time of the end-survey. $\overline{X}_{-i0}^{N}$ are then computed using the baseline data for each network peer's characteristics. This, along with the production site fixed effects, μ, serves to control for environmental and institutional factors that lead farmers to behave in a similar fashion[20]; $\overline{Y}_{-i,t-1}^{N}$ is the average productivity or technology adoption in i's network at the midline, regardless of their treatment status. This allows us to explore whether their peers' average behavior and performance directly affect farmer i's performance. Following Mekonnen et al. (2018), we use lagged rather than contemporaneous values of mean group performance or behavior in the recognition that information on agricultural technology cannot be diffused quickly; and $Network_i$ is the network size (i.e., max six), while $\frac{\#Treatment_i^N}{\#Network_i}$ is the share of treated farmers in i's information network. We expect the latter to capture peer effects, especially those mediated by treated farmers. This is akin to the methodology used by Kremer and Miguel (2007) and Oster and Thornton (2012). The original intuition behind this method is that once we control for network size (which could be potentially endogenous),[21] the share of network peers in the treatment group is random because of the randomized experiment. This exogenous variation can be then used to identify peer effects.

Note that the average peer performance and the share of treated farmers in i's network are expected to reflect different channels of peer effects; the former may partly capture learning by direct observation even without mouth-to-mouth communication, while the latter may partly capture knowledge transmission from treated farmers even when treated farmers do not actually adopt new technologies.

The estimated results in Table 3.7 show that the share of treated farmers significantly and positively affected their own behavior and performance for gross output value per hectare, leveling, and straight-row planting. We also observed a positive effect on the average performance of network members in most specifications, although this was statistically insignificant except for leveling, perhaps due partly to low statistical power and partly to difficulties in mimicking new technologies without learning through deep communication. While these results should be interpreted with caution to avoid strong causal inference, it seems to be no exaggeration to argue that the results provide further suggestive evidence on the existence of spillovers, especially mediated through treated farmers.[22]

[20] As discussed by Manski (1993), impact of social network is generally difficult to identify due to reflection problems. Our estimation method attempts to overcome these problems.

[21] In the random matching within sample method, network size should not be interpreted literally, but rather as a proxy for one's social connectedness where the more random matches a household has, the larger will be their true social network (Murendo et al. 2018).

[22] Although we attempted to minimize concerns about spurious correlation, we are aware of a potential endogeneity issue in this exercise. For example, control farmers who are more motivated, if all else is held constant, may be more willing to establish information links with treated farmers

Table 3.7 Estimated results on the linear-in-mean model

	Rice yield (ton/ha) (1)	Gross output value (000 CFAF/ha) (2)	Levelling (4)	Canal/drainage (5)	Sraight-row planting (3)
The average outcome value (lagged) in network	0.279 (0.197)	0.206 (0.155)	0.273* (0.133)	−0.085 (0.116)	0.079 (0.070)
Network size	−0.107 (0.129)	−12.155 (20.075)	−0.024 (0.017)	0.027* (0.012)	0.025 (0.021)
Share of treatment in network	2.426 (1.312)	494.419* (207.006)	0.537** (0.202)	0.256 (0.224)	0.586** (0.217)
R-squared	0.448	0.386	0.388	0.503	0.737

Sample size is 144. Clustered standard errors at the production site level are in parentheses
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$
Note Control variables included but not reported here were: baseline respondent's and average values in respondent's network of household size, head's characteristics (age and its square, years of education and the male dummy), plot characteristics (cultivation size, tenure dummies for owner, leaseholder, sharecroppers), log household asset value, and local fixed effects, as well as the treatment dummy for the respondent

3.5 Discussion

Before concluding this chapter, we must note the several caveats in our study. First, the performance of the rice-growing farmers in our study was better than we expected, due presumably to past training provided by local governmental and international organizations such as AfricaRice. Therefore, many recommended practices were known and practiced by sample farmers even before the training program, except for straight-row transplanting and, to a lesser extent, leveling. This limited our scope, since we could not explore the variations and magnitudes of the spillover effects of different cultivation practices when they are introduced to "virgin land."

Second, while rice production is sensitive to weather conditions, especially rainfall, there was a significant lack of rainfall during the growing seasons in the midline and endline surveys, which resulted in many farmers halting their rice production during our observation periods. Weather conditions cannot be controlled, so we made attempts to mitigate potential estimation bias. Nevertheless, the conclusions may have been more solid and credible if the experiments had been conducted in more ideal settings.

Third, we proposed a new experimental design to implement rigorous impact evaluation and the promotion of spillovers in a unified framework. To achieve the

who know the new technique or with peers who actually adopt it. Given the possibility that such interventions can alter the underlying network structure (Advani and Malde 2018), we admit that our constructed variables to capture social learning effects may not be free from endogeneity concerns. Most likely, if anything, our results would underestimate the true effects.

same dual objectives, many existing studies use two-step randomization, in which they randomly select treated and control villages first, and then treat and control individuals within treated villages, allowing spillovers within the treated village. However, this type of cluster-level randomization is often costly, since it requires a larger sample size than individual-level randomization to have sufficient statistical power. We added to the literature by showing an alternative, less costly approach in collaboration with farmers.

One may argue that our approach, especially the restriction of information exchange between farmers, may pose an ethical concern if the training impacts are a priori known to be positive. Although our expectation of training impact was positive as recommended rice practices were mostly established in experimental stations and several countries in SSA have successfully improved rice productivity (Otsuka and Larson 2013, 2016), we were not sure whether that was the case at our study sites. As a result, we felt it was important to evaluate the training impact through an RCT because it is common to observe differences between on-farm and on-station results as well as across countries.

We also wondered whether management practices taught in our training were ineffective for those who had already received similar training in the past or those whose productivity was already close to the production possibility frontier. Thus, we believe that our approach did not conflict with the "do no harm" principle. Rather, once positive impacts were confirmed, we encouraged information exchange between treated and control farmers. By doing so, we were able to successfully reduce any inequalities between treated and control farmers generated by our experiment. It might be valuable to build in this kind of mechanism in other models to allow control groups to catch up with treated ones, which is often overlooked in existing RCTs.

Finally, while we carefully executed an RCT to establish internal validity, we are not fully confident of the external validity of our method. First, our sample farmers were extremely collaborative, which may not always be the case. Second, we provided control farmers with improved seeds, fertilizer, and herbicide on credit. We took this approach because we wanted to isolate the impact of management training from the use of current inputs and also enhance the cooperation of control farmers. But if the provision of credit induces farmers to be more keen to learn technologies, or there is complementarity between current inputs and management practices (e.g., Ragasa and Mazunda 2018), our findings may not be reproduceable in other experiments or scaled-up implementation without this input provision.

3.6 Conclusions

This chapter executed an RCT to examine whether rice production management training has positive impacts on the adoption of recommended practices and productivity in the short term as well as whether social learning can be effective for the wider diffusion of recommended practices by facilitating information spillovers from treated to control farmers in the subsequent period. By using a random assignment of farmer's eligibility for training participation we attempted to reduce selection bias in impact evaluation. Also, by asking farmers not to exchange information in the initial phase of impact evaluation, we attempted to maintain the SUTVA.

We found positive and significant short-term effects from this training, which widen the gap in yield by 20%, the gross output value per hectare by 24%, and the adoption rates of selected rice management practices between treated and control farmers. However, after the restriction on information exchange was removed, control farmers improved their performance significantly, and, as a result, the gap between treatment and control-group farmers becomes virtually zero in the longer term. This suggests that information dissemination by farmers can be as effective in improving practices as the initial training provided by extension services. Although the generalizability of our findings may be questioned, Nakano et al. (2018a, b) found similar results in Tanzania. Our detailed analysis of learning link data and peer effects provides further supportive evidence for the existence of information and technology spillovers.

Our experiment relies on random assignments of management training without any monetary incentive scheme. For future research, it seems to be vital to inquire in different contexts how to best select treated nodes and whether monetary or other incentives should be given to them to maximize social benefit as well as to examine the external validity of our research findings (Kondylis et al. 2017; Maertens 2017; Barrett et al. 2018; Beaman and Dillon 2018; BenYishay and Mobarak 2018; Shikuku 2019).

Appendix 1: Estimation Results for the Non-attrition Probit Model

	Year 2 (1)	Year 3 (2)
Head's age (years)	−0.015	0.024
	(0.043)	(0.035)
Head's age squared (years)	0.000	−0.000
	(0.000)	(0.000)
Head's educatio n (years)	0.037	0.021
	(0.023)	(0.022)
Head is male (=1)	0.570**	−0.177
	(0.283)	(0.271)
HH size	−0.011	−0.011
	(0.021)	(0.021)
Plot size (ha)	0.116	0.641**
	(0.217)	(0.270)

(continued)

(continued)

	Year 2 (1)	Year 3 (2)
Owner (=1)	−0.078	0.191
	(0.376)	(0.381)
Leaseholder (=1)	−0.283	0.314
	(0.412)	(0.410)
Log asset value	0.194***	0.081
	(0.068)	(0.067)
Constant	−0.633	−2.175**
	(1.142)	(1.042)
Production site fixed effects	Yes	Yes

Sample size is 328. Standard errors in parentheses
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

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