Subsidized Input and Technical Assistance for the Adoption of Sustainable Agriculture

Experimental Evidence from arid Northern Ghana*

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Abstract

Sustainable land management practices (SLMP) are expected to mitigate land degradation and dwindling agricultural productivity. We implement an RCT to evaluate the effectiveness of a one-time government subsidy and technical assistance program on SLMP take-up and agricultural productivity. The program provides farmers with inputs, labor assistance, and consultation to farmers to overcome input, labor, and informational constraints to adoption, free of charge. We find that the program not only increased SLMP usage of subsidized farmers, but also of farmers whose application for the program was rejected and who then did not receive support. Our results suggest that alleviating informational constrains and the diffusion of these information through social learning from admitted to rejected farmers likely account for the positive impacts on SLMP uptake, while alleviating input and labor constraints have smaller impacts. Despite the increased uptake, the program failed to mitigate (perceived) soil erosion or to increase agricultural productivity in the first two years of the intervention.

Keywords: Input and technical assistance, land degradation, sustainable land management, soil conservation, technology adoption.
JEL Codes: Q16, Q24, Q56, Q28, O13.

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1 Introduction

Agricultural technology adoption by smallholder farmers in Sub-Saharan Africa is perceived as a key pre-requisite for mitigating or even reversing land degradation and its consequences for agricultural livelihoods (World Bank, 2008; Adamopoulos and Restuccia, 2022). Small farmers in the region cultivate their land with low input, capital, and knowledge intensity (Poulton *et al.*, 2010; Jayne *et al.*, 2014; Barbier and Hochard, 2018). Farmers shorten the fallow periods to meet the increasing demand for food, causing land degradation, soil erosion, and declining agricultural productivity (Wuepper *et al.*, 2020; Barbier and Di Falco, 2021). Sustainable land management practices (hereon SLMP), such as the construction of contour bunds to prevent water runoffs, are thought to be suitable for the arid conditions faced by farmers in the region to reduce and possibly reverse land and water degradation, improve agriculture productivity, and thus agricultural livelihoods.

However, akin to other agricultural innovations, the adoption of these sustainable agricultural practices by smallholder farmers in Africa remained low despite the increasing agronomic and on-the-field evidence on the profitability of their adoption (Pretty *et al.*, 2006; BenYishay and Mobarak, 2018; Aker and Jack, 2021; Adjognon *et al.*, 2022). There are three potential explanations for the low adoption rates that emerge from the literature. Farmers may not use these practices because they may lack information about the potential benefits from adoption, the implementation, or both (Hanna *et al.*, 2014; Kondylis *et al.*, 2017; BenYishay and Mobarak, 2018; Aker and Jack, 2021; Carter *et al.*, 2021). They may also lack access to inputs and tools to implement the practices (Suri, 2011; Arslan *et al.*, 2015; Aggarwal *et al.*, 2018; Porteous, 2020), face labor constraints in adoption (Jack, 2011; Fink *et al.*, 2020; Foster and Rosenzweig, 2022), or lack credit to finance the high ex-ante costs of adoption (Giné and Yang, 2009; Karlan *et al.*, 2014; Fink *et al.*, 2020).

In this paper we study a randomized control trial in arid Northern Ghana to evaluate the impact of a government input and technical assistance program on the adoption of SLMPs and agricultural livelihoods. The program was implemented by the government of Ghana to provide a one-time in-kind transfer to overcome the informational, input, and labor constraints to the adoption of SLMPs. As such, the program resembles other national input subsidy programs implemented throughout Sub-Saharan Africa. At the start of the program, government extension agents visited the treated communities and informed community members about the expected (environmental and agricultural) benefits and costs of SLMPs adoption and about the one-time support for SLMPs implementation that farmers in the program would receive. Farmers who were interested in adopting any of the promoted agricultural practices were then invited to submit an application to the program. The government admitted a randomly selected subset of applicants who received inputs for the adoption of SLMPs, labor assistance in the implementation process to reduce the labor cost, and the opportunity to consult with government extension agents about the SLMPs. There are thus three types of farmers in treated communities: admitted farmers who receive in-kind subsidies from the program, farmers whose application was rejected, and farmers who did not apply. Farmers in control communities continued to cultivate their land without any assistance in SLMPs adoption from the government. We evaluate the effect of the program in the first two years of the experiment.

We find that the technical assistance program increased the average number of sustainable agricultural practices used by farmers who applied for the program. Farmers who were admitted to the program increased the number of agricultural practices used by 0.7 practices, from 1.5 to 2.2 practices one year after the intervention. Compared to this large impact on the direct beneficiaries of the program, we also find a sizeable spillover effect to farmers whose application was rejected. Rejected farmers also increased their SLMPs usage from 1.5 to 1.9 practices in the first year of the program (i.e. by 0.4 practices). We also find that the increased uptake of SLMPs among farmers who applied to the program (i.e. admitted and rejected) persisted in the second year after the intervention, although the estimated impacts are smaller and imprecisely measured. We do not find positive spillover effects on SLMPs usage of farmers who were not interested in SLMPs adoption and did not apply for the intervention.

The program thus did not only enable farmers who received in-kind transfers from the government to increase SLMPs usage, but also farmers whose application for these transfers were rejected. This finding naturally raises the question what barriers of SLMPs adoption did the program alleviate such that farmers outside the program also benefited from it? We study this question by estimating the impact of the program on input use, the source of labor, and the difficulties in adoption reported by admitted and rejected farmers and by comparing these impacts between the type of farmers. The comparison shows that both admitted and rejected farmers increased input use, family labor, and labor provided by peers in the implementation of SLMPs, although the impacts on rejected farmers are imprecisely measured. Thus input and labor market constraints are unlikely to be the main barriers of SLMPs adoption, at least for a substantial share of farmers. This is quite surprising especially because the the practice that saw the largest increase in take-up is the construction of contour bunds, which is typically labor intensive to implement (Liniger *et al.*, 2011). Instead, we find evidence of the program alleviating informational barriers to technology adoption: admitted and rejected farmers reported adopting especially those agricultural practices for which they lacked information before the program. Combined with our finding that the program increased the share of farmers who implemented the SLMPs with the help of their peers, we conclude that the diffusion of information about the practices via social learning is partly responsible for the spillover effects of the program to rejected farmers' SLMPs adoption.

Despite the increase in SLMPs usage of farmers who applied for the intervention, we do not observe any improvement in agricultural productivity or in the value of agricultural production in the first two years of the program. We explore four potential explanations for this result. First, if not the adoption of a marginal SLMP but the adoption of a group of SLMPs improves agricultural productivity, then the relationship between the number of SLMPs adopted and productivity may be non-linear, and there may be a threshold number of practices adopted at which agricultural productivity improves. Second, a considerable share of farmers applied chemical fertilizers on their field in the absence of the program which may indicate that many of them already cultivated their land intensively. Increased SLMP usage may not improve agricultural productivity because intensive land cultivation with modern inputs may be a substitute to SLMPs in production. Third, there may be substantial heterogeneity between admitted and rejected farmers in the way the SLMPs were adopted, because only admitted farmers received guidance directly from government extension agents. Fourth, the SLMPs implemented by farmers may have been less effective in reducing water runoff and soil erosion than in pre-existing agronomic experiments. Our results reject the first three explanations and find support for the last one. We find that the program did not decrease the share of farmers who report flood and soil erosion to threaten their agricultural plots in the first two years of the program. In spite of this finding, we are yet to conclude that the practices promoted by the government are altogether ineffective, because the practices may take more than two years to become effective.

Our study relates to two strands of literature. First, it contributes to the literature on the efficiency of government-led subsidy programs in promoting agricultural technology adoption in Sub-Saharan Africa. Input subsidy programs have been widely implemented by governments in this region to reduce the agricultural productivity gap between Africa and the rest of the world, but experimental evidence on the effectiveness of such programs is still scarce (Jayne and Rashid, 2013; Jayne *et al.*, 2018; Magruder, 2018). Carter *et al.* (2021) find that a temporary government subsidy program in Mozambique increased the adoption of an improved seed variety not only by subsidized farmers but also by their peers in their social network. The program enabled subsidized farmers to experiment with the seeds and disseminate their experience to their peers. In contrast, Gignoux *et al.* (2022) find that a national input subsidy program in Haiti decreased the adoption rates of inputs because self-financing of inputs was crowded out by subsidies. Our finding of a positive direct and spillover effect on farmers' adoption rates supports the findings of Carter *et al.* (2021).

Second, our study closely relates to the literature on the barriers to agricultural technology adoption. The literature studied numerous constraints faced by farmers in agricultural technology adoption, most prominently credit and liquidity constraints (Karlan *et al.*, 2014; Fink *et al.*, 2020), imperfect insurance markets (Karlan *et al.*, 2014; Carter *et al.*, 2017; Casaburi and Willis, 2018), informational constraints (Kondylis *et al.*, 2017; BenYishay and Mobarak, 2018; Emerick and Dar, 2021; Aker and Jack, 2021), and limited market access (Suri, 2011; Aggarwal *et al.*, 2018; Porteous, 2020). However, in a recent review, Suri and Udry (2022) suggest that more than one binding constraints limit the diffusion of agricultural technologies in Africa and that interventions addressing multiple barriers to technology adoption may be the most effective. Our study provides evidence of a government-led policy intervention that was designed to overcome informational, input market, and labor market constraints to SLMP adoption.

The remainder of this paper is organized as follows. Section 2 provides an overview of the input and technical assistance intervention and the experimental design. Section 3 presents the survey sample and the main outcomes of the analysis. Section 4 describes the empirical strategy to identify the intention-to-treatment effect, the treatment effect on the treated, and the spillover effects. Section 5 discusses the impact of the technical assistance intervention on farmers' SLMP usage, while Section 6 presents the impact on farmers' agricultural productivity and production. Section 7 provides additional evidence on explaining the impact of the program on subsidized farmers' SLMPs adoption, the spillover effect, and the lack of impact on agricultural productivity. Section 8 concludes.

2 Experimental design

2.1 Program description and sustainable land management practices

Our field experiment is part of the Sustainable Land and Water Management Program (hereon SLWMP), a joint effort by the Government of Ghana and the World Bank to mitigate and possibly reverse land degradation. The project focuses on the northern part of Ghana where land degradation is driven by an increasing demand for food and the prevalence of unsustainable land management techniques among smallholder farmers (Wuepper *et al.*, 2020; Barbier and Di Falco, 2021). To meet the increasing demand for food, farmers shorten the fallow period, which reduces overall vegetation cover and causes soil erosion and declining agricultural productivity (López, 1997; Goldstein and Udry, 2008; Diao *et al.*, 2019).

To mitigate land degradation and dwindling agricultural productivity, the SLWMP program aimed to stimulate the adoption of twelve sustainable land management practices (hereon SLMP, see Table A.1 in Appendix A for the full list). These SLMPs were selected by the government because agronomists expected them to improve soil nutrient and water content, soil fertility and eventually agricultural productivity (typically after a few years; Liniger *et al.* (2011)). Practices promoted by the program include, for instance, "integrated nutrient management" that requires farmers to re-use organic materials (such as animal manure or composted crop residues) as organic fertilizer in the next agricultural cycle. Another practice in the program is the construction of contour bunds on agricultural plots that mitigate the impact of water runoff and soil erosion.

Existing empirical evidence suggests that the adoption of these SLMPs can improve agricultural productivity. BenYishay and Mobarak (2018) estimate that composting doubled agricultural yields in Malawi in the second year of implementation. Abdulai and Huffman (2014) estimate a 24% increase in yields due to the construction of contour bunds among rice producers in Ghana. Adjognon *et al.* (2022) find that the adoption of an additional SLMP increases agricultural production by 40% in Burkina Faso within one agricultural season. Pretty *et al.* (2006) implemented a meta-analysis and estimate an average increase of 79% in agricultural yields due to the adoption of SLMPs. Ali *et al.* (2020) show that the increased adoption of SLMPs improved soil water content and vegetation cover over the course over the period of 7 years in Ethiopia. Despite the potential of these practices to improve soil conservation and fertility, their adoption among smallholder farmers in Africa is far from widespread. Arslan *et al.*

(2015) show that the adoption rates of particular SLMPs range between 0 and 40% in Zimbabwe and Tesfaye *et al.* (2021) find similar adoption rates in rural Ethiopia. Our data show that the average farmer in Northern Ghana is aware of only 2.5 SLMPs and they use only 1.7 practices on their agricultural plots.

The government of Ghana perceived three key barriers for technology adoption limiting the diffusion of these technologies in the country: (i) a general lack of knowledge about the benefits and implementation of the practices, (ii) inability to purchase the necessary inputs and tools to adopt them due to input market imperfections, and (iii) the high labor cost of implementation (Conley and Udry, 2010; Jack, 2011; Suri, 2011; Hanna *et al.*, 2014; Arslan *et al.*, 2015; Kondylis *et al.*, 2017; Aggarwal *et al.*, 2018). To overcome these barriers of SLMP adoption, the SLWMP program provides input and technical assistance to smallholder farmers. Although the program was launched already in 2011, the government allowed us to evaluate the effect of technical assistance only in 2016 when the program expanded in three regions of Northern Ghana. We evaluate the effects of the intervention in the first two years of the program extension.[1]

2.2 Intervention

The government's program aimed to overcome the three barriers of SLMPs adoption by providing farmers unconditional in-kind transfers (input and assistance). At the beginning of the intervention government extension workers visited the villages selected for the intervention and invited all farmers in the village to an information session about the program. During these information sessions, extension agents informed farmers about the environmental and agricultural importance of sustainable land management and introduced the SLMPs promoted by the government to farmers: the expected benefits, the necessary inputs, and the main steps of the implementation for each SLMPs. Extension agents also informed farmers that those enrolled in the program would receive inputs and direct assistance to implement SLMPs and that the program would provide these only in the current agricultural season but not in subsequent seasons. Farmers then decided whether to apply for the intervention and if so, which practices would they like to receive inputs and assistance for.

Once the government received farmers' applications and (randomly) admitted farmers to

[1]The government only implemented the program in 46 communities when the program was launched (MoFA, 2016, p.61), which is a small subset of the population of communities in this part of the country. We thus do not expect that the first phase of the program affected the villages enrolled in 2016 via anticipation or general equilibrium effects.

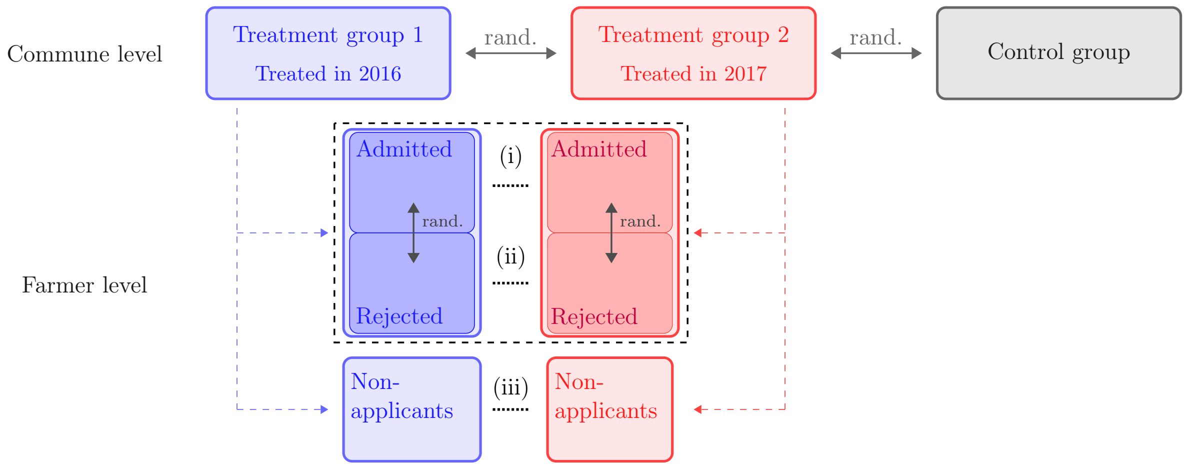
the program, the government then provided inputs, tools, and direct assistance to farmers to implement the selected agricultural practices, free of charge. For instance to implement agroforestry, farmers would receive saplings to plant and tools to keep them alive. If farmers chose composting as a means to manage soil nutrients, the government provided building materials and helped farmers to construct composting pits. To construct contour bunds, farmers received tools to measure the contour lines and to build the bunds along these lines, and assistance by the government in the construction to reduce labor costs. In addition to receiving inputs, tools, and assistance in implementation, farmers also had the opportunity to consult with government extension agents on how to implement and use the agricultural practices. Farmers were free to disseminate the knowledge they acquired and they were not prohibited from sharing the tools and inputs with their peers.

The intervention is thus a combination of input, assistance in implementation, and information provision to address different barriers to SLMPs adoption. Supplying farmers with inputs (such as seeds, seedlings, fertilizers etc.) and tools enables them to adopt the practices if lacking access to these inputs in markets is a major constraint of adoption. Direct assistance to farmers in implementing the practices reduces the labor costs of SLMPs implementation and alleviates labor market constraints. The last component of the intervention addresses information barriers as farmers may learn about the expected benefits of each SLMPs promoted by the government from the information session and also obtain knowledge about the implementation of the practices from the consultation with extension workers. Altogether the technical assistance program can be regarded as an unconditional compensation scheme for soil and water conservation because farmers received in-kind incentives for technology adoption upfront, independent of farmers' effort or result in improving environmental or agricultural outcomes.

2.3 Treatment assignment and randomization

To evaluate the effects of the technical assistance program, the intervention was randomly assigned first at the community and then at the individual level (see Figure 1). At the first level, 75 communities in three regions of Northern Ghana were randomly allocated to one of three groups stratified by district (a sub-national administrative unit below the regional level). These communities were selected by the government because the communities had not received any support prior to the experiment and the government was willing to randomize the implementation of the technical assistance intervention in these communities. Of these communities, the research

Figure 1: Experimental design.

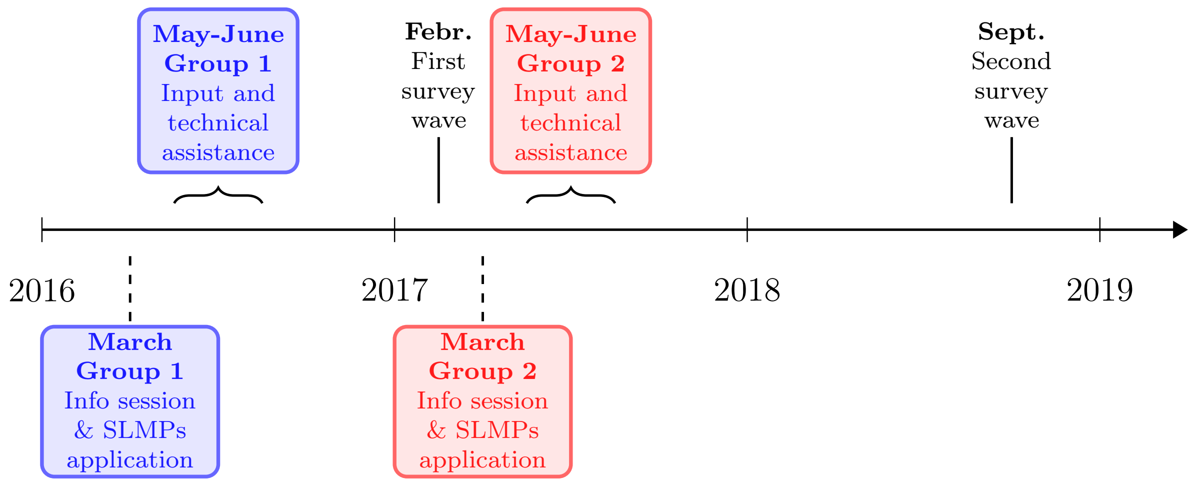


Note: Of the 75 communities, we randomly assigned 22 to receive the intervention in 2016 and 24 to receive it in 2017. The remaining 29 communities form the control group. Within treated communities, farmers who were interested in participating in the input and technical assistance intervention applied to program at the beginning of the intervention. Then the final set of program participants was determined by randomized rejection of the applicants.

team randomly assigned 22 communities to receive the intervention in 2016 (hereon "Group 1"), 24 communities to receive the intervention in 2017 (hereon "Group 2"), and the remaining 29 communities to never receive the intervention and form the control group. As a result of this community treatment assignment, the phase-in of the intervention in the 46 treated communities was determined by randomization (see Figure 2 for the timing of the program implementation in Group 1 and 2 communities). Communities in Group 2 only learned about receiving the intervention in the second year of the experiment when they were scheduled to receive it.

At the second level of treatment assignment, farmers were enrolled to the intervention follow-

Figure 2: Timeline of the study.



ing an oversubscription method (Duflo *et al.*, 2007). In treated communities, farmers interested in the program submitted an application for the SLMPs they intended to adopt. Farmers in Group 1 did so in early 2016, while farmers in Group 2 applied a year later in 2017 (see Figure 2). Due to the large number of applications in treated communities and a limited budget, the government randomized which of the applicant farmers to reject or to admit into the program. This was announced to all interested farmers when they were invited to apply for the intervention. There are thus three farmer categories in the treated communities after the farmer selection process (see Figure 1): farmers who did not apply for the intervention (hereon "non-applicant farmers"), rejected applicants, and admitted applicants. Rejected and admitted applicants are comparable to each other because of randomized admission among applicants, but we do not expect the same to hold for applicant and non-applicant farmers as the application for the intervention was voluntary.

Our experimental design allows us to identify three effects. First, we can identify the intention-to-treat effect of the program on treated communities. We can estimate this effect by comparing the outcomes of the treated communities to those of the control communities (see the top of Figure 1). This intention-to-treat approach yields the effect of the program over the whole population of the village taking into account the share of accepted, rejected, and non-applicant farmers within the communities. The estimated impact thus takes into account that (i) not all farmers were interested in participating in the program, (ii) that the program could not enroll all interested farmers in the treated villages, and (iii) that there are potential spillover effects of the program on farmers who were not part of the program.

Second, we can estimate the direct effect of the intervention on farmers who were admitted to the program. To do so, we exploit the fact that treated communities were randomly assigned to receive the intervention either in 2016 or 2017, and that the government implemented the same enrollment process into the program in both years. As a result, we can use the pre-intervention outcomes of admitted farmers in Group 2 communities to estimate the counterfactual outcomes of admitted farmers in Group 1 communities. We can do so because in the first year of the experiment (in early 2017), farmers accepted into the intervention in Group 1 had participated in the program for a year, while farmers who were yet to be admitted to the program in Group 2 had not received the invitation to apply yet (see Figure 2). Farmers admitted into the program received information about the practices (during the information sessions and the consultation with government extension agents), inputs and tools to implemented the practices, and assistance in implementing the practices (reducing the cost of implementation) from the program. Thus this comparison yields the overall effect of the intervention on admitted farmers, that is the treatment effect on the treated. If admitted farmers used these transfers to implement at least one of the selected practices and used the practices in the first year of the intervention, then we would expect the treatment effect on admitted farmers' SLMPs to be at least 1.

Third, this design also allows us to test for spillover effects of the intervention on rejected and non-applicant farmers as well. Rejected farmers did not directly benefit from input or technical assistance provided by the program. However, they could have also benefited indirectly from the intervention: rejected farmers may have learned about the implementation of the practices from their peers in the program, received inputs from the same peers, or received help from peers in the implementation of SLMPs on their own land. Since rejected farmers in Group 1 are also comparable to rejected farmers in Group 2 on unobservable characteristics (see dotted line (ii) in Figure 1), we can test whether there are spillover effects to SLMP adoption by farmers who were interested in SLMPs adoption but were not admitted to the program. Likewise, we can test the presence of spillover effects to the SLMPs adoption of non-applicant farmers in treated communities (see dotted line (iii) in Figure 1).

3 Data collection and descriptive statistics

We collected two rounds of survey data on 750 farming households from the 75 communities in the experiment. We implemented the first survey one year after the beginning of the program (in February 2017) and the second survey two years into the program (in September 2018; see Figure 2). We used the same survey instrument across the two survey waves and collected information on household demographics, agricultural production, and SLMPs usage in the last agricultural season.[2] In each of the 75 communities, we planned to follow the same 10 households over time (in total 750 households) in both survey waves.

To ensure that we surveyed households with different program status, we implemented a stratified sampling frame in Group 1 communities. In each of these communities, we collected a sample of ten households: six households that had been randomly accepted to the intervention, two that had been rejected, and two that had not applied. In each of the three categories, households were randomly selected into the survey. In the Group 2 communities we interviewed

[2] Although the 2018 agricultural season has started by the time we collected the second survey wave, the survey asked farmers about the preceding 2017 agricultural season.

ten randomly sampled households because we did not observe farmers' program participation before 2017 and because we aimed to observe the same households across the two surveys. We surveyed a random sample of ten households in control communities as well because farmers did not have the opportunity to apply for the intervention. To account for the overall sampling strategy in our community level comparisons, we assign sampling weights to the three farmer categories in each community such that the weight-adjusted share of each farmer group in the sample match the share of that group in the community population. We do so by taking the share of household in a farmer group in each community within the sample, divide it by the share of the same group in the population of the community, and then use the inverse of this ratio as probability weight in our analyses. Weighted community averages are then representative of the communities.

We evaluate the impact of the program on three groups of outcomes. Our main outcome is farmers' SLMPs usage which is captured by the self-reported number of SLMPs used by farmers in the agricultural season in each survey round. Second, we consider three dimensions of agricultural production: agricultural yield, the value of agricultural harvest (production), and agricultural input use. Agricultural productivity is captured by a yield index which is calculated by normalizing the yields for each crop (the total amount of harvested crop divided by the size of area on which the crop was cultivated) and take the unweighted average of this norm over the crops. The value of agricultural harvest is given by the monetary value of the harvest. Farmers' input use is captured by the share of farmers who use inputs for agricultural intensification (such as compost, manure, chemical fertilizer), the total size of area cultivated, and the total number of (family) labor days that farmers spent on SLMP implementation. Third, we also estimate the impact of the program on a set of intermediary outcomes. To capture the sources of external help in SLMPs implementation, we measure the share of farmers who received assistance in implementation from the SLWMP program, from peers in their social network, or from other organizations (such as NGOs, village associations). To study the role of informational constraints, we measure the share of farmers who adopted practices for which obtaining information would have been an obstacle to adoption in the absence of the program. We also take the share of farmers who report their agricultural plots to be exposed to floods and soil erosion as subjective measures of soil quality.

In Table 1, we present summary statistics for household demographics from the first survey wave to describe households in our sample. Although the data were collected one year after

Table 1: Household characteristics by treatment status from the first survey line.

	(cluster)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Total	Control	Group 1 (2016)	Group 2 (2017)	T-test (P-value)			Normalized difference		
Variable	Mean/SD	Mean/SD	Mean/SD	Mean/SD	(2)-(3)	(2)-(4)	(3)-(4)	(2)-(3)	(2)-(4)	(3)-(4)
Female (0/1)	0.098	0.096	0.142	0.058	0.301	0.130	0.015**	-0.142	0.140	0.278
	(0.402)	(0.344)	(0.523)	(0.306)						
Age	48.318	49.338	47.841	47.519	0.553	0.138	0.635	0.103	0.125	0.022
	(19.074)	(19.288)	(20.831)	(17.343)						
Married (0/1)	0.822	0.856	0.720	0.875	0.008***	0.373	0.000***	0.347	-0.056	-0.400
	(0.519)	(0.475)	(0.587)	(0.360)						
Literate (0/1)	0.122	0.144	0.093	0.121	0.107	0.311	0.517	0.154	0.069	-0.088
	(0.411)	(0.456)	(0.350)	(0.404)						
Primary education (0/1)	0.202	0.220	0.206	0.175	0.864	0.226	0.247	0.033	0.112	0.081
	(0.530)	(0.525)	(0.651)	(0.416)						
Religion: Christian (0/1)	0.264	0.265	0.269	0.258	0.720	0.852	0.609	-0.011	0.014	0.025
	(0.836)	(0.887)	(0.925)	(0.713)						
Religion: Muslim (0/1)	0.404	0.378	0.516	0.333	0.041**	0.520	0.063*	-0.278	0.093	0.369
	(1.393)	(1.419)	(1.440)	(1.315)						
Adults in HH	4.409	4.639	4.154	4.362	0.051*	0.102	0.204	0.231	0.138	-0.100
	(2.711)	(2.093)	(3.525)	(2.369)						
Total agricultural area (in ha)	3.744	3.764	3.594	3.858	0.181	0.844	0.068*	0.053	-0.031	-0.083
	(5.429)	(6.538)	(4.040)	(5.226)						
Rainfed (0/1)	0.989	0.993	0.997	0.975	0.326	0.208	0.015**	-0.046	0.149	0.170
	(0.124)	(0.081)	(0.035)	(0.192)						
Share of sloped plots	0.467	0.468	0.445	0.485	0.571	0.861	0.629	0.053	-0.041	-0.093
	(0.604)	(0.524)	(0.699)	(0.618)						
Share of eroded plots	0.485	0.456	0.491	0.516	0.144	0.149	0.565	-0.079	-0.134	-0.054
	(0.531)	(0.493)	(0.548)	(0.564)						
N	754	291	223	240						
Clusters	75	29	22	24						

Notes: Weighted average values of the characteristics for the whole sample as well as for each group with a different treatment status, are presented in columns (1)-(4); standard deviations are presented in parentheses. Columns (5)-(7) present the p-values for the treatment group indicators from regressing the characteristic on the treatment indicators and district fixed-effects. Standard errors are clustered at the community level. Columns (8)-(10) present the normalized differences between each of the sub-treatment and the control group. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

the beginning of the intervention, we do not expect the selected household characteristics to be affected by changes in agricultural production within this time frame. In column (1) of Table 1, we present descriptive statistics over the whole sample. Household heads in the experiment were predominantly male (about 91%), on average 48 years old, and 82% of them were married. Only 12% of them were literate in either English or their native language, and only 20% of them finished primary school. 40% of the households in these communities were Muslim, 26% were Christian, and the remaining households held indigenous beliefs. An average household consisted of 4-5 adult members living together and had access to 3.7 hectare of cultivable land. 46% of its cultivable plots were situated on sloped land and households considered 49% of their plots to be eroded. Farmers in these communities rely entirely on rainfall to water their crops during the agricultural season.

Comparing households characteristics between communities in different treatment groups we find some differences; see Table 1. Pairwise t-tests in columns (5)-(7) show imbalances in eight of the 36 tests at the 10% level and in five at the 5% level: gender and religion of the household head, share of married household heads, the number of adults in the household, the total area of cultivable land, and the share of farmers who relied solely on rain for irrigation. When we asses the magnitude of these differences using the normalized differences in columns (8)-(10), we find that five of them are larger than the ciritcal threshold of 0.25 standard deviations (Imbens and Rubin, 2015). These differences emerge when we compare communities in Group 1 to the control communities or to communities in Group 2. Comparing the control communities to those in Group 2, we find no significant differences. We find similar differences in household heads' gender, marriage, and religion between applicant (non-applicant) farmers in Group 1 and applicant (non-applicant) farmers in Group 2 (see Table B.2 and Table B.3 in the Appendix), but we find fewer differences between admitted and rejected farmers (see Table 2). We address any remaining imbalances by controlling for these farmers characteristics in our regressions (Athey and Imbens, 2017).

In the second survey wave, we were able to recontact nearly all households from the first survey round. Column (1) of Table B.4 in the Appendix shows that we were able to trace 92% of the households in the second wave. However, there is a difference in this rate between treatment groups. The recontact rate in the control group (see Column (2)) is 98% which is significantly higher than the close to 90% recontact rate in the treatment groups (see Columns (3)-(7)). Despite the different recontact rates between groups, households observed in both survey waves

Table 2: Household characteristics by rejected and accepted farmers in treated communities.

Variable	(1) Total Mean/SD	(2) Rejected Mean/SD	(3) Accepted Mean/SD	(4) T-test (P-value) (2)-(3)	(5) Normalized difference (2)-(3)
Female (1/0)	0.116	0.080	0.139	0.108	-0.185
	(0.321)	(0.272)	(0.347)		
Age	47.950	47.176	48.448	0.447	-0.087
	(14.574)	(13.376)	(15.309)		
Married (0/1)	0.812	0.856	0.784	0.106	0.185
	(0.391)	(0.353)	(0.413)		
Literate (0/1)	0.107	0.104	0.108	0.905	-0.014
	(0.309)	(0.306)	(0.311)		
Primary education (0/1)	0.191	0.192	0.191	0.977	0.003
	(0.394)	(0.395)	(0.394)		
Religion: Christian (0/1)	0.251	0.280	0.232	0.335	0.111
	(0.434)	(0.451)	(0.423)		
Religion: Muslim (0/1)	0.458	0.456	0.459	0.962	-0.006
	(0.499)	(0.500)	(0.500)		
Adults in HH	4.467	4.632	4.361	0.250	0.132
	(2.054)	(2.135)	(1.998)		
Total agricultural area (in ha)	4.019	3.997	4.033	0.922	-0.011
	(3.246)	(2.835)	(3.492)		
Rainfed (0/1)	0.978	0.976	0.979	0.841	-0.023
	(0.147)	(0.154)	(0.142)		
Share of sloped plots	0.460	0.527	0.416	0.022**	0.263
	(0.419)	(0.404)	(0.424)		
Share of eroded plots	0.484	0.468	0.495	0.596	-0.061
	(0.446)	(0.448)	(0.446)		
N	319	125	194		

Notes: Simple average values of the characteristics for all applicants in the sample as well as for rejected and admitted farmers, are presented in columns (1)-(3); standard deviations are presented in parentheses. Columns (4) present the p-values for the farmer treatment status from regressing the characteristic on the farmer treatment indicators and district fixed-effects. Standard errors are clustered at the community level. Column (5)presents the normalized difference between rejected and admitted farmers. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

are still balanced on characteristics from the first survey wave across treatments (see Table B.5 in the Appendix). A probit regression of the attrition indicator on the interaction of community treatment status and household characteristics in Table B.6 in the Appendix shows that attrited households in treated communities are slightly different from those in the control communities in the agricultural area managed by farmers. Importantly, there is no differential attrition along the number of SLMPs adopted by farmers or farmers' agricultural productivity – our main outcome variables. Differential attrition is thus unlikely to bias our treatment effect estimates in the second year once we control for household characteristics.

Nevertheless, we address the possibility of differential attrition in two ways. First, we include households from the second survey who were surveyed as replacements to attrited households in the analysis. Survey enumerators were given a list of replacements to each households from the first survey before the collecting the second round of survey data. In case enumerators could not reach the originally surveyed households, they interviewed these replacements who lived in the same community and had the same intervention status as the replaced household. Table B.7 shows that replacement households are similar to attrited households, except for the age of the household head and the area of land managed by the farmer, and Table B.8 shows that the full sample in the second survey wave is balanced across intervention status. Second, we estimate an upper and lower bound for the main treatment effects using the Lee bound approach under differential attrition (Lee, 2009). This approach trims the distribution of the outcome variable in the control group by the difference in the recontact rate between the control and treatment groups (97% and 88%, respectively), so that the share of households that remain are the same in the control and treatment groups. To provide an upper (lower) bound to the treatment effect estimate, the bounding approach trims the upper (lower) tail of the distribution of the outcome variable in the control group and compares the trimmed mean of the control group to the means of the treatment groups.[3]

4 Empirical strategy

4.1 Estimation of the intention-to-treat effect at community level

We first study the effect of offering the input and technical assistance program to the treatment communities. To do so, we exploit the random assignment of communities to the three experimental groups (see the top level of Figure 1) and compare the average outcomes of communities one and two years into the program to the outcomes of communities that either never received the program or were yet to receive it. We obtain these intention-to-treat effects by estimating the following linear pooled-OLS model:

$$y_{icd,t} = \alpha + \beta_1\, T1_{cd,t} + \beta_2\, T2_{cd,t} + \gamma' X_{icd,1} + W_t + D_d + \varepsilon_{icd,t}. \tag{1}$$

In the model, $y_{icd,t}$ denotes the outcome of interest for farmer i in community c and district d, either in the first ($t = 1$) or the second ($t = 2$) survey wave. $T1_{cd,t}$ and $T2_{cd,t}$ are indicator variables that equal 1 if community c has been exposed to the technical assistance program for 1

[3]The Lee bound approach assumes that monotonicity in attrition holds, implying that assignment to the treatment group affects attrition towards one direction. We expect this assumption to hold because almost all households in the control group were recontacted (97%), thus assignment to the treatment group can only increase attrition.

Table 3: Overview of community treatment status over the two survey waves.

Community treatment status	Survey wave **2017 (W1)**	**2018 (W2)**
Control (C)	0	0
Treated in 2016 (G1)	T_1	T_2
Treated in 2017 (G2)	0	T_1

Note: The table shows which community level treatment indicator takes the value of 1 for each treatment group (C, G1, G2) in each survey round (W1, W2). T_1 (T_2) equals to one if the community has been exposed to the intervention for one (two) year(s) in the given year, and 0 otherwise.

or 2 years, respectively, in survey wave t, and they equal 0 otherwise (see Table 3). That is $T1_{cd,t}$ equals 1 only for Group 1 in survey wave 1 (G1W1) and for Group 2 in survey wave 2 (G2W2). Similarly, $T2_{cd,t}$ equals 1 only for Group 1 in survey wave 2 (G1W2). We control for a set of household characteristics in vector $X_{icd,1}$ which comprises of household head characteristics, household size, and the characteristics of the agricultural land managed by households. Doing so improves the precision of our estimates and mitigates any imbalances between the treatment groups. We also control for year (survey wave) fixed effects in W_t and district (strata) fixed effects in D_d. Finally, we cluster the idiosyncratic error term (ε_{icd}) at community level because this is the level at which treatment was assigned (Abadie *et al.*, 2017).

The coefficients of interest are β_1 and β_2, the first and second year effects of offering the technical assistance program to the communities, respectively. We estimate the one year effect (β_1) by comparing the outcomes of communities one year into the intervention to the outcomes of communities not (yet) exposed to the intervention (*G1W1* and *G2W2* vs *CW1-W2* and *G2W1*). Similarly, we identify the two year effect (β_2) by comparing the outcomes of Group 1 communities in the second year of the intervention (*G1W2*) to the non-treated outcomes (*CW1-W2*, and *G2W1*). [4]

We estimate Equation (1) using weighted least squares where the weights are the inverse sampling probabilities in the communities. We do so because we compare averages at the community level to obtain intention-to-treat effects and because we implemented a stratified sampling strategy in communities treated in 2016 (Group 1) instead of a random sampling

[4]Given the estimation method, the allocation of communities between treatment groups, and assuming an intra-cluster correlation of 0.2 and that the control variables can explain 25% of the variation in the outcome, the minimum detectable effect sizes 0.25 and 0.33 standard deviations for the first and second year intention-to-treat effects, respectively, with a power of 80%.

strategy. In these communities, the share of randomly accepted, randomly rejected, and non-applicant farmers within the sample is not representative of the share of each farmer categories within the communities. As explained in Section 3, we adjust for this by dividing the share of a farmer group (accepted, rejected, or non-applicant) in the sample by the share of the same farmer group in the community population, and use the inverse of this ratio as regression weights. Using these weights the results of the weighted regression are representative of the communities in the experiment. Note that this inverse probability will equal 1 for all observations in the communities where we implemented random sampling (the control group and Group 2).

4.2 Estimation of the treatment effect on the treated and spillover effects

The above intention-to-treat approach yields the impact of the intervention on treated communities, averaged over farmers regardless of their status in the program. The estimated effect in this approach thus combines both the direct effect of the intervention on admitted farmers and any potential spillover effects on other farmers in the same communities who are not in the program. In the next step we separate the average treatment effect on farmers accepted into the intervention from the spillovers effects on rejected and non-applicant farmers.

Three key features of the experiment enable us to identify the treatment effect on the treated and the spillover effects. First, the intervention was implemented a year later in Group 2 than in Group 1. Second, treated communities were randomly assigned to these two groups. Third, participant selection were identical in Groups 1 and 2. As a result, admitted farmers in Group 1 are comparable to (yet to be) admitted farmers in Group 2 at the end of wave 1. Thus we obtain the treatment effect on the treated by re-estimating Equation (1) on the set of admitted farmers in Group 1 and Group 2. Doing so we estimate the first year effect on admitted farmers by comparing the outcomes of accepted farmers one year after the intervention to the non-treated outcomes of admitted farmers in Group 2 (*G1W1* and *G2W2* vs. *G2W1*). The second year effect is estimated by comparing the outcomes of accepted farmers two year into the intervention to the non-treated outcomes of admitted farmers (*G1W2* vs *G2W1*). Similarly, rejected farmers (or non-applicants) in Group 1 are comparable to those (yet to be) rejected farmers (or non-applicants) in Group 2, and so restricting the estimation of Equation (1) on the set of rejected (non-applicant) farmers yields the spillover effect.

Since farmers were randomly sampled in each of the three farmer categories, we do not use regression weights in the estimation of these three effects. Also note that by restricting the sample to one of the three farmer categories, we exclude farmers in the control communities from this analysis because farmers in these communities did not have the opportunity to apply for the intervention and we do not observe which farmers would have applied for the intervention.

5 Impact on SLMPs adoption

5.1 Estimated intention-to-treat impacts in the first year

We begin the analysis by estimating the intention-to-treat effect on SLMP uptake one year after the start of the program. We do so by comparing SLMP uptake one year into the program to SLMPs uptake in the absence of the program (G1W1 and G2W2 vs CW1-2 and G1W2; β_1 in Equation (1)). Column (1) of Table 4 shows that farmers in treated villages used 0.26 more practices in the first year of the input and technical assistance program than farmers in control communities. This difference in SLMPs usage is significant and corresponds to 0.21 standard deviations increase compared to the control group's mean (1.718 practices).

Table 4: Impact of the input and technical assistance intervention on SLMP usage by farmer categories in treatment communities.

	Overall imapct	**Impact by farmer categories**		
	(1)	(2)	(3)	(4)
Dep. var.: # SLMs used		Admitted	Rejected	Non-applicant
SLM - 1 year	0.263***	0.673***	0.412*	0.123
	(0.098)	(0.173)	(0.214)	(0.279)
Observations	1505	385	258	282
Adjusted R^2	0.190	0.211	0.195	0.206
Controls	Yes	Yes	Yes	Yes
District FE-s	Yes	Yes	Yes	Yes
Control mean	1.718	1.552	1.575	1.850
Control std. dev.	1.213	1.091	1.413	1.366
Effect size	0.217	0.616	0.292	0.090
Unit	#	#	#	#

Note: All three columns present the OLS estimation results of Equation (1) on the whole sample (Column (1)) and on the three sub-groups of farmers in the treated communities (Columns (2) to (4)). The dependent variable in all columns is the number of SLMPsadopted by farmers. Standard errors, clustered at the community level, are presented in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

5.2 Treatment on the treated and spillover effects in the first year

The intention-to-treatment effect estimates above are the weighted average of the treatment effect on the treated and the spillover effects. In the next step, we disentangle these effects by applying a similar empirical approach for each farmer category in the treated communities. We present the estimated effects on the number of practices used by farmers in Columns (2)-(4) of Table 4.

Column (2) shows that the effect of the intervention on farmers admitted to the program is much larger than the previously estimated intention-to-treat effect. Farmers in the program used 0.67 more agricultural techniques one year into the intervention than in the absence of the intervention, a statically significant increase (p-value of 0.00). This effect is two and a half times the intention-to-treat effect (in Column (1)) and it corresponds to an effect size of 0.61 standard deviations. Although the treatment effect on admitted farmers is large, the absolute size of this point estimate suggests that the technical assistance intervention also provided support for SLMP implementation to farmers who did not implement the practices in the first year. Admitted farmers in the intervention received inputs and technical assistance to implement at least one practice from the program, thus if all of these farmers would have implemented and used the practice, we would expect a treatment effect of at least 1 on the SLMP uptake of admitted farmers. The effect suggests that at least 30% of admitted farmers were initially interested in adopting an additional practice but did not adopt one year into the intervention, while the remaining 70% of admitted farmers increased the adoption of the practices.

The remaining two columns of Table 4 show that there is a positive spillover effect of the intervention to rejected applicants in the first year of the intervention, but not so much to farmers who did not apply to the program. Column (3) shows that rejected farmers used 0.41 more practices in treated communities compared to those in the yet to be treated communities, which is statistically significant at the 10% level (p-value of 0.06). Farmers who showed interest in adopting SLMPs but did not receive direct support from the program thus also increased their use of the practices in the first year of the program. In contrast, we find no spillover effects on the SLMP usage of farmers who did not apply to the program. Column (4) shows that the estimated impact on these farmers is 0.12 practices or 0.09 standard deviations, which is a small and statistically insignificant effect (p-value of 0.66). Together the estimated spillover effects suggest that even though not all farmers received assistance from the program to implement the agricultural practices, those who were interested in the adoption of SLMPs were able to increase SLMP uptake. For this reason, we restrict the analysis of the spillover effects to rejected farmers in the remainder of the analysis.

We next turn to whether admitted and rejected farmers increased the uptake of the same type of practices despite their different enrollment status in the program. We present the estimated

Table 5: Impact of the input and technical assistance intervention on the likelihood of adopting each SLMPs by admitted and rejected farmers.

Dep. var.: share of farm.	(1) Integr. nutrient mgmt.	(2) Contour bunds	(3) Ridges & furrows	(4) Inter-cropping	(5) Crop rotation	(6) Fire mgmt.	(7) Land rotation	(8) Other practices
Panel A. Admitted farmers (N=385)								
SLWMP program	0.168**	0.243***	0.028	0.057	0.013	0.141**	0.032	-0.008
	(0.073)	(0.066)	(0.047)	(0.069)	(0.061)	(0.053)	(0.032)	(0.038)
Adjusted R^2	0.199	0.342	0.201	0.090	0.114	0.105	0.078	0.044
Effect size	0.348	0.631	0.084	0.115	0.029	0.441	0.190	-0.038
Panel B. Rejected farmers (N=258)								
SLWMP program	0.089	0.172*	-0.019	0.044	-0.048	-0.028	0.057	0.055
	(0.090)	(0.089)	(0.067)	(0.104)	(0.075)	(0.055)	(0.041)	(0.038)
Adjusted R^2	0.124	0.137	0.040	0.041	0.099	0.054	0.026	0.019
Control mean	0.357	0.179	0.129	0.429	0.279	0.114	0.029	0.050
Control std. dev.	0.481	0.384	0.336	0.497	0.450	0.319	0.167	0.219
Effect size	0.184	0.447	-0.056	0.089	-0.107	-0.088	0.342	0.252

Note: All columns present the results of estimating Equation (1) on two different groups of farmers in the treated communities using OLS. The estimation sample in Panel A consists of admitted farmers in treated communities, while it consists of rejected farmers in Panel B. The dependent variable is a binary variable on whether the farmer used the SLMP in question. Standard errors, clustered at the community level, are presented in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

treatment and spillover effects on the likelihood of adopting each practices in Table 5. Panel A presents the effects on admitted farmers. We find that the program increased the usage of contour bunds, integrated nutrient management, and fire management. The estimated effects on these practices range from 14 to 25 percentage points (or 0.34 to 0.61 standard deviations) and they are statistically significant at the 5% level. In the absence of the the program, the uptake of contour bunds and fire management would have been a mere 10% and 7% among applicant farmers, respectively. The uptake of integrated nutrient management among applicant farmers would have been higher, 35%, but still below the 48% overall uptake rate in the control communities (see Table B.9 in the Appendix). Admitted farmers in the program thus increased the uptake of agricultural techniques that would have a (relatively) low rate of usage in the absence of the program.

Turning to the estimated spillover effects on rejected farmers in Panel B of Table 5, we only observe a substantial increase in the uptake of contour bunds. The share of farmers constructing contour bunds increased by 17 percentage points, a sizeable 0.4 standard deviations increase. The increase in the use of contour bunds by rejected farmers even without input and assistance from the program is surprising as the construction of contour bunds is labor intensive (Liniger *et al.*,

Table 6: One and two-year impacts of the intervention on SLMP uptake of treated communities, applicant farmers, and non-applicant farmers.

	At community level	**By farmer category**	
	(1)	(2)	(3)
Dep. var.: # SLMs used		Applicant farmers	Non-applicant farmers
SLWMP program			
– 1st year	0.263***	0.596***	0.123
	(0.098)	(0.183)	(0.279)
– 2nd year	0.286*	0.362	0.239
	(0.168)	(0.313)	(0.391)
Observations	1505	643	282
Adjusted R^2	0.190	0.122	0.206
Controls	Yes	Yes	Yes
District FE-s	Yes	Yes	Yes
Control mean	1.718	1.564	1.850
Control std. dev.	1.213	1.265	1.366
Effect size			
– β_1	0.217	0.471	0.090
– β_2	0.236	0.286	0.175
T-test (p-value)			
$-\beta_1 = \beta_2$	0.883	0.233	0.683
Unit	#	#	#

Note: All columns are estimated using OLS. Column (1) presents the results from the intention-to-treat approach, while columns (2) and (3) present the estimated treatment effect on the treated and spillover effect following Section 4.2. The dependent variable in all three columns is the number of SLMPs adopted by farmers. Standard errors, clustered at the community level, are presented in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

2011). This suggests that labor constraints are not a primary barrier to SLMP implementation. Combined, the estimated spillover effects show that farmers not enrolled in the program only increased the use of contour bunds, which were not widely adopted otherwise and are typically labor intensive to construct.

5.3 Impact on SLMPs adoption in the second year

Finally, we evaluate whether farmers continued using the practices two years after the technical assistance intervention in treated communities. We first present the first and second year intention-to-treat effects of the program in column (1) of Table 6, then the first and second year treatment effects on applicant farmers and non-applicant farmers in columns (2) and (3) of the same table, respectively. In column (2), we pool the randomly accepted and rejected farmers into the group of applicants to improve the precision of the second year effect estimate given that only communities in Group 1 were exposed to the program for two years by the second survey wave and that there are substantial spillover effects of the program to the SLMP uptake of rejected farmers.

Table 6 suggests that the effect of the intervention on SLMP uptake persisted in the second year of the program. Column (1) shows that farmers in treated communities still used 0.28 more techniques than farmers in control communities two years into the intervention and this difference is significant (p-value of 0.06). Column (1) of Table C.10 in the Appendix also shows that the estimated first and second year impacts of the program are unlikely to be driven by differential attrition as both the lower and upper Lee bounds are positive and significant. Next, results in columns (2) and (3) of Table 6 show that applicant farmers continued SLMPs adoption and non-applicants may increased SLMPs adoption as well in the second post-intervention year. The estimated impact on the number of SLMPs used in the second year is 0.36 practices for applicant farmers (a 0.28 standard deviation increase) and 0.24 practices for non-applicants (a 0.17 standard deviation increase). However, we do not measure these effects precisely as neither of the second year effects are statistically significant (p-values of 0.25 and 0.54 for applicant and non-applicants, respectively) and neither can be distinguished statistically from the first-year effect (p-values of 0.23 and 0.68). Overall, our results suggest that the input and technical assistance program had a persistent effect on SLMP usage in the treated communities.

6 Impact on Agricultural Production and Livelihood

6.1 Agricultural yields and production

The input and technical assistance intervention increased SLMP uptake among those who were interested in adopting. Did this increase translate into improvements in agricultural productivity and livelihood of farmers? To answer this question, we estimate the impact of the intervention on the agricultural yield index and on the value of harvest, see Table 7. We calculated the yield index[5] and the value of harvest over maize, millet, and groundnut, the three most widely produced crops among farmers in the sample. The table shows that the program did not affect agricultural productivity in treated communities significantly. The estimated differences in the agricultural productivity index and in the value of agricultural production between treated and control communities are small and insignificant in both years following the intervention (see

[5]The yield index is calculated by normalizing the yields (amount harvested in kilogram over one hectare) using the mean and standard deviation of the yields in the control communities for each crops, and taking the unweighted average of these norms over the crops produced by the farmer. The index provides a measure for yields over the most widely produced crops taking into account that some farmers may not produce all these crops.

Table 7: Impact of the intervention on agricultural outcomes.

	At community level		Applicant farmers	
	(1)	(2)	(3)	(4)
	Productivity index	IHS(Production value)	Productivity index	IHS(Production value)
SLWMP program				
– 1st year	0.004	0.223	-0.056*	0.102
	(0.020)	(0.175)	(0.032)	(0.311)
– 2nd year	0.018	-0.049	-0.068	0.068
	(0.035)	(0.225)	(0.053)	(0.356)
Observations	1437	1505	622	643
Adjusted R^2	0.245	0.262	0.251	0.224
Controls	Yes	Yes	Yes	Yes
District FE-s	Yes	Yes	Yes	Yes
Control mean	-0.065	6.897	-0.020	7.412
Control std. dev.	0.312	2.794	0.354	2.111
Effect size				
– β_1	0.012	0.080	-0.157	0.048
– β_2	0.058	-0.017	-0.192	0.032
Unit	Std.dev	IHS(GHC)	Std.dev	IHS(GHC)

Note: All columns are estimated using OLS. Columns (1) and (2) present the results from the intention-to-treat approach, while columns (3) and (4) present the treatment effects on the treated. The two dependent variables in the table are the the productivity index (the unweighted average of the standardized yields (using the means and standard deviations) of millet, maize, and groundnut) and the total value of agricultural harvest from the same crops transformed with the inverse hyperbolic sine function (to mitigate outliers). Standard errors, clustered at the community level, are presented in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

columns (1) and (2)). We also find similar small impacts on the productivity and total harvest value of applicant farmers (see columns (3) and (4)). Thus in-kind support for SLMP implementation increased SLMP uptake among applicants, but this did not subsequently improve the agricultural productivity or the agricultural livelihood in the first two years after the program.

6.2 Agricultural input use

Despite the lack of improvements in agricultural outcomes, the program may have induced farmers to adjust their input use on their lands and hence farmers' costs in agricultural production (Foster and Rosenzweig, 2010; Suri, 2011). Farmers may have increased input use to implement the SLMPs, or re-optimized input allocation in response to increased SLMP usage and the free support delivered by the program (Gignoux *et al.*, 2022). We estimate the impact of the technical assistance program on the use of inputs for agricultural intensification (such as compost, manure, and fertilizer), the area cultivated, and the number of family days that farmers invested in SLMP implementation to uncover the changes in farmers' input use.

Table 8: Impact of the input and technical assistance intervention on agricultural input use.

	(1) Compost	(2) Manure	(3) Chemical fertilizer	(4) Pesticide	(5) Herbicide	(6) Land	(7) Labor	(8) Input index
Panel A. Treated vs control communities								
(N=1505)								
– 1st year	-0.014	0.028	0.136***	0.036	0.008	-0.135	0.389***	0.061**
	(0.015)	(0.029)	(0.031)	(0.030)	(0.034)	(0.162)	(0.120)	(0.029)
– 2nd year	-0.000	0.020	0.048	0.014	0.051	-0.155	0.444**	0.058
	(0.032)	(0.050)	(0.048)	(0.045)	(0.054)	(0.277)	(0.169)	(0.042)
Adjusted R^2	0.199	0.124	0.288	0.100	0.261	0.299	0.154	0.332
Control mean	0.055	0.110	0.553	0.196	0.460	3.764	2.650	-0.000
Control std.dev.	0.228	0.313	0.498	0.398	0.499	3.061	1.817	0.436
Effect size								
– β_1	-0.059	0.090	0.273	0.090	0.016	-0.044	0.214	0.140
– β_2	-0.001	0.063	0.096	0.035	0.101	-0.051	0.244	0.133
F-test								
– $\beta_1 = \beta_2$	0.713	0.841	0.044	0.617	0.386	0.921	0.699	0.941
Panel B. Applicant farmers								
(N=643)								
– 1st year	0.017	0.051	0.253***	-0.031	-0.084	-0.060	0.767***	0.113***
	(0.022)	(0.045)	(0.045)	(0.046)	(0.059)	(0.325)	(0.204)	(0.042)
– 2nd year	0.040	0.053	0.202***	-0.067	-0.038	-0.165	0.664**	0.125**
	(0.035)	(0.075)	(0.059)	(0.055)	(0.068)	(0.398)	(0.291)	(0.054)
Adjusted R^2	0.177	0.138	0.344	0.086	0.273	0.267	0.170	0.325
Control mean	0.036	0.136	0.479	0.207	0.593	4.025	2.490	-0.019
Control std.dev.	0.186	0.344	0.501	0.407	0.493	2.770	1.822	0.316
Effect size								
– β_1	0.093	0.148	0.505	-0.076	-0.170	-0.022	0.421	0.357
– β_2	0.212	0.155	0.403	-0.164	-0.077	-0.060	0.364	0.394
F-test								
– $\beta_1 = \beta_2$	0.439	0.962	0.186	0.343	0.365	0.596	0.536	0.733

Note: All models are estimated using OLS. Results in Panel A are obtained from the intention-to-treat approach, while those in Panel B are obtained from the sub-group approach. The dependent variables are binary variables which indicate if farmers' used compost, manure, chemical fertilizer, pesticide, and herbicide; the total cultivated area measured in hectares; and the total number of labor days farmers devoted to SLMPs implementation. The input index in column (8) is the unweighted average of each of these input use measures standardized using their means and standard deviations. Standard errors, clustered at the community level, are presented in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

We present the estimated impacts in Table 8. We find significant and positive treatment effects on chemical fertilizer use and household labor in SLMPs adoption in the first year of the intervention – see columns (3) and (7), respectively. This finding is consistent across our intention-to-treat effects in Panel A and the treatment effects on applicant farmers in Panel B. Panel B also shows that the treatment effects of the intervention on applicant farmers' fertilizer use and the number of labor-days in SLMP adoption are also positive and significant in the second year. The estimated effects correspond to a $20 - 25$ percentage points increase in fertilizer take-up and to a $6-7$ days increase in time spent on SLMP adoption and maintenance (see columns (3) and (7) in Panel B). Although there are no effects on the use of other inputs for production, the treatment effects on fertilizer use and family labor are sufficiently large so that we find a significant positive impact of the intervention on the the overall input use index of applicant farmers (see column (8) in Panel B). Thus farmers who were interested in SLMP adoption before the program and used more agricultural practices due to the intervention also invested more inputs in agricultural production. In addition, the lack of productivity improvements in the first two years of the program and the increase in input use in production imply that the short-run private returns on SLMP implementation induced by the program are most likely to be negative. The costs of SLMP implementation are immediate, while the potential agricultural productivity improvements may be realized only in the future. However, we cannot to reject that the estimated increase in input use is robust to potential systemic attrition: while the upper Lee bounds for the impact on input use are positive and significant, the lower Lee bounds are virtually zero; see in column (2) of Table C.10 in the Appendix.

7 Understanding the impacts on SLMPs adoption and agricultural production

7.1 Alleviated barriers to SLMPs adoption and the spillover effect

We thus find that farmers who applied to the program increased the use of SLMPs after the intervention. Farmers admitted to the program received inputs, tools, and assistance for SLMP implementation, and the opportunity to consult with government extension agents. Rejected farmers did not receive direct support from the program, yet they increased their SLMP usage. We next explore why both admitted and rejected farmers were able to adopt more practices in treated communities. We address this question by estimating the treatment effect on admitted farmers and the spillover effect on rejected farmers on a set of intermediate outcomes (including input use, labor use, and self-reported difficulties associated to the adopted SLMPs), and compare these effects between the two types of farmers. We also address the question whether the positive impacts of the program on rejected farmers' SLMPs usage captures the spillover effect of the program rather than the artifact of imperfect treatment enforcement. An alternative explanation of the results on rejected farmers could be that rejected farmers received inputs and technical assistance from the project on the field despite the random rejection of their appli-

Table 9: Input use by admitted and rejected farmers.

	(1) Compost	(2) Manure	(3) Chemical fertilizer	(4) Pesticide	(5) Herbicide	(6) Purchased Seed	(7) Index
Panel A. Admitted farmers (N=385)							
SLWMP program	0.032	0.122**	0.296***	-0.094*	-0.131	0.081**	0.145**
	(0.024)	(0.057)	(0.069)	(0.055)	(0.081)	(0.031)	(0.055)
Adjusted R^2	0.159	0.142	0.350	0.079	0.284	0.020	0.126
Effect size	0.169	0.354	0.591	-0.232	-0.266	0.400	0.434
Panel B. Rejected farmers (N=258)							
SLWMP program	-0.010	-0.052	0.189***	0.065	-0.057	0.054	0.064
	(0.032)	(0.052)	(0.061)	(0.082)	(0.077)	(0.051)	(0.083)
Adjusted R^2	0.189	0.166	0.317	0.105	0.248	-0.016	0.145
Control mean	0.036	0.136	0.479	0.207	0.593	0.043	-0.016
Control std. dev.	0.186	0.344	0.501	0.407	0.493	0.203	0.335
Effect size	-0.052	-0.153	0.377	0.161	-0.116	0.265	0.191

Note: All models are estimated using OLS. The estimation sample in Panel A consists of admitted farmers in treated communities, while it consists of rejected farmers in Panel B. The dependent variables are binary variables indicating whether farmers use each of the inputs in the table; and an input index which is the average of the standardized input measures (standardized using their mean and standard deviation). Standard errors, clustered at the community level, are presented in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

cations. The estimated effect on rejected farmers would then have the same sign as the effect on admitted farmers, and the difference in the magnitude of these effects would reflect the difference between the share of admitted and rejected farmers having received the intervention. To distinguish between the spillover effect and imperfect treatment enforcement, we estimate the treatment effect on the use of input and labor not provided by the SLWMP program and compare the magnitude of these effects between admitted and rejected farmers.

We first assess whether improving access to inputs enabled farmers to increase SLMP usage by estimating the impact of the intervention on admitted and rejected farmers' input usage in Table 9. Panel A shows that admitted farmers increased their overall input use in response to the intervention (in line with the effects in Table 8), and especially so for animal manure, chemical fertilizer, and purchased seeds. When we exclude inputs that farmers sourced from the SLWMP program (free-of-charge) in Table 10, we find that the treatment effects on organic manure and purchased seeds remain unaffected, while the effect on chemical fertilizer becomes smaller and insignificant (p-value of 0.28). These results indicate that although the government provided fertilizers to admitted farmers in the intervention, they were able to obtain other inputs from private sources. Columns (4) and (5) in Table 10 also show that a smaller share of admitted farmers use pesticide and herbicide potentially due to the increased adoption of

Table 10: Input use excluding inputs sourced from the program by admitted and rejected farmers.

	(1) Compost	(2) Manure	(3) Chemical fertilizer	(4) Pesticide	(5) Herbicide	(6) Purchased Seed	(7) Index
Panel A. Accepted farmers (N=385)							
SLWMP program	0.031	0.116**	0.078	-0.112*	-0.144*	0.072**	0.053
	(0.023)	(0.057)	(0.071)	(0.058)	(0.082)	(0.029)	(0.057)
Adjusted R^2	0.128	0.108	0.339	0.086	0.287	0.012	0.164
Effect size	0.167	0.336	0.156	-0.276	-0.291	0.357	0.157
Panel B. Rejected farmers (N=258)							
SLWMP program	-0.010	-0.052	0.101	0.070	-0.057	0.032	0.025
	(0.026)	(0.052)	(0.069)	(0.082)	(0.077)	(0.052)	(0.083)
Adjusted R^2	0.099	0.166	0.336	0.117	0.248	-0.006	0.135
Control mean	0.036	0.136	0.479	0.207	0.593	0.043	-0.016
Control std. dev.	0.186	0.344	0.501	0.407	0.493	0.203	0.335
Effect size	-0.056	-0.153	0.202	0.173	-0.116	0.160	0.076

Note: All models are estimated using OLS. The estimation sample in Panel A consists of admitted farmers in treated communities, while it consists of rejected farmers in Panel B. The dependent variables are binary variables indicating whether farmers use each of the inputs in the table that are not sourced from the SLWMP program. The dependent variable in the last column is an input index which is the average of the standardized input measures in the previous columns (standardized using their mean and standard deviation). Standard errors, clustered at the community level, are presented in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

integrated nutrient management which encourages farmers to shift towards organic pest and weed control.

The estimated impacts of the program on rejected farmers' input use in Panel B of Table 9 suggest that these farmers were able to obtain inputs too. Rejected farmers increased the use of chemical fertilizer on agricultural plots by 19 percentage points (see Table 9), and this effect is still 10 percentage points when we exclude fertilizers sourced from the SLWMP program (see Panel B of Table 10). Although the latter effect is not significant (p-value of 0.14), these results suggests that some rejected farmers were able to obtain fertilizer provided by the program (potentially through their peers admitted into the program), while others were able to obtain fertilizers from the market. Altogether, we thus find a small effect of alleviating input market constraints on the diffusion of SLMPs and find that the increase in rejected farmers' input usage cannot be fully explained by imperfect treatment enforcement.

Next, we study whether the program enabled farmers to overcome labor constraints in the implementation and use of SLMPs. If the lack of or the high cost of labor prevented farmers from SLMP adoption, we would expect farmers, we would expect that farmers implemented more SLMPs with direct assistance from the program and family labor in SLMP adoption remains unaffected. We provide evidence of the minor role of labor constraints on SLMP adoption in

Table 11: Sources labor in SLMP implementation.

	(1) Assistance from SLWMP	(2) Family labor	(3) Help from peers in social network	(4) Assistance from others (NGOs, associations, etc.)
Panel A. **Admitted farmers (N=385)**				
SLWMP program	0.291***	0.758***	0.293**	0.091
	(0.088)	(0.224)	(0.118)	(0.068)
Adjusted R^2	0.087	0.223	0.066	0.108
Effect size	.	0.416	0.354	0.762
Panel B. **Rejected farmers (N=258)**				
SLWMP program	0.042	0.471	0.202	0.037
	(0.097)	(0.313)	(0.184)	(0.066)
Adjusted R^2	0.064	0.138	0.066	0.073
Control mean	0.000	2.490	0.336	0.014
Control std. dev.	0.000	1.822	0.828	0.119
Effect size	.	0.259	0.244	0.307
Unit	# SLMs	IHS(Labor days)	# SLMs	# SLMs

Note: All models are estimated using OLS. The estimation sample in Panel A consists of admitted farmers in treated communities, while it consists of rejected farmers in Panel B. The dependent variables are the number SLMPs practices which were implemented with assistance from the program (column (1)), the inverse hyperbolic sine of the number of labor-days invested in SLMPs implementation (column (2)), the number of practices implemented with the help of peers in the social network (column (3)), and the number of practices implemented with the help of other organizations (column (4)). Standard errors, clustered at the community level, are presented in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 11. Panel A shows that admitted farmers received assistance in the implementation of 0.3 practices from the program. However, these farmers also spent about 13 more family labor days implementing the agricultural practices and the number of practices implemented with the help of their peers increased by 0.3 practices (see columns (1) to (3) in Panel A). In Panel B we find that rejected farmers implemented the agricultural practices without program assistance (column (1) in Panel B). Rejected farmers report that they received assistance from the program in the implementation of a mere 0.042 practices, a very small and statistically insignificant impact especially when compared to the 0.29 practices reported by admitted farmers in Panel A. In line with the results on input use, this result suggests that the impact on rejected farmers is not the artifact of imperfect treatment enforcement, but it is rather a spillover effect. Turning to family labor, rejected farmers spent 7 more family labor days on SLMP implementation and they implemented 0.2 more practices with the help of their peers (columns (2) and (3) in Panel B), although these effects are imprecisely measured (p-values of 0.13 and 0.27, respectively). Altogether, the results show that both admitted and rejected farmers increased labor investment in SLMP implementation and agricultural production – regardless whether they were directly

assisted by the program or not –, which suggests that lack or high cost of labor is not a major constraint on SLMP adoption.

Finally, we evaluate whether information provision in the program induced farmers to increase SLMPs adoption. Although we did not explicitly measure farmers' knowledge about the practices to test this hypothesis, we show evidence that farmers adopted the practices for which lack of knowledge was a barrier to adoption pre-program. If the program was effective because it lifted informational constraints and farmers gained information about the practices, we would expect a higher share of treated farmers to report lack of knowledge was a barrier to adoption before the program for the adopted SLMPs. We estimate the impact of the intervention on the share of farmers who adopted integrated nutrient management, contour bunds, or fire management – the three SLMPs with increased uptake (see Table 5) – and report obtaining information about the practices was a difficulty in adoption before the program. Table 12 shows that the intervention increased both the share of admitted and rejected farmers by 7 percentage points. We thus find suggestive evidence that farmers in the intervention gained information about the practices and implemented those SLMPs for which lack of information was a barrier to their adoption. Combined with our finding that farmers increasingly adopted SLMPs with assistance from their peers due to the program, these results suggest that the spillover effects of the program on rejected farmers' SLMPs usage can be partly explained by the diffusion of information about the practices along social ties between admitted and rejected farmers. Similarly to Aker and Jack (2021), Carter *et al.* (2021), and Adjognon *et al.* (2022), rejected farmers can learn about the SLMPs by helping admitted farmers in their implementation and by asking for help from admitted farmers in SLMP implementation on their own land.

7.2 Lack of improvement in agricultural production

Despite the increased uptake of agricultural practices by farmers who were interested in adopting them, we found no impact on agricultural productivity in the first two years of the intervention. This contrasts to the findings of the literature which shows that agricultural productivity increases with the adoption of sustainable agricultural practices even in the short run (Pretty *et al.*, 2006; BenYishay and Mobarak, 2018; Adjognon *et al.*, 2022). We explore four potential explanations that may account for the contradiction between our findings and that of the literature.

First, we explore the possibility that agricultural productivity does not increase linearly in

Table 12: Knowledge as barrier to adoption before the program for adopted SLMPs.

	(1) Admitted farmers	(2) Rejected farmers
SLWMP program	0.073*	0.068*
	(0.043)	(0.038)
Observations	385	258
Adjusted R^2	0.032	0.068
Control mean	0.007	0.007
Control std.dev.	0.085	0.085
Effect size	0.858	0.802
Unit	share	share

Note: Both models are estimated using OLS. The dependent variable in the table is the share of farmers who report adopting integrated nutrient management, contour bunds, or fire management; and who report lack of knowledge as one of the main barrier to their adoption before the intervention. Column (1) presents the treatment effect on admitted farmers, while column (2) presents the spillover effects on rejected farmers. Standard errors, clustered at the community level, are presented in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

the number of practices adopted. If productivity only starts to increase beyond some threshold number of SLMPs adopted and the intervention did not induce a sufficiently large increase in average SLMP take-up, then agricultural productivity would be unaffected by the intervention (Foster and Rosenzweig, 2010). For instance, Manda *et al.* (2016) find that the adoption of individual SLMPs did not increase maize productivity in rural Zambia, but the adoption of combinations of SLMPs did. In case of such non-linearity in the relationship between SLMP take-up and productivity, we would expect that the intervention increased agricultural productivity of those farmers who used more SLMPs at baseline. We test this hypothesis by narrowing down the analysis to Group 1 and control communities (pooling applicant and non-applicant farmers), and estimating heterogeneity in the intention-to-treat effect on agricultural productivity by baseline SLMP usage. Column (1) of Table 13 shows that we find no differential impact on agricultural productivity by baseline SLMP adoption. We thus do not find evidence for a non-linear relationship between agricultural productivity and SLMP usage that would explain the lack of productivity improvement despite the increase in SLMP usage.

Next, we test whether the increased adoption of SLMPs did not increase overall agricultural productivity because a relatively high share of farmers was already using chemical fertilizers. The bottom of column (3) in Table 9 shows that 48% of applicant farmers reported applying chemical fertilizers on their plots in the absence of the program. This is a surprisingly high adoption rate of chemical fertilizers compared to those in the region (Sheahan and Barrett, 2017), suggesting that a significant share of farmers in the experiment is already cultivating their land intensively.

Table 13: Exploring the lack of productivity impacts.

	Productivity index			Soil erosion	
	(1)	(2)	(3)	(4)	(5)
Covariate (1/0):	# baseline SLMPs ≥ 2 (1/0)	Baselin chem.fert. used (1/0)	Admitted vs rejected (1/0)	Plots at risk of flood (1/0)	Plots at risk of erosion (1/0)
SLWMP program					
SLM - 1 year	-0.011	-0.029	-0.031	0.017	0.014
	(0.031)	(0.030)	(0.047)	(0.067)	(0.053)
SLM - 2 year			-0.066	0.014	-0.040
			(0.077)	(0.103)	(0.076)
SLWMP program x Covariate					
x Cov	-0.030	0.018	-0.052		
	(0.041)	(0.050)	(0.058)		
x Cov			-0.024		
			(0.069)		
Observations	517	517	622	643	643
Adjusted R^2	0.217	0.220	0.249	0.106	0.145
Controls	Yes	Yes	Yes	Yes	Yes
District FE-s	Yes	Yes	Yes	Yes	Yes
Control mean	-0.065	-0.065	-0.065	0.721	0.726
Control std. dev.	0.312	0.312	0.288	0.450	0.447
Effect size	-0.035	-0.093	-0.109	0.038	0.031
Effect size			-0.227	0.030	-0.089
Unit	Std.dev.	Std.dev.	Std.dev.	Share	Share

Note: All columns are estimated using OLS. The dependent variable is the agricultural productivity index (which is the weighted average of the standardized yields of millet, maize, and groundnut) in columns (1) and (2), and it is a binary variable in columns (3) and (4) which takes on value one if the farmer perceives her plots to be at the risk of flood or soil erosion and zero otherwise. Columns (1) and (2) present the coefficient on the interaction of the treatment indicator and a dummy variable which indicates if the farmer had at least two SLMPs in place at baseline (in column (1)) or if the farmers' application to participate in the intervention was admitted or rejected (in column (2)). Standard errors, clustered at the community level, are presented in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

If intensive cultivation is a substitute to the adoption of SLMPs, then adopting SLMPs may not have an additional impact on agricultural productivity. Adoption of SLMPs would then be expected to increase the productivity of farmers who did not use chemical fertilizers before the program. To test this hypothesis, we estimated the heterogeneous intention-to-treat effects on agricultural productivity by baseline chemical fertilizer use in Column (2) of Table 13. The estimated coefficient of the non-interacted term is small (0.03 standard errors) and statistically insignificant which indicates that the program did not improve the productivity of farmers who did not use chemical fertilizers. Therefore a relatively high share of chemical fertilizer usage cannot explain the lack of agricultural productivity improvement.

An alternative explanation is that there are substantial differences in how admitted and rejected farmers implemented the practices on their plot. Admitted farmers received direct assistance from the government and consulted with government extension agents about the practices, while rejected farmers implemented the practices only with the help of their peers. If only admitted farmers were able to implement the practices correctly as a result, we expect that SLMP adoption only increases the agricultural productivity of admitted farmers, but not of rejected farmers. Although our survey does not measure the quality of SLMP adoption or farmers' knowledge about the practices, we test this hypothesis by estimating the heterogenous impact on agricultural productivity by farmer group. We present the results of this test in column (3) of Table 13 and find no significant differences in the effect on agricultural productivity by farmer group. Therefore the efficiency of SLMPs in improving productivity does not depend on whether extension agents helped the farmer in SLMP implementation, and there is no differential impact on agricultural between admitted and rejected farmers.

As a final explanation for the absence of productivity improvement, we explore the possibility of SLMPs failing to mitigate soil erosion. We turn our attention to the increased uptake of contour bunds and estimate the impact of the intervention on farmers' perception of the risk of flood and soil erosion on their land. We limit our discussion to this one SLMP because both admitted and rejected farmers increased the uptake of contour bunds and the increase was largest for this practice.[6] In addition, contour bunds are expected to first have an impact on mitigating floods (or water runoffs) and on farmers' perception of this risk (Liniger *et al.*, 2011) from the three SLMPs which saw increased usage because of the program. Our results in column (4) and (5) of Table 13 show that the share of applicant farmers who perceive flood and soil erosion as a risk did not decrease in the two years after the intervention. The finding on perceived risk of floods is especially informative because 72% of applicant farmers were affected by floods at baseline and the intervention increased the share of farmers who used this practice from 18% to $35-42\%$ among applicant farmers. Thus, our result suggests that contour bunds implemented by admitted and rejected farmers were ineffective in mitigating soil erosion. Despite this finding, we are yet to conclude that they are ineffective altogether. If contour bunds were implemented incorrectly and farmers require more time to master the construction of bunds, then this practice may take more than two years to become effective. Testing for these explanations require further studies measuring the quality of SLMPs implementation and their impacts on the longer run.

[6] We also observed the increased uptake of integrated nutrient management and fire management by admitted farmers (see Panel A of Table 5), but these may take longer to mitigate soil degradation than contour bunds (Liniger *et al.*, 2011).

8 Conclusions

In this study, we evaluated the impact of a government subsidy program in Northern Ghana on the adoption of sustainable land management practices. Sustainable agricultural practices are expected by agronomist to mitigate and possibly reverse land degradation and soil erosion, and hence increase agricultural productivity. Despite the environmental and agricultural benefits, the adoption of these practices remained low. To promote the adoption of sustainable agricultural practices, the government of Ghana designed a program that addressed three potential reasons for the low adoption rates: the lack of information about the technologies, input market imperfections, and labor market imperfections. The program provided farmers with inputs, assistance in implementation, and the opportunity to consult with government extension agents for the adoption of SLMPs for free. We implemented a Randomized Controlled Trial to evaluate the impact of the program on usage of sustainable agricultural practices and agricultural production.

We show that the program increased the average number of SLMPs used by farmers in the communities. In particular, we find that the program was not only effective in increasing the number of SLMPs used by subsidized farmers in the program, but also in increasing the SLMPs usage of farmers who were interested in adopting the practices but were not admitted to the program – rejected farmers. We show that this positive spillover effect on rejected farmers' SLMPs usage became apparent already in the first year of the program. We provide suggestive evidence that rather than addressing input or labor market imperfections, the program was effective because it alleved informational constraints and because farmers learned from others' experience. We also showed that the increase in SLMPs usage persisted in the second year after the program, but we find no subsequent improvement in agricultural productivity.

Our study thus shows that government input subsidy and technical assistance programs can be effective in increasing the uptake of agricultural practices in the short run, but it also casts doubt about longer run impact of the policy. Farmers may observe the lack of improvement in agricultural productivity in the first two years of adopting the practices, deem that the practices are not productivity enhancing, and decide to disadopt the practices. Thus it is important for the improvement of the subsidy policy to understand why farmers' SLMPs adoption did not improve soil retention and agricultural productivity in the first two years of the program as suggested by agronomist.

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A List of sustainable land management practices in the SLWMP

Table A.1: List of sustainable land management practices promoted in the SLWMP program.

	Name
1	Integrated nutrient management
2	Contour bunds
3	Ridges and furrow techniques
4	Intercropping/Mixed cropping
5	Crop rotation
6	Strip cropping
7	Vegetation barriers
8	Minimum or zero tillage
9	Fire management
10	Compound farming system
11	Land rotation/Improved fallow system
12	Fodder banks

B Descriptive statistics

Table B.2: Comparison of applicants between Group 1 and Group 2 communities.

Variable	(gender) Total Mean (SD)	(2) Group 1 (2016) Mean (SD)	(3) Group 2 (2017) Mean (SD)	(4) T-test (P-value) (2)-(3)	(5) Normalized difference (2)-(3)
Female (1/0)	0.116	0.162	0.057	0.004***	0.327
	(0.321)	(0.369)	(0.233)		
Age	47.950	48.944	46.679	0.169	0.155
	(14.574)	(14.538)	(14.573)		
Married (0/1)	0.812	0.765	0.871	0.016**	-0.271
	(0.391)	(0.425)	(0.336)		
Literate (0/1)	0.107	0.106	0.107	0.977	-0.003
	(0.309)	(0.309)	(0.310)		
Primary education (0/1)	0.191	0.201	0.179	0.613	0.057
	(0.394)	(0.402)	(0.384)		
Religion: Christian (0/1)	0.251	0.274	0.221	0.286	0.120
	(0.434)	(0.447)	(0.417)		
Religion: Muslim (0/1)	0.458	0.514	0.386	0.022**	0.257
	(0.499)	(0.501)	(0.489)		
Adults in HH	4.467	4.559	4.350	0.369	0.102
	(2.054)	(2.061)	(2.046)		
Total agricultural area (in ha)	4.019	4.014	4.025	0.976	-0.003
	(3.246)	(3.582)	(2.770)		
Rainfed (0/1)	0.978	0.989	0.964	0.138	0.167
	(0.147)	(0.105)	(0.186)		
Share of sloped plots	0.460	0.449	0.473	0.625	-0.055
	(0.419)	(0.427)	(0.411)		
Share of eroded plots	0.484	0.472	0.499	0.589	-0.061
	(0.446)	(0.444)	(0.450)		
N	319	179	140		

Note: Simple average values of the characteristics for all applicants in treated communities as well as for applicants in Group 1 and in Group 2 communities, are presented in columns (1)-(3); standard deviations are presented in parentheses. Column (4) presents the p-values for the treatment status from regressing the characteristic on the community treatment indicator and district fixed-effects. Standard errors are clustered at the community level. Column (5) presents the normalized differences between applicants in Group 1 and applicants in Group 2 communities. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

Table B.3: Comparison of non-applicants between Group 1 and Group 2 communities.

Variable	(gender) Total Mean (SD)	(2) Group 1 (2016) Mean (SD)	(3) Group 2 (2017) Mean (SD)	(4) T-test (P-value) (2)-(3)	(5) Normalized difference (2)-(3)
Female (1/0)	0.069	0.091	0.060	0.505	0.121
	(0.255)	(0.291)	(0.239)		
Age	47.427	44.545	48.695	0.123	-0.279
	(14.865)	(14.841)	(14.772)		
Married (0/1)	0.826	0.705	0.880	0.010**	-0.462
	(0.380)	(0.462)	(0.327)		
Literate (0/1)	0.132	0.114	0.140	0.669	-0.078
	(0.340)	(0.321)	(0.349)		
Primary education (0/1)	0.167	0.159	0.170	0.873	-0.029
	(0.374)	(0.370)	(0.378)		
Religion: Christian (0/1)	0.292	0.250	0.310	0.469	-0.132
	(0.456)	(0.438)	(0.465)		
Religion: Muslim (0/1)	0.340	0.523	0.260	0.002***	0.553
	(0.475)	(0.505)	(0.441)		
Adults in HH	4.354	4.295	4.380	0.830	-0.039
	(2.166)	(2.681)	(1.911)		
Total agricultural area (in ha)	3.715	3.923	3.624	0.587	0.099
	(3.022)	(2.894)	(3.086)		
Rainfed (0/1)	0.993	1.000	0.990	0.509	0.120
	(0.083)	(0.000)	(0.100)		
Share of sloped plots	0.484	0.441	0.503	0.443	-0.139
	(0.441)	(0.418)	(0.452)		
Share of eroded plots	0.533	0.523	0.538	0.848	-0.035
	(0.442)	(0.458)	(0.436)		
N	144	44	100		

Note: Simple average values of the characteristics for all non-applicants in treated communities as well as for non-applicants in Group 1 and in Group 2 communities, are presented in columns (1)-(3); standard deviations are presented in parentheses. Column (4) presents the *p*-values for the treatment status from regressing the characteristic on the community treatment indicator and district fixed-effects. Standard errors are clustered at the community level. Column (5) presents the normalized differences between non-applicants in Group 1 and non-applicants in Group 2 communities. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

Table B.4: Recontact rates between the first and second survey wave by treatment status.

	(cluster)	(2)	(3)	(4)	T-test		
	Total	Control	Group 1 (2016)	Group 2 (2017)	P-value		
Variable	Mean/SD	Mean/SD	Mean/SD	Mean/SD	(2)-(3)	(2)-(4)	(3)-(4)
Recontacted (0/1)	0.919	0.976	0.888	0.879	0.001***	0.020**	0.981
	(0.467)	(0.161)	(0.384)	(0.686)			
N	754	291	223	240			
Clusters	75	29	22	24			

Notes: The value displayed for t-tests are p-values. Standard deviations are clustered at variable community_vl. Fixed effects using variable district_vl are included in all estimation regressions. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

Table B.5: Balance test on households characteristics of recontacted households in the first survey wave.

	(cluster)	(2)	(3)	(4)	T-test			Normalized		
	Total	Control	Group 1 (2016)	Group 2 (2017)	P-value			difference		
Variable	Mean/SD	Mean/SD	Mean/SD	Mean/SD	(2)-(3)	(2)-(4)	(3)-(4)	(2)-(3)	(2)-(4)	(3)-(4)
Female (0/1)	0.098	0.099	0.132	0.066	0.459	0.185	0.036**	-0.105	0.116	0.217
	(0.378)	(0.347)	(0.473)	(0.315)						
Age	48.274	49.255	48.111	47.107	0.592	0.105	0.527	0.079	0.149	0.068
	(19.116)	(18.806)	(21.215)	(17.583)						
Married (0/1)	0.835	0.859	0.741	0.891	0.031**	0.204	0.002***	0.309	-0.095	-0.401
	(0.518)	(0.470)	(0.600)	(0.397)						
Literate (0/1)	0.123	0.141	0.099	0.123	0.215	0.335	0.673	0.126	0.052	-0.076
	(0.423)	(0.462)	(0.352)	(0.434)						
Primary education (0/1)	0.202	0.218	0.206	0.175	0.811	0.236	0.368	0.031	0.107	0.078
	(0.534)	(0.498)	(0.680)	(0.429)						
Religion: Christian (0/1)	0.264	0.264	0.281	0.246	0.793	0.670	0.409	-0.038	0.040	0.078
	(0.833)	(0.872)	(0.936)	(0.700)						
Religion: Muslim (0/1)	0.409	0.377	0.506	0.360	0.041**	0.612	0.117	-0.260	0.034	0.293
	(1.353)	(1.403)	(1.365)	(1.295)						
Adults in HH	4.443	4.637	4.186	4.422	0.066*	0.325	0.122	0.216	0.107	-0.114
	(2.663)	(2.147)	(3.434)	(2.342)						
Total agricultural area (in ha)	3.783	3.799	3.516	4.014	0.063*	0.784	0.012**	0.086	-0.071	-0.153
	(5.482)	(6.587)	(4.187)	(5.034)						
Rainfed (0/1)	0.991	0.996	0.999	0.976	0.407	0.120	0.028**	-0.045	0.184	0.192
	(0.118)	(0.059)	(0.009)	(0.198)						
Share of sloped plots	0.477	0.469	0.455	0.509	0.694	0.478	0.483	0.033	-0.096	-0.128
	(0.583)	(0.508)	(0.691)	(0.578)						
Share of eroded plots	0.481	0.457	0.488	0.508	0.151	0.187	0.661	-0.070	-0.116	-0.045
	(0.536)	(0.492)	(0.553)	(0.587)						
N	693	284	198	211						
Clusters	74	29	22	23						

Notes: The sample in the table consists of households that we observe both in the first and second survey wave. Weighted average values of the characteristics over all groups as well as for each group with a different type of treatment status, are presented in columns (1)-(4); standard deviations are presented in parentheses. Columns (5)-(7) present the *p*-values for the treatment group indicators from regressing the characteristic on the treatment indicators and district fixed-effects. Standard errors are clustered at the community level. Columns (8)-(10) present the normalized differences between each of the sub-treatment and the control group. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

Table B.6: Test for systemic attrition in the first survey round sample.

	(1) probit$_{s}ysattrition$ Attrited (0/1)
Treatment indicator (SLWMP program)	-0.124
	(1.637)
Treatment indicator ×	
- # SLMPs used by farmers	0.157
	(0.128)
- Overall crop productivity	-0.112
	(0.130)
- Age	-0.018
	(0.013)
- Married	-0.196
	(0.432)
- Primary education	-0.380
	(0.529)
- Religion: Christian	-0.792
	(0.647)
- Religion: Muslim	-0.839
	(0.533)
- Adults in HH	-0.065
	(0.071)
- Total agricultural area (in ha)	0.283***
	(0.107)
- Rainfed	2.147*
	(1.287)
- Share of sloped plots	-0.575
	(0.425)
- Share of eroded plots	0.393
	(0.376)
Observations	715
District FE-s	Yes
Wald-test (p-value)	.002

Notes: The table shows the coefficients from a probit regression of the attrition indicator on the interaction of the pooled community treatment indicator (0 for control and 1 for any treated groups) and household characteristics from the first survey wave. The tables only shows the coefficients of the community treatment indicator and the interaction terms. The sample consists of all households from the first survey wave. Standard errors, reported in parentheses, are clustered at the community level. The bottom of the table shows the p-value from the jointly testing the presented coefficients in a Wald-test. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

Table B.7: Characteristics of attrited households and replacement households.

Variable	(1) Attrited HHs Mean/SD	(2) Replacement HHs Mean/SD	T-test P-value (1)-(2)	Normalized difference (1)-(2)
Gender (Female/Male)	0.093	0.255	0.005***	-0.429
	(0.349)	(0.388)		
Age	49.491	48.863	0.795	0.042
	(11.025)	(15.939)		
Married (0/1)	0.685	0.765	0.355	-0.177
	(0.572)	(0.431)		
Literate (0/1)	0.093	0.039	0.291	0.213
	(0.275)	(0.198)		
Primary education (0/1)	0.167	0.078	0.157	0.267
	(0.367)	(0.237)		
Religion: Christian (0/1)	0.278	0.353	0.268	-0.161
	(0.450)	(0.460)		
Religion: Muslim (0/1)	0.333	0.314	0.843	0.042
	(0.809)	(0.779)		
Adults in HH	4.056	4.098	0.937	-0.020
	(2.209)	(2.800)		
Total agricultural area (in ha)	3.516	2.469	0.104	0.441
	(3.627)	(3.063)		
Rainfed (0/1)	0.963	0.961	0.941	0.011
	(0.192)	(0.206)		
Share of sloped plots	0.369	0.420	0.466	-0.118
	(0.321)	(0.375)		
Share of eroded plots	0.557	0.571	0.813	-0.033
	(0.291)	(0.410)		
N	54	51		
Clusters	27	26		

Notes: The sample in the table consists of attrited households from the first survey wave and their replacements from the second survey wave. Simple average values of the characteristics of the attrited group and the replacement group are presented in columns (1)-(2); standard deviations are presented in parentheses. Column (3) presents the p-values for the treatment group indicators from regressing the characteristic on the farmer replacement indicator and district fixed-effects. Standard errors are clustered at the community level. Column (5) present the normalized differences between the two groups of farmers. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

Table B.8: Balance test on households characteristics of all households surveyed in the second wave.

	(cluster)	(2)	(3)	(4)	T-test			Normalized		
	Total	Control	Group 1 (2016)	Group 2 (2017)	P-value			difference		
Variable	Mean/SD	Mean/SD	Mean/SD	Mean/SD	(2)-(3)	(2)-(4)	(3)-(4)	(2)-(3)	(2)-(4)	(3)-(4)
Female (0/1)	0.109	0.100	0.141	0.091	0.335	0.843	0.110	-0.124	0.031	0.153
	(0.393)	(0.338)	(0.506)	(0.334)						
Age	49.203	50.005	49.514	47.955	0.981	0.106	0.178	0.034	0.142	0.106
	(19.777)	(18.985)	(22.812)	(17.836)						
Married (0/1)	0.829	0.852	0.740	0.884	0.023**	0.212	0.000***	0.289	-0.096	-0.384
	(0.490)	(0.468)	(0.555)	(0.331)						
Literate (0/1)	0.116	0.138	0.098	0.107	0.196	0.202	0.974	0.121	0.092	-0.031
	(0.419)	(0.458)	(0.374)	(0.415)						
Primary education (0/1)	0.190	0.214	0.195	0.157	0.641	0.110	0.231	0.047	0.145	0.100
	(0.544)	(0.498)	(0.707)	(0.415)						
Religion: Christian (0/1)	0.269	0.269	0.260	0.277	0.565	0.927	0.920	0.021	-0.018	-0.039
	(0.828)	(0.895)	(0.891)	(0.711)						
Religion: Muslim (0/1)	0.405	0.379	0.519	0.331	0.036**	0.511	0.055*	-0.282	0.102	0.382
	(1.393)	(1.418)	(1.437)	(1.315)						
Adults in HH	4.488	4.621	4.494	4.322	0.754	0.152	0.521	0.061	0.152	0.088
	(3.143)	(2.512)	(4.172)	(2.751)						
Total agricultural area (in ha)	3.292	3.355	3.259	3.246	0.432	0.377	0.827	0.038	0.046	0.006
	(4.370)	(4.850)	(4.219)	(4.063)						
Rainfed (0/1)	0.890	0.893	0.864	0.909	0.662	0.824	0.861	0.092	-0.053	-0.147
	(0.512)	(0.439)	(0.691)	(0.395)						
Share of sloped plots	0.425	0.415	0.477	0.389	0.070*	0.621	0.034**	-0.143	0.061	0.204
	(0.758)	(0.696)	(0.993)	(0.568)						
Share of eroded plots	0.558	0.541	0.604	0.536	0.046**	0.492	0.281	-0.144	0.011	0.154
	(0.521)	(0.495)	(0.595)	(0.473)						
N	755	290	223	242						
Clusters	75	29	22	24						

Notes: The sample in the table consists of households that we observe both in the first and second survey wave. Weighted average values of the characteristics over all groups as well as for each group with a different treatment status, are presented in columns (1)-(4); standard deviations are presented in parentheses. Columns (5)-(7) present the p-values for the treatment group indicators from regressing the characteristic on the treatment indicators and district fixed-effects. Standard errors are clustered at the community level. Columns (8)-(10) present the normalized differences between each of the sub-treatment and the control group. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

Table B.9: Baseline SLMP usage in control communities.

	Mean	Std. dev.
Integr. nutrient mgmt.	0.481	0.501
Contour bunds	0.151	0.359
Ridges & furrows	0.124	0.330
Inter- cropping	0.474	0.500
Crop rotation	0.302	0.460
Fire mgmt.	0.089	0.286
Land rotation	0.041	0.199
Other practices	0.052	0.221
Observations	291	

Notes: The table presents the share of farmers (with corresponding standard deviations) who reported using each SLMPs in the control communities. Shares and standard deviations are calculated using the farmers' responses in the first survey wave.

C Additional results

Table C.10: Lower and upper Lee (2009) bounds for the intention-to-treat effects.

	(1) # SLMPs adopted	(2) Input index
1^{st} year ITT impact	0.263***	0.061**
	(0.098)	(0.029)
- Lower bound	0.147*	0.003
	(0.083)	(0.023)
- Upper bound	0.367***	0.065***
	(0.084)	(0.023)
2^{nd} year ITT impact	0.286*	0.058
	(0.168)	(0.042)
- Lower bound	0.269**	-0.006
	(0.111)	(0.032)
- Upper bound	0.625***	0.098***
	(0.113)	(0.032)

Notes: The table presents intention-to-treat effects and the corresponding lower and upper bounds for the two main outcomes where we find a significant effect. Clustered standard errors from the OLS estimation for the intention-to-treat effects and bootstrapped standard errors for the bounds (over 1000 replications) are presented in parentheses. Bounds are tightened using strata fixed effects. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level.