Incentivizing Social Learning for the Diffusion of Sustainable Agriculture *

Guigonan Serge Adjognon[1], Tung Nguyen Huy[2], Jonas Guthoff[1], and Daan van Soest[2]

[1]Development Impact Evaluation Department (DIME), The World Bank Group, Washington D.C., USA

[2]Dep. of Economics and Tilburg Sustainability Center, Tilburg University, The Netherlands

Abstract

We implemented an RCT in arid Burkina Faso to estimate the impact of financial incentives on the adoption of sustainable land management practices. We did so in the context of a cascade training program, in which some farmers were trained in SLMP implementation, who were subsequently asked to disseminate their newly acquired knowledge and expertise to other farmers in their social networks. This paper reports two important findings. First, we find that offering payments conditional on adoption improves both the transfer of information from the trained to the peer farmers, as well as the peer farmers' adoption rates. Offering financial incentives thus mitigates two key barriers to SLMP adoption: the (perceived) lack of private benefits, and insufficient diffusion of technical implementation information. Second, reminiscent of the Coase theorem, the effectiveness of the financial incentive in inducing adoption is independent of how the money is initially allocated between the trained farmers and their peers, implying that it is the size of the total surplus that drives both the adoption and information acquisition decision. Combined with the result that the returns to adoption are, in fact, sizeable, our findings suggest that subsequent diffusion of the technologies' actual profitability will ultimately obviate the need for future adoption payments.

Keywords: Land degradation, sustainable land management, soil conservation, technology adoption, technology diffusion, cascade training programs.

JEL Codes: Q24, Q56, Q28, O13.

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1 Introduction

Sustainable land management practices, such as pit planting and organic fertilization, are thought to be both environmentally desirable as well as privately profitable (World Bank, 2008; Liniger *et al.*, 2011b). Adoption of these practices is of key importance especially in arid Sub-Saharan Africa where soil erosion and soil depletion threaten the long-run viability of agriculture on existing arable lands as well as necessitate the continued conversion of forested areas to create new arable land (World Bank, 2008; FAO, 2019).

Despite their promise of not just being environmentally beneficial but also privately profitable, take-up of these sustainable land management practices is typically low. Lack of benefits, actual or perceived, has been documented to be an important barrier to sustainable land management practices (SLMP) adoption (Jack, 2011; Beaman and Dillon, 2018), and the same holds for the lack of knowledge of how to implement them (Conley and Udry, 2010; Jack, 2011; BenYishay and Mobarak, 2018; Aker and Jack, 2021). These two barriers may be related, because of the cost-benefit considerations of information acquisition. If the technologies are deemed to not be very profitable (or even costly) to implement, farmers are less likely to be receptive to SLMP information provision – let alone to actively start searching for the information themselves.

In this study, we test whether offering financial incentives for the adoption of sustainable land management practices increases take-up, and to what extent it can help overcome the above two key barriers – limited (or even negative) perceived private benefits, and the lack of implementation know-how. We cooperated with the government of Burkina Faso and implemented a randomized controlled trial (RCT) to estimate the impact of offering farmers financial compensation conditional on the adoption of up to nine different SLMPs. The standard program is a so-called cascade training scheme (Banerjee *et al.*, 2013; BenYishay and Mobarak, 2018; Kondylis *et al.*, 2017; Behaghel *et al.*, 2020). A selected set of farmers were invited to participate in a four-day training program offered by the government's agricultural extension services. The program consisted of providing information on the benefits and costs of each of the nine SLMPs, as well as of training in how to implement them. Upon completion, the trained farmers (henceforth referred to as "contact farmers") were asked to actively disseminate their newly acquired knowledge and expertise among other farmers in their existing social network (henceforth "peer farmers"). All contact farmers received the cascade training program; the only difference between the treatment and the control group was that in the former, farmers were offered financial compensation for the number of SLMP technologies present on their peer farmers' land at endline.

We find that offering financial incentives increases SLMP adoption among peer farmers by about 0.4 standard deviation. Financial payments obviously improve the cost-benefit ratio of SLMP adoption, but we also find that it is especially the less widely used technologies (as measured at baseline) that experience the largest increase in uptake. Importantly, we also document that offering financial incentives increases the peer farmers' demand for knowledge and expertise: offering financial compensation for downstream adoption resulted in more frequent meetings between peer and contact farmers, in peer farmers being more likely to reach out to their contact farmers to ask for advice, and in more effort by contact farmers to support their peer farmers' adoption efforts. Finally, we also document that, at endline, "a lack of information" is significantly less of an adoption barrier in the payment treatment than in the control group.

We thus find that offering conditional adoption payments renders the cascade training scheme more effective. We also test whether for a given total financial surplus, the exchange of information and SLMP uptake can be fostered even more by providing contact farmers with a direct financial stake for disseminating SLMP information. We implemented two sub-treatments within the conditional payment treatment. While in both sub-treatments the payment to be disbursed is conditional on the number of SLMPs adopted by the peer farmer at endline, we vary how the payment is allocated between the peer farmer and her contact farmer. In one sub-treatment, the peer payment treatment, the full amount of the financial compensation is disbursed to the peer farmer; in the other sub-treatment, the payment is split, 80-20, between the peer and contact farmer. Interestingly, we find no statistically significant differences in either our output or input measures of information dissemination between the peer payment and the split payment treatment arms, and also no difference in the number of SLMPs adopted. This is an important outcome as it implies that if the downstream demand for information is sufficiently strong, supply will follow – independent of whether the contact farmer is directly rewarded for supplying information, or not. This result is reminiscent of the Coase theorem (Coase, 1960) which, in this context, states that for a given surplus generated by transferring knowledge, the amount of information transferred is independent of the initial allocation of the surplus. Previous research documented that providing contact farmers with explicit *additional* incentives to share their knowledge fosters both the exchange of information as well as subsequent technology adoption (BenYishay and Mobarak, 2018; Sseruyange and Bulte, 2018; Shikuku *et al.*, 2019). While highly relevant, it is important to note that such interventions are not likely scalable – the transaction costs of having to go out and monitor peer farmer adoption to determine the amount of compensation the contact farmer is entitled to, are likely prohibitive. Our Coasean result, however, indicates that it is the *total* surplus that matters for information dissemination and adoption – not its initial allocation. This is an important insight because it suggests that if SLMPs prove to be profitable by themselves (and we find evidence for this, see below), increased current adoption will induce fellow farmers to update their expectations, causing the cascade training scheme to become increasingly more effective even in the absence of explicit financial incentives for either adoption or information dissemination.

Our study contributes to three different strands in the literature. First, it speaks to the literature on the effectiveness of subsidizing sustainable land use practices. Despite the fact that the SLMPs are thought to increase agricultural production, subsidies are warranted because of the relatively low take-up using standard information dissemination practices. Farmers may be reluctant to invest in technologies that require considerable up-front costs and/or yield uncertain private returns in the distant future. Society may, however, be willing to subsidize the adoption of these technologies. By definition the societal benefits of sustainable techniques exceed the private benefits (as the agent adopting them typically only reaps a small and oftentimes negligible share of the environmental gains these technologies give rise to), and also the societal discount rate is typically lower than the private one. As such, our intervention can be viewed as an example of a "Payments for Ecosystem Services" (PES) scheme – a policy that aims to stimulate the private provision of nature conservation by offering financial compensation conditional on actual environmental service delivery (Wunder, 2007; Engel *et al.*, 2008; Engel, 2016). PES schemes have been shown to be effective in inducing forest and water conservation (Jayachandran *et al.*, 2017; Börner *et al.*, 2017); our study contributes to this literature by documenting the effectiveness of PES on the diffusion of sustainable land management practices. Our paper is thus related to Aker and Jack (2021) who study the effect of cash transfers and farmer training on the uptake of rain harvesting practices in Niger and find that cash transfers do not improve the effect of farmer training. Contrary to their result, we find that conditional payments can improve the diffusion of sustainable agricultural practices when information is transferred from farmers to farmers and knowledge acquisition is costly.

Second, our study speaks to the literature on the efficient dissemination of new (sustainable) agricultural technologies. Cascade training programs have been developed as an alternative to the traditional model of government agricultural extension services for essentially two reasons (Krishnan and Patnam, 2014; Kondylis *et al.*, 2017; BenYishay and Mobarak, 2018). One, large-scale diffusion of agricultural innovations is challenging in Sub-Saharan Africa, as the available resources are oftentimes insufficient for a nation-wide coverage of high-quality government extension services. And two, the standard extension services' approach of top-down information provision (from an extension worker to a farmer) is not always effective in convincing the latter of the desirability of adopting the new technology – oftentimes because of doubts whether the new technology is sufficiently well suited for the local agronomic circumstances. Cascade training systems may be able to alleviate (if not overcome) both issues. They hold the promise of being both more efficient as well as more effective than the traditional diffusion model. They may be more efficient as relatively few farmers need to be trained directly. And they may also be more effective, as information provided by a fellow farmer from the same region may be perceived as more reliable and better adapted to the local agronomic conditions than the information provided by (non-local) government extension workers.

Despite these promises, the evidence on the effectiveness of cascade training programs is mixed. Takahashi *et al.* (2019) experiment with a cascade training program aimed at disseminating rice management practices in Côte d'Ivoire and find that knowledgeable contact farmers are able to successfully disseminate the management practices among their peers. Kondylis *et al.* (2017), however, document that information dissemination is limited in a cascade training program in Mozambique, because of the contact farmers' low willingness to actively share their knowledge. A number of different interventions have been proposed that may increase the effectiveness of cascade training programs. Shikuku *et al.* (2019) explore whether social recognition and private in-kind rewards are effective in inducing contact farmers to better disseminate their newly acquired knowledge and expertise. They find that although both interventions increase dissemination effort, neither results in increased SLMP take-up among peer farmers.

A series of recent studies suggest that financial incentives may be more promising. In their seminal study in Malawi, BenYishay and Mobarak (2018) offer financial compensation to contact farmers dependent on their peer farmers' rate of agricultural practice adoption, and find that this increases both information dissemination and practice uptake among peer farmers. Sseruyange and Bulte (2018) and Berg *et al.* (2017) document that financial payments are also effective in raising the overall effectiveness of cascade training schemes in domains other than agricultural extension work. Sseruyange and Bulte (2018) make use of a financial literacy cascade training program among farmers in Uganda, and find that offering contact farmers money as a function of their peers' knowledge acquisition substantially improves peer farmers' financial literacy test scores. Berg *et al.* (2017) focus on the uptake of health insurance in India, and find that financial incentives are able to increase insurance uptake even among peers that are not socially close.[1] All these studies have in common that offering financial compensation to contact farmers does not

[1] Social distance between farmers can be a barrier to knowledge dissemination in a cascade training scheme. For example, Kondylis *et al.* (2016) study the role of farmers' gender within a cascade training program in Mozambique and find that the training does not increase female peer farmers' awareness and knowledge about agricultural technologies if their contact farmers are male. BenYishay *et al.* (2020) show that male peer farmers are reluctant to request information from female contact farmers. Berg *et al.* (2017) show that financial incentives can help mitigate these social barriers to information dissemination.

only provide incentives for knowledge sharing; the payments also increase the total surplus associated with knowledge dissemination and technology uptake. By implementing both the peer and split payment schemes (in addition to the control group), we shed light on the question whether, next to the size of the total surplus, there is an additional impact of the way in which the surplus is distributed.

Third, the insights provided in this paper are not limited to providing a proof of concept of PES in inducing SLMP uptake – it also allows us to actually estimate the farmers' short-run benefits (positive, or negative) of SLMP adoption. While the sustainable land use literature claims that SLMPs are not only socially desirable but also privately profitable, reliable productivity estimates are still scant (Liniger *et al.*, 2011a; Pretty *et al.*, 2011; Pittelkow *et al.*, 2015; Pretty *et al.*, 2018). Productivity impacts are typically estimated using either matching designs (Abdulai and Huffman, 2014; Kassie *et al.*, 2015; Manda *et al.*, 2016) or by exploiting time variation in the SLMP adoption decision (Arslan *et al.*, 2015; Khonje *et al.*, 2018; Kassie *et al.*, 2018; Tesfaye *et al.*, 2021). Estimates may, however, be biased because neither method is able to fully control for the role of unobservable characteristics in determining the (timing of the) adoption decision. Partly due to this, productivity estimates differ substantially between studies. For example, estimates of the productivity impacts of intercropping range between no effect (Arslan *et al.*, 2015) and 136% (Thierfelder and Wall, 2010), and anything in between (see for instance Manda *et al.* (2016); Tesfaye *et al.* (2021)). Our study complements those of BenYishay and Mobarak (2018) and Takahashi *et al.* (2019) by exploiting the treatment-induced exogenous variation in SLMP adoption to estimate the average short-run impact on agricultural revenues. As already hinted at above we find that, in the year of implementation, the impacts of SLMP adoption on agricultural productivity and income are positive and sizeable. As such, our study is among the few that provide causal evidence on the short-run impacts of SLMP adoption on farmer welfare. And it also suggests that, combined with the insight that it is total surplus that matters for the dissemination of information, the effectiveness of cascade SLMP training programs will increase with the dissemination of the information on profitability (via word-of-mouth, or via social learning). Combined, these results thus imply that temporary financial incentives can give rise to dynamics resulting in improved SLMP adoption not only in the short- but also in the longer run.

The remainder of this paper is organized as follows. In Section 2 we present the cascade training scheme, the interventions, and the experimental design, and in Section 3 we describe the identification strategy as well as the outcomes of the randomization process. The treatment effect estimates on the adoption of practices and on agricultural production are presented in Section 4, and those on knowledge dissemination in Section 5. Section 6 concludes the paper.

2 Program Description and Experimental Design

2.1 The Cascade Training Program

The RCT is embedded in a large-scale environmental conservation project, the Forest Investment Program (FIP), implemented by the government of Burkina Faso with financial support from the World Bank, the African Development Bank, and the Climate Investment Fund. One of the FIP's key objectives is to reduce the dependence of rural communities on (the unsustainable) exploitation of nearby forest areas – especially of those with protected forest status. Burkina Faso's protected forests are threatened by an increased demand for agricultural land, caused by rapid population growth as well as by dwindling productivity on existing agricultural lands (Pouliot *et al.*, 2012; Goldstein and Udry, 2008; Etongo *et al.*, 2015). The decline in agricultural productivity can be mitigated (and even reversed) by implementing so-called Sustainable Land Management Practices (SLMPs) – techniques and measures aimed at reducing soil depletion and erosion, as well as usage of sustainable inputs like organic fertilizers (Liniger *et al.*, 2011a; Pretty *et al.*, 2011).

In cooperation with the FIP we identified nine SLMPs that were deemed to be most promising in arid Burkina Faso; see Table 1. The nine SLMPs span three agricultural domains: agronomy, agro-sylvo-pastoralism, and agro-forestry. The agronomy-oriented

SLMPs focus on maintaining land productivity by conserving soil nutrients and retaining rainwater on farmers' plots. They include planting seeds in purposely prepared pits, constructing bunds (made from earth or stone) on plot perimeters, and building adequate structures for composting crop residue. The SLMPs in the agro-sylvo-pastoral domain (sometimes also referred to as integrated crop and livestock management) consist of producing and storing fodder from residues of agricultural production and from direct cultivation of forage crops, as well of (re-)using agricultural and forest by-products. These practices enhance agricultural productivity and reduce the grazing pressure on nearby lands. Finally, practices in agroforestry improve soil and water management by conserving tree and shrub cover on agricultural plots, to improve nutrient recycling and to reduce soils' exposure to direct sunlight. These nine SLMPs were selected because they were expected to improve short-term growing conditions as well as agricultural resilience to climate change, and especially so for low-input agriculture in arid countries (Liniger *et al.*, 2011b).

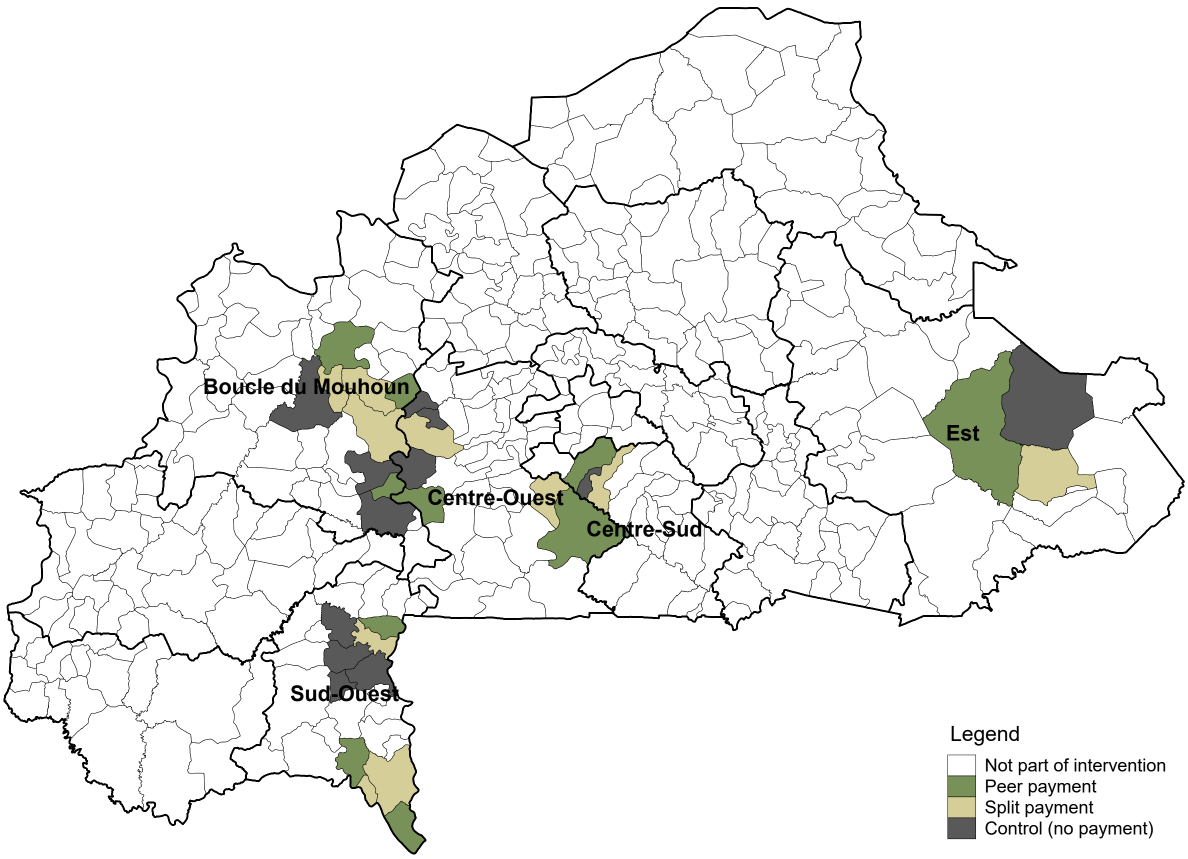
Table 1: Overview of our project's nine focal SLMPs.

Group	**Practice**
Agronomy	Pit planting (Zaï)
	Stone and earth bunds
	Heap and pit composting
Agro-sylvo-pastoral	Mowing and conservation of natural fodder
	Forage crop cultivation
	Use of agricultural and wood by-products
Agroforestry	Controlled clearing
	Assisted natural regeneration
	Living hedges

The FIP aimed to foster adoption of these nine SLMPs in 32 *communes* (municipalities, each consisting of several villages or hamlets) across five different regions: Boucle du Mouhoun, Centre Sud, Centre Ouest, Est and Sud-Ouest (see Figure 1). These regions were selected because they were among the FIP's target areas. The essence of the FIP project was to stimulate the adoption of SLMPs via dissemination of SLMP knowledge and expertise by means of a cascade training program; the timeline of the RCT is presented in Figure 2. In April 2019, the FIP recruited 320 farmers, 10 in each of the 32 communes, to participate in a four-day training on the nine key SLMPs described above. These farmers were selected because they had participated in earlier activities organized by the FIP, and because they were thought to be effective entry points for the diffusion of the practices in their respective communities. During the recruitment process, the farmers were informed of the general set-up of the intervention: (i) that they themselves would receive training in the implementation of nine climate-change resilient SLMPs that are considered effective in raising long-run agricultural yields; and (ii) that they would be expected to actively transfer the acquired knowledge and expertise to fellow farmers in their village. All farmers agreed to participate. Upon having accepted the invitation, each of these 320 farmers were asked to provide the names of five fellow farmers in their village whom they would expect to be interested in the adoption and usage of (some of) the SLMPs, and whom they would be willing to disseminate their newly acquired knowledge and expertise to. Importantly, the contact farmers in the treatment group were not informed of any possible payments until after they had completed the training and had returned to their villages. Therefore, the contact farmers' treatment status cannot have affected either their choice of whom to nominate as peer farmers nor the extent to which they paid attention during the SLMP training.

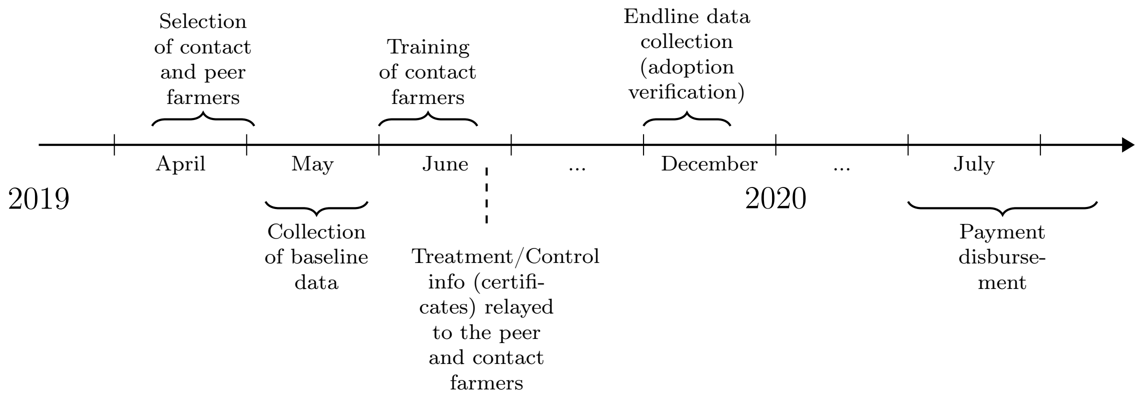
Because each contact farmer was asked to provide the names of five fellow farmers most likely to be interested in SLMP adoption, our experimental sample consists of, in total 1,920 farmers – the 320 so-called contact farmers who received the training, and the 1,600 so-called peer farmers whom the contact farmers may or may not have transferred their newly acquired SLMP knowledge and expertise to. As shown in Table A3 in Appendix A, contact farmers were, on average, older, more educated, and wealthier than the peer farmers; they also had more land as well as more experience with SLMP usage at baseline. This suggests that the contact farmers were indeed well-positioned to understand the benefits of these techniques and to disseminate them. This is in line with the FIP's goal of selecting contact farmers who are more likely to be good transmitters of knowledge given their education and experience. Also note that our contact farmers are similar (for example in terms of their role and social status in their respective communities) to those

Figure 1: Geographic location of the 32 communes involved in the RCT.



Note: This map depicts the 32 communes involved in the study and color-coded according to treatment status.

Figure 2: Timeline of the study.

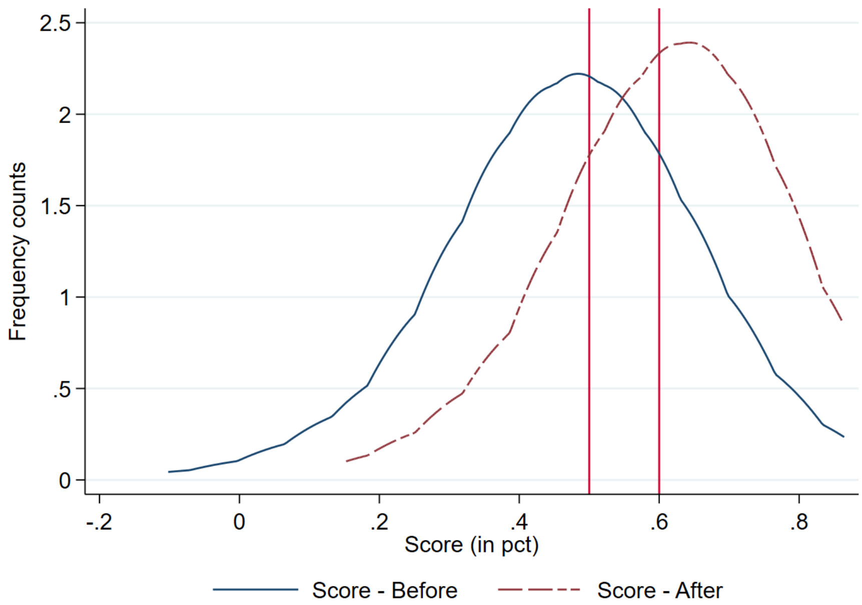


of Kondylis *et al.* (2017), and also to the lead farmers of BenYishay and Mobarak (2018).

The four-day training program for the contact farmers was developed by experts from the Ministry of Agriculture. The training itself was implemented at the commune level, in May and June 2019, by specially-trained government extension workers. On the first day of the training, the contact farmers received information on the longer-run consequences of soil erosion, on the theoretical benefits of each of the nine SLMPs in terms of reducing soil erosion and maintaining land productivity, as well as under what circumstances each SLMP was likely to be particularly useful or effective. The remaining three days were dedicated to practical training on the actual implementation of each of the nine SLMPs on demonstration plots.

To evaluate the effectiveness of the training and quantify the learning outcomes, we administered a test at the beginning and the end of the training on the content that was taught during the training. Figure 3 shows the distribution of farmers' test scores before and after training. It shows that farmers' knowledge improved substantially: the score of the median farmer increased by 30%, from 47% correct answers to 61%.

Figure 3: Distribution of the contact farmers' scores on the SLMP knowledge test before and after the training.



Note: The knowledge test consisted of multiple choice questions covering all nine SLMPs to be diffused via the cascade training scheme. The scores presented in this figure are adjusted for the expected score obtained by pure guessing, which is 41%. Denoting the unadjusted score by x, the adjusted score is: $x^{\text{adj}} = \frac{x-0.41}{1-0.41}$.

Upon completion of the training, all contact farmers were provided with a knowledge dissemination kit that included cheat sheets summarizing the key information on the benefits and implementation processes of each of the nine SLMPs. In addition, each contact farmer received an SLMP implementation kit containing agricultural equipment (including a wheelbarrow, a pickaxe, a shovel, a fork, and a bundler) as well as inputs (seeds and plants). Contact farmers were told that they were free to use these tools and inputs to facilitate the implementation of the various SLMPs on their own lands, but that these tools and inputs were also meant to be made available to their peer farmers upon request. At the end of the four-day training the contact farmers went back to their villages, and each of them was asked to actively disseminate the newly acquired information to the five peer farmers they had previously selected to be included in the study. They were also explicitly told that the project team would visit them as well as their five peers, at the end of the agricultural season, to evaluate the outcomes of the dissemination and SLMP adoption processes.

2.2 Experimental Design

Our main intervention consisted of offering financial compensation depending on the number of SLMPs present, at endline, on each peer farmer's land. In May 2019 our survey team georeferenced the perimeter of a maximum of five plots managed by each participant – those that the farmer had planned to cultivate in the 2019 agricultural season, and also those that were planned to be left fallow.[2] In the treatment group the amount to be disbursed thus depended on the number of SLMPs present, at endline, on each peer farmer's georeferenced plots. The payment scheme is presented in Table 2. If, in December 2019, between one and three SLMPs were present on a peer farmer's land, 30,000 FCFA would be disbursed; if the number of SLMPs present was between seven and nine, 50,000 FCFA would be paid out.

Four aspects of the payment scheme require additional discussion. First, payments were not conditional on the number of SLMPs adopted *during* the 2019 agricultural season, but rather on the number of SLMPs present on a peer farmer's land at endline.

[2]For practical reasons, we implemented the rule that if a participant managed more than five plots, she would be asked to point out those most suited for SLMP implementation (and prioritizing those which she intended to cultivate in the upcoming agricultural season). In our sample, farmers had control over an average of 1.66 plots, and the rule of georeferencing maximally five plots was binding for only three farmers.

Table 2: The total amount of money to be disbursed as a function of the number of SLMPs present on a peer farmer's land.

# of SLMPs present	**Payment**
0	0 FCFA
1-3	30,000 FCFA (≈ $ 50)
4-6	40,000 FCFA (≈ $ 68)
7-9	50,000 FCFA (≈ $ 85)

This was because the FIP considered that imposing strict additionality was unfair vis-à-vis those farmers who had already adopted SLMPs prior to the start of the intervention. Second, the payment scheme was such that the average payment per SLMP decreased with the cumulative number of SLMPs adopted. We implemented this because the adoption process is likely to be characterized by non-negligible set-up costs, such as the time and effort spent on acquiring information on the range of available technologies (Liniger *et al.*, 2011a; Giger *et al.*, 2018). Note that these two design choices affect the incentives to adopt, but not the internal validity of our RCT. Third, the amounts of money to be disbursed were sizeable. With an average farmer household's annual agricultural production of about $960 (WB, 2016, p. 52), the payments offered amounted to between 5% and 9% of an average farmer's annual revenues, or between 40% and 70% of the country's average annual per-capita food consumption of about $120 (WB, 2016, p. 29).

Fourth, and importantly, the disbursement of the payments only started in July 2020 – well after the end of the 2019 agricultural season; see Figure 2.[3] While informing treated farmers that they would be eligible to receive compensation based on the number of SLMPs adopted is expected to have fostered SLMP adoption – with possibly subsequent consequences for input use, crop choice, quantities harvested and agricultural revenues earned – it is unlikely to have affected farmers' budget constraint during the 2019 agricultural season itself. The prospect of receiving payments, almost a year after the start of the intervention, can only affect investment decisions via mechanisms other

[3]A delay was foreseen because of the administrative processes needed to clear the payments for each individual farmer, conditional on the independent verification of the end-line number of SLMPs present on each treatment farmer's land. At the start of the intervention farmers had been informed of the time needed for the payments to be approved; the actual length of the delay was longer due to the impact of COVID-19. The bulk of the payments took place in July and August 2020, and the last payments were made in November 2020.

than SLMP adoption if the farmers were able to borrow against such future payments. We will come back to this in Section 4.2.1.

The schedule presented in Table 2 determined the total amount of payments to be disbursed in the treatment group; farmers in the control treatment were not offered any financial compensation. The conditional payments treatment consisted, however, of two sub-treatments that only differed in how the payment was disbursed. In one sub-treatment, the full amount was disbursed to the peer farmer; in the other, the peer farmer would receive 80% of the payment, and her contact farmer would receive the other 20%. We will refer to these two sub-treatments as the peer and split payment treatments, respectively. All farmers in the treatment group were offered personalized certificates acknowledging participation and detailing the relevant conditional payment scheme.[4] As stated before, all farmers in the two sub-treatments were informed of the details of the relevant payment scheme only after the contact farmers had returned from the training. Both the selection of the peer farmers and the contact farmers' effort to gain knowledge in their training are thus independent of whether financial incentives were offered, and also of how the transfers were to be divided between the peer and contact farmers.

Regarding the RCT's implementation, assignment to each of the three (sub-) treatment groups was randomized at the commune level, stratified by region. Of the 32 communes, 12 were assigned to the control group, and ten to each of the two sub-treatment groups. That means that there were, in total, 720 farmers in the control group and 600 in each of the two sub-treatment groups. We motivate these design choices as follows. First, government extension services are organized at the commune level, and hence commune level randomization avoids the risk of incorrect treatment implementation that might occur when randomizing at the level of the village or even the contact farmer. Randomization at the commune level also mitigates concerns regarding both treatment spillovers and possible conflict. Spillovers can occur if contact farmers in the treatment group directly communicate with farmers in the control group, or if control group farmers

[4]Farmers in the control group also received personalized certificates that show the name of the farmer, the village, commune, and region name, the name of the georeferenced plots, and the name of the contact farmer. In the payment group, the certificate also detailed the condition and the structure of the payments. See Figure A1a-A1b in the Appendix.

observe the increased uptake of the technologies in the treatment groups and they decide to also adopt more SLMPs themselves. Given the communes' size and the distances between them, spillovers are less likely to be a concern when using cluster-randomized treatment assignment at the commune level than if we would we have randomized at the village or even the individual contact farmer level.[5] Second, our decision to assign 10 communes to each of the two payment sub-treatment groups and 12 to the control group was driven by statistical power considerations. We wanted to have a high-powered test of the overall impact of offering financial incentives, but we also wanted to be able to have a good chance of detecting the disbursement mechanisms' differential impact if there is one. Assuming both to be equally important and using z to denote the number of (sub-) treatment groups (in addition to the control group), statistical power is maximized when assigning a share of $1/(z + \sqrt{z})$ of the available treatment clusters to each of the z treatment groups, and hence a share of $\sqrt{z}/(z + \sqrt{z})$ of the available clusters to the control group (List *et al.*, 2011). Intuitively, because the control group is used as a reference outcome for the test of the effectiveness of either treatment, the joint statistical power of the two sub-treatment impact estimates is maximized by (slightly) oversampling the control group. Because of indivisibilities, we approximate this optimal allocation (for $z = 2$) by assigning 10 communes to each of the two sub-treatment groups, and 12 to the control group. With this treatment allocation, assuming an intra-cluster correlation of 0.1 and baseline covariates being able to explain 30% of the variation, we have an 80% chance of detecting an adoption impact of 0.4 standard deviations, or better.

2.3 Main Effects Identified in the Experiment

Our experimental design allows us to estimate three effects. First, the impact of offering financial compensation on SLMP adoption, and then especially whether offering compensation indeed stimulates the adoption of especially the lesser known (or lesser used) SLMPs. In the longer run, the usage of SLMPs is expected to be beneficial for the farmer adopting them, but their adoption may be less than perfect because of incomplete knowl-

[5]The average Euclidean distance between farmers in the control group and those in the closest treatment commune is 18 kilometers. Distances via the road network are likely to be substantially larger.

edge about the practice or because of the (perceived) riskiness of implementing them (Teklewold *et al.*, 2013; Karlan *et al.*, 2014). Offering farmers payments conditional on their adoption is thus expected to increase take-up, as the conditional payments increase the cost-effectiveness of the SLMPs (Foster and Rosenzweig, 1995; Koundouri *et al.*, 2006). Offering farmers a choice of which of the nine technologies to adopt implies that the estimated impact on take-up rate also reflects farmers' perceptions about which practices they expect to be beneficial (Teklewold *et al.*, 2013; Kassie *et al.*, 2015; Kpadonou *et al.*, 2017).

Second, in case of a significant impact on the uptake of SLMPs, this analysis will yield insight into the farmers' short-run benefits (positive, or negative) of SLMP adoption – by comparing agricultural productivity and income between the treatment and control groups. Because the payments were not disbursed until at least seven months after the end of the agricultural season, any difference in endline outcomes between the treatment and control groups is driven by the *prospect* of receiving money. Note however, that the estimate is unbiased if and only if farmers cannot borrow against (uncertain) future income (see below).

Third, offering peer farmers financial incentives to adopt SLMPs increases the benefits of adoption, and hence peer farmers' demand for knowledge and expertise from the contact farmer (Conley and Udry, 2010; Dupas, 2014). Offering adoption payments to the peer farmers presumably increases their willingness to pay for more detailed information on how to implement the SLMP technologies. In turn, the contact farmer may also have incentives to actively provide the required knowledge and expertise – think of side payments from the peer to the contact farmer, or the peer farmer reciprocating to the contact farmer in ways other than via a direct financial transfer. If the markets for information are perfect, the contact farmers' incentives to share information are independent of the allocation of the payments (100% for the peer farmer, or 80% to the peer farmer and 20% for her contact farmer). If this is indeed the case, the initial payment allocation will neither affect the peer farmers' SLMP adoption rates nor the knowledge transfer from the contact farmer to their peer farmer. The alternative hypothesis is that the market frictions affect the efficient transfer of information, and hence providing the contact farmer with a direct stake in their peer farmers' SLMP adoption decisions would result in higher actual adoption rates.

3 Empirical Framework

3.1 Identification Strategy

We implement two types of regression models. First, we use a simple linear model to capture the intention-to-treat impact of offering conditional payments on key outcome variables capturing SLMP uptake, agricultural production, and knowledge dissemination:

$$y^1_{icr} = \alpha + \beta y^0_{icr} + \tau T_{cr} + \gamma' X_{icr} + \delta' W_{cr} + R_r + \epsilon_{icr}. \tag{1}$$

In equation (1), y^t_{icr} denotes the outcome variable of interest observed at either baseline ($t = 0$) or endline ($t = 1$), for farmer i located in commune c in region r. When available, controlling for the baseline value of the outcome variable improves the precision of the treatment estimates (see McKenzie, 2012).[6] Next, T_{cr} captures the treatment status of commune c in region r the farmer is located in. We run two types of analyses, so that $T_{cr} = \{T^{Pooled}_{cr}, T^{Split}_{cr}\}$. In the first, we pool the peer and split payment groups to estimate the average impact of offering conditional payments on our outcome variables of interest. Treatment status is then denoted by T^{Pooled}_{cr} which takes on value 1 if the peer and contact farmers in commune c in region r have been assigned to either the peer payment treatment or the split payment treatment, and zero otherwise. In the other, we test how the initial allocation of the conditional payments affects outcomes. In that case, the analysis is restricted to just the peer and split payment groups. Treatment status is then captured by T^{Split}_{cr} which takes on value 1 if the peer and contact farmers in commune c in region r were assigned to the split payment sub-treatment, and 0 if they were assigned to the peer

[6]This modeling approach is typically referred to as the ANCOVA specification. The key difference with standard difference-in-difference models is that in equation (1) β can be estimated freely, while in the difference-in-difference (DID) models β is restricted to be equal to 1. The extent to which ANCOVA outperforms DID thus depends on the extent to which the value of the coefficient on the lagged dependent variable differs from 1 (McKenzie, 2012).

payment subtreatment. That means that in equation (1), τ is the estimate of either the average impact of offering conditional payments (if $T_{cr} = T_{cr}^{Pooled}$), or of the differential impact, for a given surplus, of the way in which the conditional payments are distributed (if $T_{cr} = T_{cr}^{Split}$).

Regarding the other covariates in equation (1), vector (X_{icr}) contains a series of baseline characteristics of the farmer (age, gender, and level of education), her household (family size and composition, and asset index) and of her farm (total agricultural land area, a land quality indicator, employment of family and hired labor, and agricultural and non-agricultural household income). In case farmer i is a peer farmer, vector X_{icr} also contains the knowledge score obtained by the contact farmer she was matched with, as an indicator of the (potential) quality of SLMP adoption information farmer i (may have) had access to. This is important because with a standard deviation of about 14%, there is substantial variation in contact farmers' test scores obtained; see Figure 3 and Table A1 in the Appendix. The vector of baseline commune level controls, W_{cr}, includes the share of farmers in the commune who have one or more SLMPs in place at baseline, as well as its quadratic term. These covariates are intended to control for possible social aspects associated with technology adoption (including learning by watching spillovers, or the strategic consideration to postpone adoption to wait and see; Bandiera and Rasul, 2006). Finally, R_r is a vector of region fixed effects, and ϵ_{icr} is the idiosyncratic error term which is clustered at the level of randomization – the commune level; see Abadie *et al.* (2017). For ease of interpretation, in the main body of this paper we present equation (1)'s regression results using ordinary least squares. Results of robustness tests regarding estimation methods (e.g., negative binomial models for the analysis of SLMP adoption) and hypothesis testing (randomized inference, and also multiple hypothesis testing) are presented in Appendix A.5.

We use equation (1) to estimate the impact of financial incentives on, among others, the number of SLMPs at endline, but we are also interested in uncovering which types of technologies experience the largest increase in take-up. One approach would be to separately estimate equation (1) for each of the nine SLMPs. This would, however, disregard the fact that the decisions of whether to adopt each of these practices, are not independent. Using s_1 and s_2 to index SLMPs, this implies that for $s_1 \neq s_2$ we have $\text{cov}(\epsilon_{icr,s_1}, \epsilon_{icr,s_2}) = \sigma_{s_1,s_2} \neq 0$. We take into account the possibility of unobserved factors simultaneously affecting the adoption decisions of multiple SLMPs by estimating our nine models using seemingly unrelated regression (SUR; see Wooldridge, 2010). The analyses of the SLMP-specific adoption decisions provides insight into the farmers' ex-ante assessment of the relative profitability (or more generally, desirability) of adopting techniques in the various domains (agronomy, agroforestry and agro-sylvo-pastoralism).

Equation (1) provides us with the intention-to-treat impacts of financial incentives, but we are also interested in estimating the treatment-on-the-treated effect of SLMP adoption on (indicators of) agricultural production. This can be done by estimating the following two-stage least-squares (2SLS) model:

$$\#SLMP^1_{icr} = \alpha + \beta\ \#SLMP^0_{icr} + \tau T^{Pooled}_{cr} + \gamma' X_{icr} + \delta' W_{cr} + R_r + \epsilon_{icr}, \tag{2}$$

$$y^1_{icr} = \mu + \theta\ \#\widehat{SLMP}^1_{icr} + \psi' X_{icr} + \eta' W_{cr} + R_r + \nu_{icr}. \tag{3}$$

Using $\#SLMP^t_{icr}$ to denote the number of SLMPs present at time $t = \{0, 1\}$ on farmer i's land, equation (2) estimates the exogenous increase in SLMP adoption as induced by the prospect of receiving the financial incentive; equation (3) then estimates the marginal impact of SLMP adoption on the key variable of interest, y^1_{icr}, by regressing that variable on the predicted number of SLMPs ($\#\widehat{SLMP}^1_{icr}$) as derived from equation (2). If offering financial incentives significantly and substantially increases SLMP uptake without any direct impact on y^1_{icr} itself (that is, if the exclusion restriction holds; see Section 4.2.1), the coefficient on these predicted values of the number of adopted practices (θ) captures the marginal impact of SLMP adoption on the relevant outcome variable of interest.

3.2 Data and Descriptive Statistics

We collected two types of data; see also the time line presented in Figure 2. First, we collected information on the types as well as number of SLMPs present at baseline and at endline (in May and December 2019, respectively) on each farmer's land. At baseline we identified which plots were eligible for SLMP implementation, and georeferenced up to five plots that were controlled by the farmers.[7] We also documented the type of SLMPs that were already in place, as well as the plot they were located on. At endline independent teams went back to each of the georeferenced plots to verify the presence and the types (and hence also the number) of SLMPs. Independent verification is important, especially at endline, because the endline number of SLMPs at a peer farmer's land determined the amount of money to be disbursed. To ensure truthful reporting, the teams were tasked to document the presence of the SLMPs by taking photographs of the identified SLMPs and by asking follow-up questions about the implementation of the practices. Identification of the SLMPs at endline was possible because the practices either do not appreciatively depreciate over the agricultural season, or leave visible traces on the field. Bunds, living hedges, and measures to assist tree growth (for assisted natural regeneration) stayed intact after the harvest. The presence of composting pits and storage of fodder was also easy to verify because they were implemented after harvest, right before endline data collection. And also the holes of pit planting remained visible at endline (see Figure A2 in the Appendix).

Second, we implemented two surveys, one at baseline and one at endline. In the baseline survey we collected information on the socio-demographic characteristics of all participants as well as on their family's size and composition, farm and non-farm activities, indicators of wealth and assets, and behavioral traits. We also collected detailed information at the plot level on how farmers cultivate their land. Overall, our baseline sample included 1,914 farmers, 319 contact and 1,595 peer farmers.[8] The endline survey was administered seven months after the baseline, in December 2019, at the end of the agricultural season. Of the 1,914 farmers interviewed at baseline, 1,901 (99.3%) were surveyed again at endline (see Table A2 in the Appendix). The potential bias from systemic

[7]As stated before, the constraint of a maximum of five plots was binding for just 3 of the more than 1,900 farmers in our sample.

[8]With 10 contact farmers per commune and with five peers for each contact farmer, we intended to have a sample consisting of 1,600 peer farmers and 320 contact farmers. Due to security concerns we were unable to reach and survey one contact farmer and his corresponding five peer farmers in the East region.

attrition is thus negligible. Our main outcome variables from the endline survey capture agricultural production, farmer livelihood, and the (intensity of) communication between peer and contact farmers. We analyze the impacts of the payments and the adoption of practices on agricultural production and livelihoods. On the input side of agricultural production we georeferenced the total area cultivated and registered the share of plots which were manually sowed (instead of mechanically), the amount of fertilizers and pesticides applied on the plots, the number of household members who worked on the plots, and the amount of money spent on hired labor. Agricultural production was captured by crop productivity, calculated as the total amount of a crop produced divided by the size of area (in hectares) on which the crop was produced. Livelihood outcomes were captured by the value of agricultural production,[9] the total income derived from keeping livestock and the amount of income obtained from non-agricultural activities. The last set of outcome variables measured the extent to which SLMP knowledge was disseminated by the contact farmer to each of her peer farmers. We captured interactions between contact and peer farmers by asking how frequently they met to discuss the SLMPs, how frequently the contact farmers verified if SLMPs were correctly adopted on the plots of the peers, and how often peer farmers asked their contact farmers for advice on the practices.

Table 3 presents the baseline characteristics of our peer farmers as well as the results of the balance tests across the three (sub-)treatment arms. As shown in column (1) of Table 3, the peer farmers in our sample were 41 years old, about 17 percent of them were female, and about 29 percent had at least some primary education. Furthermore, almost 75 percent were household heads, and 86 percent were living in rudimentary dwellings[10] and, on average, managed less than two plots with a total surface of about five hectares. Regarding the relationship between peer and contact farmers, 43% of the peer farmers were kin of their contact farmer; the remaining peers were neighbors or

[9]We asked farmers how much they harvested of each crop on each agricultural plot to measure agricultural production. We converted agricultural harvest to kilograms and summed up the produced quantities at the crop-farmer level. We also summed up the estimated value of harvest from the crop-plot level to the farmer level as a measure of total agricultural revenue. We tried to obtain more precise responses by asking farmers about agricultural production at the crop-plot level to induce deliberated responses.

[10]This is a measure of housing quality which equals one if the floor, the wall or the roof was made of rudimentary materials, and zero otherwise.

friends of the contact farmer, or acquaintances via wider social networks. Finally, note that the baseline use of SLMPs was quite low; on average, farmers used less than 2.5 SLMPs on their lands.

Table 3: Baseline characteristics of peer farmers, and the results of the pair-wise balance tests.

	(1)		(2)		(3)		(4)		(5)	(6)	(7)	(8)	(9)	(10)
	Total		Control group		Peer payment group		Split payment group		T-test (P-value)			Normalized difference		
Variable	N/[Clusters]	Mean/SD	N/[Clusters]	Mean/SD	N/[Clusters]	Mean/SD	N/[Clusters]	Mean/SD	(2)-(3)	(2)-(4)	(3)-(4)	(2)-(3)	(2)-(4)	(3)-(4)
Age	1595	41.377	600	42.382	500	40.542	495	41.002	0.148	0.205	0.545	0.169	0.128	-0.042
	[32]	(19.035)	[12]	(23.968)	[10]	(18.716)	[10]	(8.829)						
Female respondent (0/1)	1595	0.173	600	0.140	500	0.176	495	0.210	0.301	0.053	0.273	-0.099	-0.186	-0.086
	[32]	(0.734)	[12]	(0.870)	[10]	(0.557)	[10]	(0.683)						
Respondent is Head of Household (0/1)	1595	0.740	600	0.775	500	0.746	495	0.693	0.504	0.065	0.121	0.068	0.187	0.118
	[32]	(0.776)	[12]	(1.004)	[10]	(0.508)	[10]	(0.614)						
Has some primary education (0/1)	1595	0.285	600	0.298	500	0.320	495	0.234	0.683	0.050	0.031	-0.047	0.144	0.191
	[32]	(0.721)	[12]	(0.801)	[10]	(0.748)	[10]	(0.480)						
Adults in household	1595	11.695	600	12.217	500	11.084	495	11.679	0.333	0.651	0.436	0.170	0.077	-0.092
	[32]	(20.279)	[12]	(24.799)	[10]	(18.273)	[10]	(17.042)						
Deprived house (0/1)	1595	0.858	600	0.875	500	0.802	495	0.893	0.258	0.721	0.178	0.200	-0.056	-0.253
	[32]	(1.216)	[12]	(1.025)	[10]	(1.463)	[10]	(1.188)						
Asset index	1595	-0.136	600	-0.058	500	-0.174	495	-0.191	0.479	0.470	0.962	0.052	0.058	0.008
	[32]	(9.141)	[12]	(8.869)	[10]	(8.669)	[10]	(10.772)						
Association membership (0/1)	1595	0.657	600	0.635	500	0.614	495	0.727	0.654	0.159	0.064	0.043	-0.197	-0.241
	[32]	(1.437)	[12]	(1.740)	[10]	(1.400)	[10]	(1.016)						
Hired labor in previous agri. season (0/1)	1595	0.544	600	0.595	500	0.550	495	0.475	0.509	0.163	0.316	0.091	0.241	0.150
	[32]	(2.003)	[12]	(2.081)	[10]	(2.067)	[10]	(1.955)						
Number of plots under the control of the farmer	1595	1.699	600	1.802	500	1.516	495	1.760	0.051	0.844	0.119	0.366	0.049	-0.297
	[32]	(2.708)	[12]	(2.667)	[10]	(2.329)	[10]	(2.891)						
Number of eroded plots	1595	2.453	600	2.588	500	2.294	495	2.451	0.191	0.549	0.485	0.211	0.097	-0.110
	[32]	(3.989)	[12]	(3.677)	[10]	(4.465)	[10]	(3.964)						
Landholdings (ha)	1595	4.976	600	5.168	500	4.018	495	5.712	0.014	0.554	0.030	0.269	-0.107	-0.396
	[32]	(14.935)	[12]	(13.233)	[10]	(5.677)	[10]	(20.956)						
Number of SLMPs adopted at baseline	1595	2.349	600	2.367	500	2.234	495	2.444	0.488	0.918	0.347	0.096	-0.056	-0.150
	[32]	(5.782)	[12]	(5.891)	[10]	(6.301)	[10]	(5.633)						
Contact farmer is family member (0/1)	1594	0.432	599	0.474	500	0.400	495	0.412	0.156	0.289	0.878	0.149	0.125	-0.025
	[32]	(1.090)	[12]	(0.727)	[10]	(1.035)	[10]	(1.479)						
Income from agricultural production (IHS transformed)	1595	12.815	600	13.086	500	12.894	495	12.409	0.269	0.347	0.553	0.092	0.227	0.158
	[32]	(11.334)	[12]	(3.806)	[10]	(3.909)	[10]	(19.936)						
Household has income from non-agricultural activities (0/1)	1595	0.518	600	0.538	500	0.494	495	0.519	0.229	0.739	0.733	0.089	0.038	-0.050
	[32]	(1.090)	[12]	(0.881)	[10]	(0.912)	[10]	(1.505)						

Notes: Average values of the characteristics for the total sample of peer farmers as well as for each of the three sub-samples thereof, 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 region fixed-effects. Standard errors are clustered at the commune level. Columns (8)-(10) present the normalized differences between each of the sub-treatment and the control group.

Comparing the average values of all the baseline characteristics across the three treatment arms, differences are generally small; see columns (2)-(4) of Table 3.[11] As shown by the results of the pairwise t-tests in columns (5)-(8), peer farmers' characteristics are reasonably well-balanced across the treatment groups. Only eight of the, in total, 48 differences are statistically significant at the 10% level (including gender, age, education level and the size of their land holdings), and the differences themselves are also relatively small. Balance is thus decent, and this conclusion is reinforced when assessing the size of the normalized differences for each of the characteristics; see columns (8)-(10) of Table 3. Normalized differences are generally preferred to t-tests because they provide a scale-free comparison, and imbalances are typically identified as problematic if they are equal to 0.25, or higher (Imbens and Rubin, 2015; Abadie and Imbens, 2011). Only five out of the 48 normalized differences are larger than 0.25 standard deviations. Still, we follow Bruhn and McKenzie (2009) and mitigate the consequences of possible imbalances on our impact estimates by including all variables with significant differences as covariates in our regression analyses.

While Table 3 presents the overall picture of the (differences in) the characteristics of our sample, it is of particular interest to also check balance for the various types of SLMPs present at baseline. As shown in Table 4 farmers used on average 2.35 practices at baseline, and about 90% of them were using at least one SLMP. That is, most farmers are familiar with at least one practice, but the overall take-up rate of all the practices is still low compared to set of available practices. Regarding the presence of each of the nine SLMPs, three make up the bulk of the practices already in place, with usage rates of 37% and higher: heap and pit composting, use of agricultural and woody by-products, and controlled land clearing. At a usage level of 27.5%, stone and earth bunds were also quite widespread. Pit planting ("Zaï"), forage crop cultivation, and living hedges were practiced by only very few farmers. The low baseline usage rates of especially these techniques suggest that incentivizing SLMPs adoption can substantially improve the spread of SLMPs among farmers.

[11] Balance tests for the subsample of contact farmers are presented in Table A4 of the Appendix. We find no major imbalances for this subsample either.

Table 4: Presence of SLMPs on peer farmers' lands at baseline

	(1) Total		(2) Control group		(3) Peer payment group		(4) Split payment group		(5) T-test (P-value)	(6)	(7)	(8) Normalized difference	(9)	(10)
Variable	N/[Clusters]	Mean/SD	N/[Clusters]	Mean/SD	N/[Clusters]	Mean/SD	N/[Clusters]	Mean/SD	(2)-(3)	(2)-(4)	(3)-(4)	(2)-(3)	(2)-(4)	(3)-(4)
Number of SLMPs present at baseline	1595	2.349	600	2.367	500	2.234	495	2.444	0.488	0.918	0.347	0.096	-0.056	-0.150
	[32]	(5.782)	[12]	(5.891)	[10]	(6.301)	[10]	(5.633)						
At least one practice already present	1595	0.908	600	0.910	500	0.892	495	0.923	0.680	0.896	0.558	0.060	-0.048	-0.108
	[32]	(1.017)	[12]	(1.088)	[10]	(1.317)	[10]	(0.586)						
Zai	1595	0.045	600	0.045	500	0.030	495	0.059	0.774	0.646	0.535	0.078	-0.062	-0.139
	[32]	(0.852)	[12]	(1.015)	[10]	(0.530)	[10]	(0.968)						
Heap and pit composting	1595	0.470	600	0.480	500	0.456	495	0.471	0.462	0.570	0.852	0.048	0.019	-0.029
	[32]	(1.954)	[12]	(1.995)	[10]	(1.967)	[10]	(2.097)						
Stone and earth bounds	1595	0.275	600	0.278	500	0.254	495	0.293	0.717	0.865	0.605	0.055	-0.032	-0.087
	[32]	(1.319)	[12]	(1.399)	[10]	(1.218)	[10]	(1.441)						
Mowing and conservation of natural fodder	1595	0.173	600	0.210	500	0.150	495	0.152	0.341	0.284	0.975	0.155	0.151	-0.004
	[32]	(1.108)	[12]	(1.278)	[10]	(1.027)	[10]	(1.014)						
Forage crop cultivation	1595	0.059	600	0.053	500	0.052	495	0.073	0.991	0.727	0.670	0.006	-0.080	-0.086
	[32]	(0.828)	[12]	(0.659)	[10]	(0.844)	[10]	(1.053)						
Use of agricultural and wood by-products	1595	0.517	600	0.502	500	0.506	495	0.547	0.949	0.760	0.709	-0.009	-0.092	-0.083
	[32]	(2.410)	[12]	(2.448)	[10]	(2.676)	[10]	(2.335)						
Controlled clearing	1595	0.371	600	0.343	500	0.418	495	0.356	0.526	0.919	0.559	-0.154	-0.026	0.128
	[32]	(1.900)	[12]	(1.906)	[10]	(1.904)	[10]	(2.047)						
Assisted natural regeneration	1595	0.394	600	0.412	500	0.318	495	0.448	0.265	0.733	0.129	0.194	-0.074	-0.268
	[32]	(1.645)	[12]	(1.688)	[10]	(1.519)	[10]	(1.739)						
Living hedges	1595	0.046	600	0.043	500	0.050	495	0.046	0.679	0.835	0.808	-0.032	-0.015	0.016
	[32]	(0.353)	[12]	(0.318)	[10]	(0.252)	[10]	(0.492)						

Notes:: Average values, for the total sample of peer farmers as well as for each of the three sub-samples thereof, 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 region fixed-effects. Standard errors are clustered at the commune level. Columns (8)-(10) present the normalized differences between each of the sub-treatment and the control group.

Cascade training of farmers can ensure the accessibility of knowledge for farmers who only have experience with a small set of practices. This is especially evident if we look at the correlations between the number of adopted practices at baseline and farmer characteristics. Table A5 shows that young, female, less educated, and poorer farmers tend to have fewer practices at baseline. For this group of farmers, formal sources of information tend to be less accessible, either due to a lack of a social network, financial means, or abilities to access and understand formal sources of information (Krishnan and Patnam, 2014; Vasilaky and Leonard, 2018; BenYishay and Mobarak, 2018). Incentivizing these groups to learn via existing social ties is potentially an effective method to disseminate information between these farmers. Below we will use these characteristics to test for possible heterogeneities in the impacts of SLMP adoption on agricultural productivity and on livelihood outcomes.

4 The Impact of Conditional Payments on SLMP Adoption, and the Agricultural Consequences

4.1 The Impact of Offering Conditional Payments on SLMP Adoption

We start our analysis by addressing the first central question of this study: whether financial payments incentivize peer farmers to adopt SLMPs, and if so, what type of SLMPs are adopted – the already known ones, or also the ones that were used relatively little to date? We do so by pooling the peer payment and split payment groups, and then using equation (1) to estimate the impact of offering conditional payments on the number of SLMPs adopted. We then proceed by estimating the treatment effect for each of the nine SLMPs, to see what type of technologies saw the strongest increase in adoption. The results are presented in Table 5.

As shown in column (1) of Table 5, offering conditional payments increased the number of SLMPs present at endline by about half a practice. This difference is statistically significant (with a p-value of 0.036 for the relevant t-test, and with a value of the F-test of 62.62) and it is also sizeable as it represents an increase of 0.381 standard deviation (or an 18% increase in SLMP usage). This result is robust to re-estimating the model using negative binomial regression that takes into account the count nature of the SLMP usage data; see Table A10 in the Appendix.

The average increase of half a practice adopted by peer farmers may mask substantial variation in uptake between the nine practices. Columns (2)-(10) of Table 5 present the treatment impact estimates of the payment incentives on the likelihood of adopting each of the nine SLMPs. All nine models are estimated simultaneously using seemingly unrelated regressions, because time constraints, land constraints and/ or technical complementarities and substitutabilities may result in the various SLMP adoption decisions being correlated. We find that the treatment increased adoption of almost all SLMPs. Four saw the largest increase in uptake – the establishment of stone and earth bunds, mowing and conservation of natural fodder, assisted natural regeneration, and living hedges; see columns (4), (5), (9) and (10) of Table 5. The estimates range from 8 to 14 percentage point increases in the share of farmers adopting these practices, corresponding to effect sizes of between 0.20 and 0.41 standard deviations. The practices that saw the largest increases in take-up cover each of the three agricultural domains presented in Table 1; they were not concentrated in just one or two domains. We also observe positive and significant effects on the adoption of earth and stone bunds (which are typically thought of as labor intensive), and on the adoption of assisted natural regeneration and living hedges (which require investments in protecting existing trees and in planting shrubs); see Liniger *et al.* (2011a). This suggests that offering conditional payments did not necessarily induce farmers to just adopt those SLMPs with the least costs. Finally, the practices with the highest percentage point increases in usage were among the technologies least frequently used in the region (as measured by their usage in the control group). This holds especially for mowing and conservation of natural fodder and living hedges (see columns (5) and (10)).[12] The conditional payments are thus found to have

[12]Zaï and forage crop production also had very low baseline utilization rates with, in relative terms, fairly large increases in uptake. These effects are not measured with sufficient precision for these impacts

induced farmers to especially adopt the less known (or at least the lesser used) practices.

to be statistically significant; see columns (2) and (6) in Table 5.

Table 5: Treatment effects on the average number of SLMPs in use as well as on the usage of each of the nine individual SLMPs.

		Agronomy SLMPs			**Agro-sylvo-pastoral SLMPs**			**Agroforestry SLMPs**		
	# SLMPs adopted	**Zai**	**Heap and pit composting**	**Stone and earth bunds**	**Mowing and cons. of nat. fodder**	**Forage crop cultivation**	**Use of agr. and wood by-products**	**Controlled clearing**	**Assisted natural regeneration**	**Living hedges**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Payment treatment	0.507	0.024	0.083	0.101	0.131	0.040	-0.057	-0.012	0.138	0.084
	(0.232)	(0.032)	(0.058)	(0.055)	(0.055)	(0.064)	(0.062)	(0.104)	(0.071)	(0.021)
F-Statistic	62.623									
Observations		1574	1574	1574	1574	1574	1574	1574	1574	1574
Baseline outcome	Yes	No	No	No	No	No	No	No	No	No
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region FE-s	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	2.784	0.070	0.564	0.352	0.148	0.084	0.619	0.617	0.268	0.060
Control std.dev.	1.331	0.256	0.496	0.478	0.355	0.277	0.486	0.486	0.444	0.238
Effect size (in std.dev.)	0.381	0.095	0.167	0.210	0.370	0.144	-0.118	-0.024	0.311	0.354
Unit	#	share	share	share	share	share	share	share	share	share

Notes: Column (1) is estimated using OLS regression. Columns (2)–(10) are estimated simultaneously using Seemingly Unrelated Regression. Standard errors, presented in parentheses, are clustered at the commune level. Covariates include age, gender, a dummy variable that captures whether the farmer finished primary education, household size, a household asset index, the farmer's total agricultural land area, a binary variable that captures whether the farmer made use of hired labour, the number of plots threatened by erosion, the knowledge score of the peer farmer's contact farmer, and a binary variable on whether household has non-agricultural sources of income.

4.2 The Short-Run Productivity Impacts of SLMP Adoption, and the Livelihood Consequences

We thus document a sizeable increase in SLMP usage as induced by the prospect of receiving cash transfers conditional on the number of SLMPs used at endline. In this subsection we exploit the resulting exogenous variation in SLMP usage to estimate the short-run impact of SLMP adoption and usage on agricultural productivity. Whether our RCT provides reliable evidence on the short-run productivity impacts crucially depends on whether our treatment instrument, the prospect of receiving conditional payments well after the end of the current agricultural season, increased SLMP adoption without having affected any of the constraints faced by the farmers – especially their financial constraints. In other words, the key question is whether our instrument violated the 2SLS model's exclusion restriction by affecting the key outcome variable of interest, agricultural revenues at endline, via mechanisms other than just via increased SLMP usage.

In Section 4.2.1 we provide (suggestive) evidence that indeed offering future compensation conditional on current usage did not appreciably affect farmers' budget constraints during the current agricultural season. In Section 4.2.2 we turn to estimating the short-run impacts on agricultural productivity, revenues, and livelihoods.

4.2.1 Future Payments and Current Budget Constraints

Payments were not disbursed until at least seven months after the end of the 2019 agricultural season (see Figure 2). Still, the prospect of future payments may have affected endline agricultural revenues via mechanism other than just via increased SLMP take-up. For this to be the case, two conditions need to have been met. First, the prospect of future payments should have facilitated access to credit, allowing farmers to borrow against these future cash transfers. Second, they should have invested these funds to optimize their production process – by acquiring more land, by hiring more labor, and/ or by purchasing more inputs such as chemical fertilizer and pesticides. We now explore the relevance of each of these two conditions in turn.

We provide two pieces of evidence that farmers did not borrow against their future payments. First, access to credit is typically very poor among farmers in Burkina Faso. According to the 2017 Global Findex Database collected by the World Bank (Demirgüç-Kunt *et al.*, 2018), only 12% of the Burkinabé population borrowed from formal financial institutions, and only 6% of the Burkinabé did so for agricultural purposes. In addition – and contrary to conceived wisdom – Adjognon *et al.* (2017) document that even informal credit use is extremely low in Sub-Saharan Africa (across credit type, country, crop and farm size categories), and also that farmers primarily finance modern input purchases with cash from nonfarm activities and crop sales.

Our second piece of evidence on the implausibility of the access to credit channel is based on evaluating whether treatment households invested more in either alternative sources of income or in acquiring productive assets. In Table 6 we present the regression results aimed at detecting treatment differences in either non-agricultural sources of income, or in livestock revenues. Livestock is an asset farmers are especially likely to channel their funds to for two reasons: as a potentially productive asset (including the production of meat and milk; Wouterse and Taylor, 2008; Balboni *et al.*, 2021), and also to diversify risk (Fafchamps *et al.*, 1998; Carter and Lybbert, 2012; Janzen and Carter, 2019).

The results of this test are presented in Table 6. To mitigate the impact of outliers and to avoid having to drop those farmers from the analysis with no livestock or non-agricultural income, the dependent variables in this table are the inverse hyperbolic sine (IHS) transformations of the revenues of livestock and non-agricultural sources of income. Using IHS implies that the treatment estimates are semi-elasticities; the coefficients presented can thus be interpreted as percentage changes (Burbidge *et al.*, 1988; Bellemare and Wichman, 2020).[13] As is clear from Table 6, we do not find any evidence that treat-

[13]Taking the natural logarithm is the standard way of reducing the impact of outliers on coefficient estimates, and the treatment coefficients can then be interpreted as percentage changes. However, the logarithmic transformation results in all observations being dropped from the analysis that have a zero value for the variable of interest. The IHS of a variable with value z equals $\ln(z + \sqrt{z^2 + 1})$. Because essentially the IHS transformation boils down to shifting up the $ln(z)$ function by a constant (equal to $ln(2)$) for values of z that are not too close to zero, the elasticities generated by the two functions are very similar as well (Bellemare and Wichman, 2020; Aihounton and Henningsen, 2021). We therefore apply the IHS transformation in all models in which either livestock income or non-agricultural income is the dependent variable; see Tables 8 and 9 as well as Figure 4. We also apply the IHS transformation to one other variable for which many observations have a zero value – hired labor; see Table 7. Because

ment farmers were more likely (or more able) to invest in these alternative sources of income than the farmers in the control group.

Table 6: Intention-to-treat effects of conditional payments on income from livestock and non-agricultural activities.

	IHS Income	
	Livestock	Non-agricultural
	(1)	(2)
Payment treatment	0.010	0.022
	(0.110)	(0.402)
R^2	0.142	0.105
Observations	1231	1549
Adjusted R2	0.129	0.094
Baseline outcome	Yes	Yes
Covariates	Yes	Yes
Region FE-s	Yes	Yes
Control mean	9.915	5.313
Control std.dev.	4.283	5.931
Unit	IHS(FCFA)	IHS(FCFA)

Notes: Both models are estimated using OLS. Standard errors, presented in parentheses, are clustered at the commune level. To control for outliers and because of the relatively high incidence of participants without livestock and/or non-agricultural sources of income, the dependent variables are transformed by applying the inverse hyperbolic sine (IHS) function. The treatment coefficients can thus be interpreted as percentage changes.For the vector of covariates, see Table 5.

Next, we assess whether the agricultural input mix used by treatment farmers is markedly different from that used by the farmers in the control group. We can test this by estimating to what extent treatment status affected the use of agricultural inputs including land, labor and fertilizer. While the adoption of SLMPs may affect the optimal input mix, changes therein are expected to be relatively minor unless the prospect of future payments substantially reduced the farmers' current budget constraints. The results of this test are presented in Table 7.

Overall, we find that the use of inputs is not markedly different between households in the treatment groups compared to those in the control group. The impacts are typically small and statistically insignificant. We do not find any changes in the area cultivated, the method of sowing, the number of household members who worked on the plots, the total cost of hired labour, or in the amount of chemical or organic fertilizers; see columns

our explanatory variable of interest (T in equation (1), with coefficient τ) is a dummy variable, the treatment's percentage impact is approximated by $e^{\tau} - 1 \approx \tau$.

Table 7: The impact of treatment status on agricultural input use.

	Cultivated area (1)	# Manually sowed plots (2)	Household labor (3)	IHS transf. of hired labor cost (4)	Use chem. fertizer (5)	Use org. fertilizer (6)	Use pesticides (7)	Input index (8)
Payment treatment	0.106	0.023	-0.702	-0.439	22.671	150.513	3.447	0.047
	(0.082)	(0.050)	(0.573)	(0.399)	(27.461)	(201.122)	(1.223)	(0.037)
Constant	0.465	1.015	-5.050	1.516	12.809	2386.687	25.741	-0.382
	(0.886)	(0.703)	(5.161)	(4.716)	(333.630)	(1999.779)	(12.963)	(0.402)
Observations	1574	1574	1545	1574	1574	1574	1574	1574
Adjusted R^2	0.925	0.762	0.144	0.242	0.294	0.178	0.207	0.522
Baseline outcome	Yes	Yes	No	No	No	No	No	No
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region FE-s	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	4.874	1.641	3.634	6.089	161.010	1141.435	7.161	0.004
Control std.dev.	4.753	0.825	5.187	5.265	265.891	2173.358	10.952	0.550
Effect size (in std.dev.)	0.022	0.028	-0.135	-0.083	0.085	0.069	0.315	0.085
Unit	Hectare	Share	# HH member	IHS(FCFA)	Kilogram	Kilogram	Liter	Std.dev.

Notes: All columns are estimated using OLS. Standard errors, presented in parentheses, are clustered at the commune level. As not all farmers use hired labor, expenditures on hired labor are transformed using the inverse hyperbolic sine function (IHS). The treatment estimates can thus be interpreted as percentage changes. For the vector of covariates, see Table 5. The input index is calculated as the unweighted average of the normalized values of the inputs in columns (1)-(7) of this table (Kling *et al.*, 2007).

(1)-(6) of Table 7.[14]

While most of the changes in input use are relatively small and statistically insignificant, this does not hold for pesticides use. As shown in column (7) of Table 7, having been offered conditional payments increased the usage of pesticides by almost 50 percent (from, on average, 7.2 liters in the control group to 10.6 liters in the treatment group). Based on a national average price of \$7 per liter of pesticides (or 4000 FCFA; USDA, 2017), this would imply an increase in average expenditures of about \$25. This increase in usage may be the result of some of the actors in the farmer's supply chain being willing to extend credit, because of the prospect of the farmer receiving payments in the due time. If this is the mechanism, then it is surprising to see that the supply chain's willingness to offer credit was just limited to purchasing pesticides and not, for example, chemical fertilizers. An alternative explanation may be that the change in practices applied increased the marginal productivity of some inputs – *in casu* pesticides –, resulting in an

[14] Although the coefficient on hired labor is not statistically significant, its point estimate implies that the prospect of receiving future payments results in a $e^{-0.44} - 1 \approx 35.3\%$ decrease in the amount of money spent on hired labour ($\approx$ USD\$ 10). This seems like a large decrease, but it is inflated by the large share of farmers who are not using hired labour and the concave shape of the inverse hyperbolic function. These insights are reinforced by our analyses of the differences in the share of farmers employing hired labor and in the amount spent; see columns (1) and (2) of Table A9. Neither of these shares differs between the payment treatment and the control groups.

increase in the farmers' willingness to purchase those inputs.

Overall, the results presented in Tables 6 and 7 suggest that the prospect of receiving conditional payments in the future did not result in a large and instantaneous relaxation of the treatment farmers' budget constraints during the current agricultural season. Thus it is unlikely that the treatment affected agricultural productivity via mechanisms other than increased SLMP usage. This conclusion is reinforced by the results presented in column (8) of Table 7, where we estimate the impact of treatment status on an index of input use, constructed by averaging the normalized usages of each of our seven input variables.[15] We do not find any impact of having been offered conditional payments on this index: the point estimates indicates a change in the index of just 0.047 standard deviation.

4.2.2 The Impact of SLMP Usage on Agricultural Productivity and Farmers' Livelihood Outcomes

Having established that it is not very likely that the prospect of receiving conditional payments affected agricultural outcomes via mechanisms other than via the increase in SLMP take-up, we now employ our two stage least squares model (see equations (2) and (3) in Section 3.1) to estimate the short-run impact of SLMP adoption on farm income and on the agricultural productivity of some of Burkina Faso's most important crops. Using the treatment status of farmers to instrument SLMPs adoption, the first stage regression analyses have already been presented in column (1) of Table 5. We now focus our attention on the second stage results presented in Table 8.[16]

Column (1) of Table 8 presents our estimate of the marginal impact of SLMP usage on farmers' agricultural income. We find that having one additional SLMP in place increases the adopter's agricultural revenues by almost 40%. Taking the average agricultural pro-

[15] The index is created by standardizing each of the seven inputs using the means and standard deviations from the control group, and subsequently calculating the unweighted average. Following Kling *et al.* (2007) we do so to reduce the number of statistical tests (addressing multiple hypothesis testing) and to test the overall effect of payments on input use. The other indices presented in the remainder of this paper are constructed in essentially the same fashion (see Tables 8, 10, 11, and 12); the only difference is that in some instances some of the constituent variables needed to be recoded to realign their ordinal interpretation with those of the other variables that are included the index.

[16] For the intention-to-treat impact estimates, see Table A6.

Table 8: The impact of SLMP usage on farmers' agricultural productivity and revenues.

	IHS Transfromed Income	Productivity				
	Agriculture	Maize	Millet	Sorghum	Cowpea	Index
	(1)	(2)	(3)	(4)	(5)	(6)
# SLMPs	0.402	320.018	379.850	678.756	86.331	0.482
	(0.241)	(232.197)	(169.921)	(405.474)	(64.283)	(0.217)
Observations	1532	994	721	897	371	1467
R^2	0.192	-0.013	-0.190	-0.802	0.083	-0.390
Adjusted R^2	0.182	-0.040	-0.235	-0.855	0.016	-0.414
Baseline outcome	Yes	No	No	No	No	No
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Region FE-s	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	13.162	1151.229	717.798	745.729	289.560	-0.074
Control std.dev.	1.122	1213.386	933.566	726.059	411.660	0.759
Effect size (in std. dev.)	0.358	0.264	0.407	0.935	0.210	0.635
Unit	IHS(FCFA)	kg/ha	kg/ha	kg/ha	kg/ha	Std.dev.

Notes: All columns are estimated using two-stage least squares regression. Standard errors, presented in parentheses, are clustered at the commune level. The dependent variable in column (1) is the inverse hyperbolic sine of agricultural income; the coefficient can thus be interpreted as a percentage change. For the vector of covariates, see Table 5. The productivity index is calculated as the unweighted average of the normalized productivities of the four crops in columns (2)–(5) (Kling *et al.*, 2007).

duction value of FCFA 468,139 (or ≈ $USD 790) in the control group, this percentage point difference amounts to $USD 316 (or 0.35 standard deviations). Our estimates are in line with the results of earlier studies that estimate the production gains from specific practices. BenYishay and Mobarak (2018) find that in the second year of their study in Uganda, pit planting had increased agricultural productivity by 19%, and also that composting had increased productivity by 105%. Similarly, Takahashi *et al.* (2019) report that the adoption of improved rice management techniques in Cote d'Ivoire resulted in a 46% increase in rice yields in the first year since adoption. Our study confirms these studies' insights that SLMPs are not just likely to enhance productivity in the longer run, but in the shorter run as well.

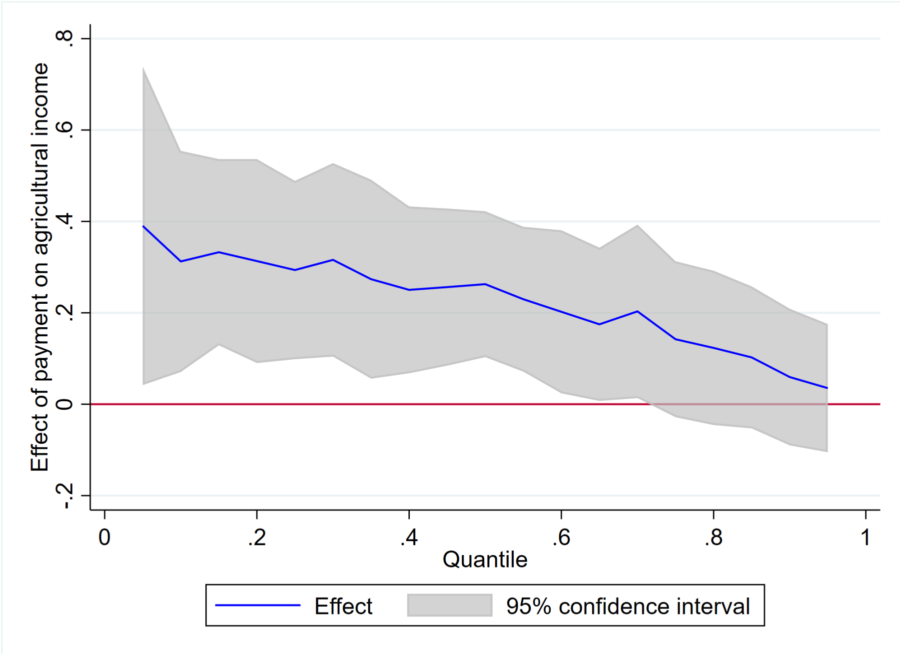
In the remaining columns of Table 8 we present the marginal impact of SLMP adoption on productivity of Burkina Faso's four key crops (see columns (2)–(5)) as well as on an agricultural productivity index thereof (see column (6)). We show the productivity impact of SLMPs on Burkina Faso's four most important crops: maize, millet, sorghum, and cowpea (FAO, 2021). We find sizeable productivity impacts of SLMPS adoption on all crops, and also on the overall productivity index. Yield increases range between 86 kg/ha (for cowpea, 0.21 standard deviation) and 678 kg/ha (for sorghum, 0.94 standard deviation), although only the yield responses of millet and sorghum are measured with sufficient precision to be significant. Overall, we find that adopting an additional SLMP raises productivity by almost half a standard deviation; see column (6).

4.3 Heterogeneous Effects on Agricultural Livelihood

Having documented that adopting an additional sustainable land management practice increases agricultural productivity and income even in the short run, we explore the importance of impact heterogeneity. Are there any subgroups among the peer farmers who benefited more than proportionally from the adoption of SLMPS? Or possibly more important, are there subgroups for whom SLMP adoption resulted in a decrease in agricultural productivity? To answer these questions, we first compare the distribution of the (IHS transformed) agricultural income between the different groups in a series of quantile regressions (Koenker and Hallock, 2001; Abadie *et al.*, 2002; Koenker, 2005). We plot the estimated effects and the corresponding 95% confidence interval for each quantile in Figure 4. The plot shows that the effects are largest for farmers at the lower end of the distribution (starting from an impact of 0.4, roughly a 40% increase in agricultural revenues), and also that the impact becomes smaller when moving through the various quantiles until they become insignificant at the 75$^{\text{th}}$ quantile.

Having documented that farmers with lower productivity tend to benefit more, we now proceed to use equation (1)'s intention-to-treat approach to test for the existence of heterogeneous treatment effects for some of the key farmer characteristics for which we have baseline data. Column (1) of Table 9 presents our baseline estimate of the *average* impact of the conditional payment treatment on agricultural revenues. In columns (2)–(6) of that Table we present the heterogeneous treatment effects, where each non-binary variable (like plot size and the asset index) was re-coded into a binary variables so it takes on a value of one for above-median values, and zero otherwise. As shown in columns (2)–(6), the benefits in terms of agricultural income are similar regardless of gender (column (2)), peer farmers' education (column (3)), wealth (columns (4) and (5)), or their number of degraded plots (column (6)). Based on the results presented in Figure 4 and in Table 9,

Figure 4: Quantile treatment effects on agricultural income.



Notes: The continuous line in this figure presents the intention-to-treat impact estimates on the inverse hyperbolic sine of agricultural income using quantile regressions; the vertical axis of this figure thus reflects percentage changes. The percentage change impacts are estimated at 5 percentile intervals, from the 5^{th} to the 95^{th} percentile. The shaded area around the continuous line is the 95% confidence interval around the corresponding point estimate, which is calculated using robust standard errors clustered at the commune level. We estimated the quantile treatment effects following Machado *et al.* (2011).

we thus conclude that adoption of SLMPs is beneficial even in the first year, and especially so for those farmers with the lowest levels of agricultural income.

5 Conditional Payments and Knowledge Dissemination

As shown in Section 4.1, we find that offering financial incentives resulted in increased SLMP adoption. One mechanism for this impact is that the prospect of receiving conditional payments directly improves the expected cost-benefit ratio of SLMP adoption. A second possible mechanism is that the treatment-induced increase in the profitability of SLMP adoption may have affected the effectiveness of the cascade training program. The extent to which information dissemination occurs in such programs is determined by both peer farmers' demand for and the contact farmers' supply of information. Did the increased (perceived) profitability of SLMP adoption result in an increase in peer farmers' demand for information on how to implement them? And if so, did this increased

Table 9: Testing for heterogeneous treatment effects on agricultural incomes.

	Average effect	Heterogenous treatment effects				
	No interaction	**Female**	**Education**	**Asset**	**Farmsize**	**Eroded plots**
	(1)	(2)	(3)	(4)	(5)	(6)
Payment treatment	0.225	0.216	0.205	0.256	0.244	0.238
	(0.089)	(0.097)	(0.083)	(0.097)	(0.087)	(0.094)
Payment treatment × Covariate		0.085	0.082	-0.053	-0.060	-0.030
		(0.140)	(0.107)	(0.123)	(0.130)	(0.092)
Observations	1542	1542	1542	1542	1542	1542
Adjusted R^2	0.420	0.421	0.421	0.423	0.445	0.420
Wald-Test p($\beta_{Paym} + \beta_{Paym \times Covar.} = 0$)		0.031	0.045	0.097	0.143	0.066
Baseline outcome	No	No	No	No	No	No
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Region FE-s	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	-0.048	-0.048	-0.048	-0.048	-0.048	-0.048
Control std.dev.	0.779	0.779	0.779	0.779	0.779	0.779
Effect size (in std.dev.)	0.289	0.386	0.369	0.260	0.237	0.267
Unit	IHS(FCFA)	IHS(FCFA)	IHS(FCFA)	IHS(FCFA)	IHS(FCFA)	IHS(FCFA)

Notes: All columns are estimated using OLS regression. The outcome variable is inverse hyperbolic since transformed agricultural income, therefore coefficients can be interpreted as percentage changes. In columns (2)-(6), the coefficient of the interaction between the treatment indicator and the dimension of heterogeneity is presented. Standard errors clustered at the commune level are in parentheses. For the vector of covariates, see Table 5.

demand induce contact farmers to provide it?

We answer these questions by assessing the payments' impact on the knowledge transmission process in the cascade training scheme. We hypothesize that information about SLMP implementation is more valuable the higher the stakes associated with SLMP adoption, and hence that higher stakes increase both the demand for and supply of information. In the presence of conditional payments peer farmers' willingness to pay for information is higher, and if the market for information is efficient, more information will be exchanged. As a corollary and reminiscent of the Coase theorem, note that if the market for information is efficient, only the size of the surplus matters – not the initial allocation of the payment. Whether this is true or whether providing contact farmers with a direct financial stake in peer farmer adoption increases SLMP uptake, is an open question. In this section we thus test (i) whether the cascade training scheme is more effective if the adoption stakes are higher, and (ii) whether or not the flow of information can be improved further – for the same surplus – when giving contact farmers a direct financial stake in their peer farmers' adoption decisions. In the next two subsections we test each of these two hypotheses.

5.1 The Impact of Higher Adoption Stakes on Information Dissemination

In this subsection we test whether higher adoption stakes improve the exchange of knowledge and information in the cascade training program. We do so by combining observed behavior and survey evidence; see Table 10. The amount of information exchanged is the outcome of both demand and supply. Column (1) of Table 10 documents that offering conditional payments resulted in a 12 percentage points increase in the share of peer farmers having asked their contact farmers for SLMP advice (from 35% in the control group to 47% in the pooled treatment group). As shown in column (2), this higher demand resulted in more intensive information sharing: the share of peer farmers indicating that they frequently met with the contact farmer to discuss SLMP implementation is 13 percentage points higher in the treatment group than in the control group (about 55% in the pooled treatment group compared to 41% in the control group). Next, columns (3) and (4) of Table 10 indicate that the treatment-induced increase in the demand for information induced the contact farmers to increase their information supply, in two ways. Column (3) shows that contact farmers in the payment group had higher SLMP adoption rates than those in the control group (although this difference fails to be significant at conventional levels; $p = 0.11$). Since payments were offered conditional on peer farmer adoption, the adoption of more SLMPs by contact farmers in the payment communities plausibly reflects a stronger willingness to lead by example and/ or to use their own land as demonstration plots. And as shown in column (4), contact farmers in the payment communities are also found to have a higher propensity to actively engage in on-site monitoring and verification of whether and how peer farmers adopted the practices. Consequently, as shown in column (5), these treatment-induced increases in information supply and demand mitigated the importance of the lack of SLMP implementation know-how as a barrier to adoption. The share of peer farmers not having adopted SLMP because of a lack of knowledge is estimated to be 11 percentage points lower in the pooled treatment group than in the control group. Finally, upon combining all indicators into a single index, we again find a significant increase of 0.4 standard deviation; see column (6). We thus find that larger adoption surpluses (associated with offering financial incentives) increases the effectiveness of the cascade training program.

Table 10: Treatment effects on contact farmer's adoption and communication between farmers about SLMPs.

	PF asked for advice (1)	Farmers discuss SLMPs (2)	# SLMPs adopted by CF (3)	CF monitor PF adoption (4)	% of SLMPs not adopted due to lack of knowledge (5)	Knowl. exch. index (6)
Payment treatment	0.119	0.134	0.398	0.112	-0.110	0.270
	(0.063)	(0.061)	(0.245)	(0.063)	(0.033)	(0.095)
Observations	1573	1573	315	1573	1571	1574
Adjusted R^2	0.078	0.073	0.268	0.082	0.119	0.189
Baseline outcome	Yes	Yes	Yes	Yes	No	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Region FE-s	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	0.356	0.408	3.900	0.336	0.231	-0.000
Control std.dev.	0.479	0.492	1.746	0.473	0.263	0.656
Effect size (in std.dev.)	0.249	0.272	0.228	0.237	-0.416	0.412
Unit	share	share	#	share	share	Std.dev.

Notes: Measures of knowledge exchange include the observed number of SLMPs adopted by contact farmers, as well as a series of indicator variables on contact-peer farmer interaction which are equal to one if the frequency of interaction was at least once per month and zero otherwise. All columns are estimated using OLS. Standard errors clustered at the commune level are in parentheses. For the vector of covariates, see Table 5. Upon having recoded the knowledge barrier variable (such that higher values reflect barriers being less important), the knowledge exchange index is calculated as the unweighted average of the normalized values of the indicators presented in columns (1)–(5) of this table (Kling *et al.*, 2007).

5.2 The Impact of the Payment Structure on Information Dissemination and SLMP Uptake

We thus find that offering conditional payments improved exchange of SLMP adoption information between peer and contact farmers. The most plausible underlying mechanism is that the conditional payments increased the (expected) profitability of SLMP adoption, and thereby peer farmers' demand for information. In this subsection we analyze whether the effectiveness of the cascade training program can be further enhanced by explicitly incentivizing, for the same surplus, not just the demand for information but also its supply – by giving contact farmers a direct financial stake in their peer farmers' adoption decisions. We do so by testing whether the results regarding demand and supply, as documented in Table 10, differ between the peer and split payment treatments.

Whether information dissemination can be furthered even more by explicitly incentivizing the supply side is of obvious importance for the design of cascade training programs. However, it is also important from a more general perspective – whether it is necessary to continue to offer conditional adoption payments if, over time, farmers learn increasingly more about the beneficial consequences of SLMP adoption. The argument is as follows. As stated in the introduction, the perceived lack of private benefits is one adoption barrier; the lack of SLMP implementation know-how is another. As shown in Section 4.2.2, adoption of another SLMP is highly profitable, even in the first year of application. If more and more SLMPs are being adopted, farmers who have not adopted yet receive increasingly more signals about the productivity impacts of SLMP adoption in the short and in the longer run, and hence the perceived insufficient profitability barrier is likely to become less important over time. However, the information barrier may continue to remain important unless the expected profitability-induced increase in demand for information results in an increased willingness of contact farmers to share this information. Differently stated, if the market for information is efficient, it may still be important for the government to offer the cascade training program, but it would no longer be necessary to subsidize SLMP adoption, nor to provide explicit incentives for information dissemination by contact farmers.

Table 11 provides insight into whether, for the same surplus, the exchange of information is better when providing contact farmers with a direct financial stake in their peer farmers' adoption. This table repeats the analysis of Table 10 by comparing the knowledge demand and supply indicators between the two conditional cash sub-groups, the peer and the split payment ones. These two treatments only differ in how the payment is divided between the peer and the contact farmer, so that the size of the surplus remains the same. Column (1) of Table 11 shows that giving contact farmers a direct financial stake in their peers' adoption does not result in significantly more practices being adopted by the peer farmers. We also do not find that the demand for information (column (2)), the frequency of interaction (column (3)), or the contact farmers' efforts to disseminate knowledge (columns (4) and (5)) are significantly different between the peer and split payment group. And in line with the lack of significant differences in columns (1)-(5), we also find no difference in neither the share of peer farmers reporting lack of knowledge as a barrier of adoption across the two groups (in column (6)), nor in the overall knowledge dissemination index (see column (7)). Therefore adoption of the SLMPs and knowledge dissemination do not depend on how the financial payment for adoption is allocated between the peer and the contact farmer.

Table 11: The impact of offering direct financial incentives for information dissemination to the contact farmers.

	# SLMPs adopt. by PFs (1)	PF asked for advice (2)	Farmers discuss SLMPs (3)	# SLMPs adopted by CF (4)	CF monitor PF adoption (5)	% of SLMPs not adopted due to lack of knowledge (6)	Knowl. exch. index (7)
Split payment	0.048	0.065	0.031	0.501	0.074	-0.024	0.134
	(0.274)	(0.068)	(0.073)	(0.320)	(0.093)	(0.040)	(0.101)
Observations	978	978	978	195	978	976	978
Adjusted R^2	0.342	0.091	0.124	0.345	0.117	0.166	0.255
Baseline outcome	Yes	Yes	Yes	Yes	Yes	No	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region FE-s	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	3.307	0.435	0.521	4.061	0.415	0.136	0.237
Control std.dev.	1.925	0.496	0.500	1.963	0.493	0.218	0.674
Effect size (in std.dev.)	0.025	0.131	0.062	0.255	0.150	-0.112	0.199
Unit	#	share	share	#	share	share	Std.dev.

Notes: All columns are estimated using OLS regression. Standard errors clustered at the commune level are in parentheses. For the vector of covariates, see Table 5. Upon having recoded the knowledge barrier variable (such that higher values reflect barriers being less important), the knowledge exchange index is calculated as the unweighted average of the normalized values of the indicators presented in columns (2)–(6) of this table (Kling *et al.*, 2007).

So we find that offering financial incentives was equally effective in the peer and split payment groups. Providing contact farmers with a direct financial stake in their peer farmers' SLMP adoption decisions does not affect the amount of effort the contact farmer provides in disseminating information on and in assisting the implementation of the sustainable land management technologies, and also the outcomes in terms of number of SLMPs adopted and of knowledge exchanged are independent of the payment's initial allocation. We consider two explanations to better understand the irrelevance of the payment allocation between peer and contact farmers. One possible reason is what we will loosely refer to as "altruism" – the relationship between the contact and peer farmer is such that the contact farmer is indifferent who ends up receiving what share of the payment. This can indeed be altruism within or between families, but it can also be more mechanical, in case the contact and peer farmer have a shared budget – for example if they are members of the same family. Indeed, no fewer than 43% of the peer farmers are kindred to their contact farmer, and hence the lack of a difference between the peer and split payment schemes may be due to the strength of (extended-) family ties (see

Table A7).

A second possible explanation is that contact farmers are only willing to put in effort if they themselves become better off too, but that markets for information are perfect in the Coasean sense – independent of how the payments are allocated, (unobservable) side payments ensure implementation of the efficient amount of information dissemination. If this is indeed the case, we expect non-kindred peer and contact farmers to behave similarly in the two payment treatments. If the markets for information are indeed efficient, the adoption of the SLMPs and knowledge dissemination should again be independent of how the financial payment for adoption is allocated between the peer and the contact farmer.

We test the relevance of both the altruism mechanism and the efficient market hypothesis by re-estimating Table 11 and allowing for heterogeneous treatment effects based on kinship. More specifically, the key covariates of interest in our regression model are the split payment group indicator, a kinship indicator (which is 1 if the peer and contact farmer in a dyad are family related, and 0 otherwise), as well as their interaction term. In this model, the omitted category is thus the group of peer and contact farmers in the peer payment treatment who are not kindred. The coefficients on each of the three dummies can then be interpreted as follows. First, the coefficient on kin captures whether the outcome variable of interest differs depending on whether the individuals in the peer-contact dyad are family, for those peer and contact farmers in the peer payment group. A positive coefficient would be suggestive evidence of the importance of altruism in the transfer of information. Second, the coefficient on the split payment dummy captures whether the initial payment allocation affects the outcome variable of interest if the peer and contact farmer in the dyad are not kin. A significant coefficient would provide evidence that the initial payment allocation matters, and that markets are not fully efficient. Third, the coefficient on the interaction term (split payment times kin) reflects whether the initial payment allocation still affects the information exchange even if both farmers in the dyad are kin.

The results of this analysis are presented in Table 12. We find that kinship matters at least to some extent, albeit that the outcomes are ambiguous; see the first row of

Table 12. If the peer and contact farmers in a dyad are kin, they meet more often to discuss SLMP implementation (9.5 percentage points, or 0.19 standard deviations; see column (3)). However, whether this is indeed indicative of an improved exchange of information is not obvious, as we also find that lack of knowledge is a barrier for the adoption of practices for a larger share of these farmers (3.5 percentage point, or 0.13 standard deviation; see column (6)). Next, in all six models the coefficients on both the split payment indicator as well as on the interaction term are statistically insignificant. The lack of significance of the coefficient on the split payment dummy suggests that indeed the markets for information are sufficiently efficient to not affect the final outcomes, and the lack of significance on the interaction term suggests that the initial payment allocation is by and large inconsequential for the flow of information among kindred dyads as well.

Table 12: Treatment effects of split payments compared to peer payment along family ties.

	# SLMPs adopt. by PFs (1)	PF asked for advice (2)	Farmers discuss SLMPs (3)	# SLMPs adopted by CF (4)	CF monitor PF adoption (5)	% of SLMPs not adopted due to lack of knowledge (6)	Knowl. exch. index (7)
Kin	-0.052	0.016	0.094	0.538	0.059	0.035	0.072
	(0.147)	(0.048)	(0.045)	(0.584)	(0.054)	(0.018)	(0.055)
Split payment	0.022	0.031	0.026	0.289	0.041	-0.008	0.099
	(0.326)	(0.067)	(0.075)	(0.878)	(0.104)	(0.041)	(0.099)
Split payment × Kin	0.198	0.072	0.005	0.220	0.072	-0.040	0.073
	(0.259)	(0.074)	(0.067)	(0.848)	(0.078)	(0.032)	(0.093)
Observations	978	978	978	195	978	976	978
R^2	0.317	0.110	0.141	0.439	0.135	0.191	0.280
Adjusted R^2	0.301	0.089	0.122	0.349	0.115	0.166	0.258
Baseline Outcome	Yes	Yes	Yes	Yes	Yes	No	No
Covariates Included	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
T-tests (p-values)							
– $\beta_{80/20} + \beta_{80/20\times\text{Kin}} = 0$	0.527	0.275	0.733	0.099	0.264	0.274	0.187
– $\beta_{80/20} = \beta_{\text{Kin}}$	0.813	0.822	0.397	0.694	0.851	0.317	0.804
– $\beta_{80/20\times\text{Kin}} = 0$		0.346	0.936	0.798	0.367	0.229	0.445
Control mean	2.784	0.356	0.408	3.900	0.336	0.257	0.257
Control std.dev.	1.670	0.479	0.492	1.746	0.473		
Effect size (in std.dev.)							
– $\beta_{80/20}$	0.013	0.064	0.052	0.166	0.086	-0.029	0.369
– $\beta_{80/20\times\text{Kin}}$	0.119	0.150	0.011	0.126	0.152	-0.151	0.273

Notes: All columns are estimated using OLS regression. In the regressions, the split payment treatment indicator is fully interacted with an indicator on whether the peer and contact farmers are kin. Standard errors clustered at the commune level are in parentheses. For the vector of covariates, see Table 5. Upon having recoded the knowledge barrier variable (such that higher values reflect barriers being less important), the knowledge exchange index is calculated as the unweighted average of the normalized values of the indicators presented in columns (2)–(6) of this table (Kling *et al.*, 2007).

6 Conclusions

Adoption of sustainable agricultural practices in arid Sub-Saharan Africa is hindered by limited knowledge about the practices as well as by the low (perceived) private profitability. Cascade training programs, in which some farmers – the so-called contact farmers – are trained by government extension workers about the benefits and usage of new agricultural techniques and who are subsequently asked to disseminate their newly acquired knowledge and expertise among fellow – or peer – farmers in their local social network, have been developed to overcome this information barrier.

In this paper we argue that in the context of sustainable land management practices, aimed at conserving soil and water to reduce the need for new land clearing, conditional adoption payments can help overcome both the perceived lack of profitability barrier as well as the information barrier. Offering compensation conditional on downstream adoption is likely to improve not only the new technology's perceived cost-benefit ratio, but also the transfer of the contact farmer's newly acquired knowledge and expertise to her peer farmers. Offering payments for downstream SLMP adoption increases the demand for knowledge and expertise, but it is an open question whether this will also translate in improved information transfer from the contact to the peer farmer.

We implemented a Randomized Controlled Trial in arid Burkina Faso to test to what extent offering cash transfers, to be paid out conditional on downstream SLMP adoption, are effective in inducing increased uptake, and also in improving knowledge dissemination. The contact farmers in all three treatment arms participated in a cascade training program; upon completion of the training they were asked to disseminate the newly acquired knowledge to peer farmers in their network. Our two treatments consisted of offering financial compensation based on SLMP adoption by the peer farmers in these treatment groups. The two treatments only differed in the initial allocation of the payment. In the one treatment arm the peer farmer received the full amount whereas in the other treatment the payment was split, 80-20, between the peer and the contact farmer.

We find that peer farmers adopted significantly more practices when there is a financial incentive to do so. Contact farmers also put in more effort to disseminate their information, and they also adopted more practices themselves (albeit significant only at $p = 0.11$)– presumably to lead by example, or because of field demonstration purposes. Interestingly we do not find that the size of these effects vary with how the payment is initially allocated between the peer and the contact farmer. In other words, we find evidence that larger adoption surpluses render the cascade training scheme more effective, but that there is no reason to also provide direct financial stakes to contact farmers to ensure improved information dissemination. Finally, our RCT also speaks to the short-run benefits and costs of SLMP adoption. We find that offering payments, conditional on SLMP adoption and to be disbursed after the end of the agricultural season, results in substantially higher productivity even in the first year of SLMP implementation. Because the payments were to be disbursed in the future, our intervention is unlikely to have substantially affected farmers' agricultural production constraints, and hence the difference in agricultural productivity plausibly reflects the impact of SLMP adoption on productivity and revenues.

Together, our results provide interesting new insights with respect to fostering the adoption of SLMPs – but probably also, more broadly, to agricultural development via technology diffusion. Our use of financial incentives can be defended on two grounds. First, the advertized SLMPs are (perceived) not to be privately profitable at least in the first few years of usage, while the global community benefits from the positive externalities generated by sustainable land use. Second, even if technologies are acknowledged to be profitable, financial incentives may still be necessary to ensure the active dissemination of knowledge and expertise by the contact farmers (see for example BenYishay and Mobarak (2018), Sseruyange and Bulte (2018), and Shikuku *et al.* (2019)). Our results suggest that conditional subsidies may still be indispensable in the short run, but not in the longer run. With continued SLMP diffusion, farmers receive more and more signals about the productivity impacts of SLMP adoption in the short and in the longer run. Hence, the perceived lack of profitability barrier is likely to become less important over time. However, we also expect the information barrier to become less important over time. This is because we find (i) that a larger surplus generated by SLMP adoption increases the effectiveness of our cascade training program, and (ii) that this increase is equally large independent of whether or not contact farmers are offered direct financial stakes in their peer farmers' adoption decisions. If, over time, the perceived profitability of SLMP adoption goes up, the perceived size of the surplus of SLMP adoption also increases, and contact farmers are predicted to increase their dissemination effort over time as well – even so in the absence of a direct financial stake.

While it is an open question to what extent the dynamics described above result in a socially optimal rate of SLMP adoption and dissemination, it is reassuring that two of the currently largest hindrances for the effectiveness of cascade training program, lack of perceived profitability and insufficient effort by contact farmers to disseminate their expertise, are likely to decrease with the continued implementation of cascade training programs.

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A Appendix

A.1 Project Material

Figure A1: Example of certificates handed out to peer farmers

(a) In control groups

NOTIFICATION POUR LA PROMOTION DES PRATIQUES DE GESTION DURABLE DES TERRES (GDTs)

Cette notification est attribuée, par le Programme d'Investissement Forestier (PIF) du Burkina Faso, au producteur collègue dont les informations d'identification suivent :

Nom complet : [redacted] Village : [redacted]
Commune : [redacted] Région : [redacted]

Liste des parcelle(s) géoréférencée(s) :

1. Nom : CHAMP BAS FOND
2. Nom : CHAMP DE MONTAGNE
3. Nom : CHAMP DE SABLE

pour confirmer sa sélection par le producteur relais, Mr/Mme [redacted], qui s'engage à lui fournir un appui technique pour la mise en œuvre des pratiques de Gestion Durable des Terres (GDTs) sur ces parcelles géoréférencées.

Délivrée le ***Signature du représentant du PIF***

Samedi 08 Juin 2019

(b) In split payment groups

NOTIFICATION POUR LA PROMOTION DES PRATIQUES DE GESTION DURABLE DES TERRES (GDTs)

Cette notification est attribuée, par le Programme d'Investissement Forestier (PIF) du Burkina Faso, au producteur collègue dont les informations d'identification suivent :

Nom complet : [redacted] Village : [redacted]
Commune : [redacted] Région : [redacted]

Liste des parcelles géoréférencées :

1. Nom : CHAMP AVANT LA RIVE
2. Nom : CHAMP APReS LA RIVE

pour confirmer sa sélection pour bénéficier des activités des bonnes pratiques de Gestion Durable des Terres (GDTs) du Programme d'Investissement Forestier (PIF).

Ce producteur collègue recevra un appui technique de la part de [redacted], son producteur relais, pour l'application des techniques de GDTs.

Le producteur collègue et son producteur relais bénéficieront d'une incitation financière de la part du PIF pour l'application des techniques de GDTs sur les parcelles géoréférencées du producteur collègue. Le PIF paiera :

- 30 000 FCFA pour l'adoption de 1 à 3 bonnes pratiques des GDTs ;
- 40 000 FCFA pour l'adoption de 4 à 6 bonnes pratiques des GDTs et
- 50 000 FCFA pour l'adoption de 7 à 9 bonnes pratiques des GDTs.

Remarque : Le producteur relais recevra **20%** de la somme et le producteur collègue recevra **80%** de la somme.

Délivrée le ***Signature du représentant du PIF***

Samedi 08 Juin 2019

Notes: Certificates that peer farmers have received for their participation in the experiment. All certificates show the name of the farmer, the village, commune, and region of residence, the name of the georeferenced agricultural plots where SLMP adoption would be verified at endline, and the name of the contact farmer. In payment communes, the certificate also details the conditions, structure, and allocation of payments. The certificate in the peer payment group is similar to that in the split payment group except for the last remark.

A.2 Verification of the SLMPs

Figure A2: Comparison of pit planting at the beginning and at the end of the agricultural season

(a) Implementation of pit planting

(b) Holes of pit planting at endline

Notes: The two figures show the how pit planting looks like at the beginning of the agricultural season and after harvest (at endline). The second figure shows that the holes are still visible around the crop remains at endline. Pit planting is highlighted separately from the other SLMPs because this is the only practices which is optimally implemented at the beginning of the agricultural season and which stays intact only for one agricultural season.

A.3 Descriptive statistics and balance

Table A1: Scores of the contact farmers on the SLMP knowledge tests.

	(1)		(2)		(3)		(4)		(5)	(6)	(7)	(8)	(9)	(10)
	Total		Conrol group		Peer payment		Split payment			Difference			Normalized difference	
Variable	N	Mean/SE	N	Mean/SE	N	Mean/SE	N	Mean/SE	(2)-(3)	(2)-(4)	(3)-(4)	(2)-(3)	(2)-(4)	(3)-(4)
Agronomy (in percent)	317	0.907	120	0.912	99	0.914	98	0.893	-0.002	0.019	0.021*	-0.028	0.207	0.256
		(0.005)		(0.009)		(0.008)		(0.008)						
Integrated crop and livestock management (in percent)	317	0.734	120	0.748	99	0.718	98	0.732	0.030	0.016	-0.014	0.207	0.113	-0.097
		(0.008)		(0.013)		(0.015)		(0.015)						
Agroforestry (in percent)	317	0.662	120	0.655	99	0.668	98	0.665	-0.012	-0.009	0.003	-0.093	-0.072	0.021
		(0.008)		(0.010)		(0.015)		(0.015)						
Total score (in percent)	317	0.770	120	0.775	99	0.769	98	0.765	0.006	0.010	0.005	0.073	0.141	0.053
		(0.005)		(0.006)		(0.009)		(0.009)						

Notes: Average raw knowledge scores of contact farmers in the whole sample and in the three treatment groups are presented in columns (1)-(4); standard deviations are presented in parentheses. Knowledge scores for each agronomic subsection are the share of correct answers within the specific section of the test. Columns (5)-(7) present the p-values for the treatment group indicators from regressing the characteristics on the treatment group indicators and region fixed-effects. Standard errors are clustered at the commune level. Columns (8)-(10) present the normalized differences for each of the test parts.

Table A2: Differential attrition rates between treatments.

	(1)		(2)		(3)		(4)		(5)	(6)	(7)	(8)	(9)	(10)
	Total		Conrol group		Peer payment		Split payment			Difference			Normalized difference	
Variable	N	Mean/SE	N	Mean/SE	N	Mean/SE	N	Mean/SE	(2)-(3)	(2)-(4)	(3)-(4)	(2)-(3)	(2)-(4)	(3)-(4)
Attrited (0/1)	1914	0.007	720	0.006	600	0.005	594	0.010	0.001	-0.005	-0.005	0.008	-0.052	-0.059
		(0.002)		(0.003)		(0.003)		(0.004)						

Notes: Columns (1)-(4) present the share of farmers in the whole sample as well as in each treatment group who were not surveyed at endline. Standard deviations are reported in parentheses. Columns (5)-(7) present the p-values for the treatment group indicators as obtained by regressing the characteristics on the treatment group indicators and region fixed-effects. Standard errors are clustered at the commune level. Columns (8)-(10) present the normalized differences.

Table A3: Differences in baseline characteristics peer and contact farmers.

	(1) Total		(2) Contact farmer		(3) Peer farmer		(4) T-test (P-value)	(5) Normalized difference
Variable	N/[Clusters]	Mean/SE	N/[Clusters]	Mean/SE	N/[Clusters]	Mean/SE	(2)-(3)	(2)-(3)
Age	1914	42.039	319	45.351	1595	41.377	0.000***	0.366
	[32]	(0.440)	[32]	(0.654)	[32]	(0.477)		
Female respondent (0/1)	1914	0.177	319	0.194	1595	0.173	0.216	0.056
	[32]	(0.017)	[32]	(0.019)	[32]	(0.018)		
Has some primary education (0/1)	1914	0.313	319	0.451	1595	0.285	0.000***	0.358
	[32]	(0.019)	[32]	(0.035)	[32]	(0.018)		
Adults in household	1914	11.935	319	13.135	1595	11.695	0.000***	0.209
	[32]	(0.501)	[32]	(0.566)	[32]	(0.508)		
Deprived house (0/1)	1914	0.845	319	0.784	1595	0.858	0.002***	-0.205
	[32]	(0.032)	[32]	(0.045)	[32]	(0.030)		
Asset index	1914	-0.000	319	0.678	1595	-0.136	0.000***	0.359
	[32]	(0.219)	[32]	(0.194)	[32]	(0.229)		
Association membership (0/1)	1914	0.688	319	0.840	1595	0.657	0.000***	0.395
	[32]	(0.034)	[32]	(0.031)	[32]	(0.036)		
Hired labor in previous agri. season (0/1)	1914	0.560	319	0.643	1595	0.544	0.000***	0.200
	[32]	(0.050)	[32]	(0.053)	[32]	(0.050)		
Number of plots under the control of the farmer	1914	1.742	319	1.956	1595	1.699	0.000***	0.302
	[32]	(0.069)	[32]	(0.089)	[32]	(0.068)		
Number of eroded plots	1914	2.522	319	2.868	1595	2.453	0.000***	0.281
	[32]	(0.106)	[32]	(0.155)	[32]	(0.100)		
Landholdings (ha)	1914	5.252	319	6.629	1595	4.976	0.000***	0.343
	[32]	(0.370)	[32]	(0.465)	[32]	(0.374)		
Number of SLMPs adopted at baseline	1914	2.531	319	3.439	1595	2.349	0.000***	0.738
	[32]	(0.147)	[32]	(0.177)	[32]	(0.145)		
Income from agricultural production (IHS transformed)	1914	12.895	319	13.294	1595	12.815	0.000***	0.177
	[32]	(0.290)	[32]	(0.332)	[32]	(0.284)		
Household has income from non-agricultural activities (0/1)	1914	0.536	319	0.621	1595	0.518	0.002***	0.205
	[32]	(0.027)	[32]	(0.036)	[32]	(0.027)		

Notes: Average values, for the total sample as well as for each of the two farmer types, are presented in columns (1)-(3); standard deviations are presented in parentheses. Column (4) presents the p-values for the farmer type indicator derived from a region fixed-effects model. Standard errors are clustered at the commune level. Column (5) presents the differences normalized by the sample variance.

Table A4: Baseline characteristics of the contact farmers in each of the three (sub-)treatment arms.

	(1) Total		(2) Conrol group		(3) Peer payment		(4) Split payment		(5) T-test (P-value)	(6)	(7)	(8) Normalized difference	(9)	(10)
Variable	N/[Clusters]	Mean/SE	N/[Clusters]	Mean/SE	N/[Clusters]	Mean/SE	N/[Clusters]	Mean/SE	(2)-(3)	(2)-(4)	(3)-(4)	(2)-(3)	(2)-(4)	(3)-(4)
Age	319	45.351	120	45.158	100	44.540	99	46.404	0.654	0.415	0.186	0.062	-0.124	-0.184
	[32]	(0.654)	[12]	(1.071)	[10]	(0.893)	[10]	(1.435)						
Female respondent (0/1)	319	0.194	120	0.175	100	0.170	99	0.242	0.929	0.107	0.094*	0.013	-0.166	-0.179
	[32]	(0.019)	[12]	(0.028)	[10]	(0.037)	[10]	(0.032)						
Has some primary education (0/1)	319	0.451	120	0.450	100	0.540	99	0.364	0.149	0.332	0.022**	-0.180	0.175	0.353
	[32]	(0.035)	[12]	(0.060)	[10]	(0.045)	[10]	(0.070)						
Adults in household	319	13.135	120	13.675	100	12.450	99	13.172	0.395	0.730	0.509	0.160	0.069	-0.096
	[32]	(0.566)	[12]	(1.038)	[10]	(0.933)	[10]	(0.989)						
Deprived house (0/1)	319	0.784	120	0.833	100	0.630	99	0.879	0.031**	0.545	0.017**	0.463	-0.128	-0.576
	[32]	(0.045)	[12]	(0.066)	[10]	(0.087)	[10]	(0.068)						
Asset index	319	0.678	120	0.788	100	0.725	99	0.496	0.672	0.313	0.477	0.032	0.140	0.108
	[32]	(0.194)	[12]	(0.289)	[10]	(0.394)	[10]	(0.361)						
Association membership (0/1)	319	0.840	120	0.800	100	0.870	99	0.859	0.330	0.405	0.863	-0.187	-0.154	0.033
	[32]	(0.031)	[12]	(0.055)	[10]	(0.056)	[10]	(0.053)						
Hired labor in previous agri. season (0/1)	319	0.643	120	0.675	100	0.670	99	0.576	0.998	0.330	0.308	0.011	0.205	0.194
	[32]	(0.053)	[12]	(0.096)	[10]	(0.090)	[10]	(0.092)						
Number of plots under the control of the farmer	319	1.956	120	2.050	100	1.810	99	1.990	0.215	0.844	0.337	0.247	0.063	-0.189
	[32]	(0.089)	[12]	(0.153)	[10]	(0.138)	[10]	(0.173)						
Number of eroded plots	319	2.868	120	3.050	100	2.640	99	2.879	0.272	0.700	0.389	0.230	0.102	-0.144
	[32]	(0.155)	[12]	(0.266)	[10]	(0.283)	[10]	(0.268)						
Landholdings (ha)	319	6.629	120	6.333	100	6.198	99	7.423	0.810	0.414	0.255	0.025	-0.196	-0.204
	[32]	(0.465)	[12]	(0.586)	[10]	(0.516)	[10]	(1.236)						
Number of SLMPs adopted at baseline	319	3.439	120	3.508	100	3.160	99	3.636	0.304	0.766	0.208	0.223	-0.087	-0.292
	[32]	(0.177)	[12]	(0.238)	[10]	(0.373)	[10]	(0.333)						
Income from agricultural production (IHS transformed)	319	13.294	120	13.640	100	13.483	99	12.684	0.436	0.285	0.406	0.107	0.322	0.270
	[32]	(0.332)	[12]	(0.188)	[10]	(0.150)	[10]	(1.045)						
Household has income from non-agricultural activities (0/1)	319	0.621	120	0.642	100	0.580	99	0.636	0.288	0.964	0.586	0.126	0.011	-0.115
	[32]	(0.036)	[12]	(0.047)	[10]	(0.053)	[10]	(0.091)						

Notes: Average values, for the total sample as well as for each of the three sub-samples, are presented in columns (1)-(4); standard deviations are presented in parentheses. Columns (5)-(7) present the p-values obtained by means of region fixed-effects regressions. Standard errors are clustered at the commune level. Columns (8)-(10) present the normalized differences between each of the treatment and the control group.

Table A5: Determinants of the number of SLMPS at baseline.

		Agronomy SLMPs			Int. Crop & Livestock Manag. SLMPs			Agroforestry SLMPs		
	# SLMPs adopted	Zai	Heap and pit composting	Stone and earth bounds	Mowing and cons. of natu. fodder	Forage crop cultivation	Use of agr. and wood by-products	Controlled clearing	Assisted natural regeneration	Living hedges
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Age	0.017***	0.002***	0.004***	0.003***	0.002**	0.001	0.000	-0.001	0.003***	0.002***
	(0.003)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Female respondent (0/1)	-0.224*	-0.003	0.037	-0.059*	-0.022	0.005	-0.027	-0.031	-0.068***	-0.056***
	(0.110)	(0.016)	(0.036)	(0.034)	(0.029)	(0.012)	(0.042)	(0.039)	(0.022)	(0.019)
Has some primary education (0/1)	0.155**	0.042**	0.026	0.049**	0.021	0.013	-0.013	-0.005	0.005	0.016
	(0.068)	(0.020)	(0.025)	(0.018)	(0.024)	(0.014)	(0.025)	(0.033)	(0.025)	(0.011)
Adults in household	-0.000	-0.001	0.002	-0.000	0.002	-0.001	0.004*	-0.005	0.001	-0.002
	(0.005)	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)	(0.003)	(0.002)	(0.001)
Asset index	0.132***	-0.004	0.019**	0.041***	0.026***	0.012***	0.019**	0.009	0.000	0.010***
	(0.017)	(0.005)	(0.008)	(0.005)	(0.007)	(0.004)	(0.007)	(0.009)	(0.007)	(0.003)
Hired labor in previous agri. season (0/1)	0.148*	-0.011	0.050	0.021	0.014	-0.023	0.015	0.018	0.049	0.014
	(0.075)	(0.031)	(0.036)	(0.026)	(0.024)	(0.019)	(0.039)	(0.039)	(0.034)	(0.015)
Number of plots under the control of the farmer	0.352***	0.048	0.189***	0.098***	-0.035	-0.016	0.057	-0.082*	0.076**	0.017
	(0.066)	(0.030)	(0.025)	(0.030)	(0.028)	(0.025)	(0.042)	(0.044)	(0.034)	(0.011)
Number of eroded plots	-0.121**	-0.005	-0.076***	-0.039**	0.002	0.002	-0.013	0.047*	-0.027	-0.013*
	(0.046)	(0.007)	(0.017)	(0.018)	(0.014)	(0.012)	(0.020)	(0.026)	(0.020)	(0.007)
Landholdings (ha)	0.025***	-0.005*	0.000	0.006**	0.002	0.005**	0.005	0.003	0.007	0.002
	(0.009)	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)	(0.004)	(0.006)	(0.005)	(0.001)
Household has income from non-agricultural activities (0/1)	0.020	0.014	-0.015	0.022	0.034	-0.007	-0.075*	0.022	0.027	-0.002
	(0.075)	(0.014)	(0.023)	(0.025)	(0.023)	(0.019)	(0.039)	(0.031)	(0.032)	(0.013)
Score total (en pct) - Apres	1.659**	-0.096	0.441**	0.609**	-0.366	0.247**	1.059***	-0.567	0.378	-0.046
	(0.720)	(0.137)	(0.173)	(0.250)	(0.249)	(0.106)	(0.312)	(0.475)	(0.428)	(0.084)
Number of adopters in commune	-0.063	0.012	-0.005	-0.030*	-0.037	0.014	-0.041	-0.000	0.009	0.014
	(0.055)	(0.012)	(0.021)	(0.017)	(0.024)	(0.017)	(0.031)	(0.043)	(0.032)	(0.009)
Number of adopters in commune^2	0.001*	-0.000	0.000	0.000**	0.000*	-0.000	0.001	0.000	0.000	-0.000
	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Constant	-0.562	-0.200	-0.621	0.232	0.834	-0.416	0.005	0.617	-0.737	-0.275
	(1.221)	(0.209)	(0.497)	(0.456)	(0.500)	(0.351)	(0.702)	(0.976)	(0.729)	(0.178)
Observations	1889	1889	1889	1889	1889	1889	1889	1889	1889	1889
Observations	1889	1889	1889	1889	1889	1889	1889	1889	1889	1889
Adjusted R^2	0.350	0.098	0.130	0.297	0.058	0.042	0.268	0.082	0.097	0.053
Mean	2.537	0.053	0.308	0.503	0.206	0.070	0.534	0.375	0.419	0.067
Std. dev.	1.477	0.224	0.462	0.500	0.405	0.256	0.499	0.484	0.493	0.251

Notes: All columns are estimated using OLS regression where the baseline number of SLMPs or the indicator of SLMP adoption at baseline is regressed on farmers' baseline characteristics and region fixed effects. Standard errors, presented in parentheses, are clustered at the commune level. The regressions are run using the data of all farmers in the sample (i.e., both peer and contact farmers), with, in total, 1889 observations. The last two rows of the table present the mean and standard deviation of the outcome variables.

A.4 Supplementary results

Table A6: The impact of treatment status on agricultural revenues and individual crop yields.

	IHS Transfromed Income	Productivity				
	Agriculture	**Maize**	**Millet**	**Sorghum**	**Cowpea**	**Index**
	(1)	(2)	(3)	(4)	(5)	(6)
Payment treatment	0.221**	181.379	197.541***	347.612***	67.253	0.255***
	(0.089)	(136.579)	(67.861)	(82.401)	(50.290)	(0.071)
Observations	1532	994	721	897	371	1467
Observations	1532	994	721	897	371	1467
Adjusted R^2	0.420	0.020	0.032	0.014	0.058	0.015
Baseline	Yes	No	No	No	No	No
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Region FE-s	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	467494.812	1151.229	717.798	745.729	289.560	-0.074
Control std.dev.	540515.154	1213.386	933.566	726.059	411.660	0.759
Unit	FCFA	kg/ha	kg/ha	kg/ha	kg/ha	Std.dev.

Notes: Column (1) reports the OLS estimate of the intention-to-treat effect on the inverse hyperbolic sine of agriculture income; the coefficient can thus be interpreted as a percentage change. Column (2)-(5) report similar intention-to-treat effect estimates on crop productivity calculated as the ratio of total quantity produced and of total area where the crop was cultivated. Column (6) reports the intention-to-treat effect estimate on the productivity index which is calculated as the unweighted average of the normalized productivities of the four crops (Kling *et al.*, 2007). The samples in columns (2)-(6) consist of peer farmers who produced the crops. Standard errors, clustered at the commune level, are presented in parentheses. For the vector of covariates, see Table 5.

Table A7: Uncovering the types of ties between peer and contact farmers.

	Control group	**Peer payment group**	**Split payment group**	**Total**
Family	0.472	0.401	0.417	0.433
	(0.500)	(0.491)	(0.494)	(0.496)
Neighbor	0.190	0.220	0.258	0.220
	(0.393)	(0.415)	(0.438)	(0.415)
Friend	0.239	0.275	0.237	0.250
	(0.427)	(0.447)	(0.426)	(0.433)
Village association/cooperative	0.0790	0.0842	0.0757	0.0796
	(0.270)	(0.278)	(0.265)	(0.271)
Other	0.0202	0.0200	0.0123	0.0177
	(0.141)	(0.140)	(0.110)	(0.132)
Observations	1583			

Notes: The table presents the frequency of each type of ties between peer and contact farmers for each treatment group and for the whole sample. The numbers in the table represent the share of peer farmers within the group. Standard deviations are presented in parentheses. The total sample of peer farmers is 1583 in the table.

Table A8: Intended Expenditures of the Payments

	What are the main intended investments?				
Dependent Variable:	**Ag. Inputs**	**School Fees**	**Livestock Production**	**Food**	**Miscellaneous Goods**
	(1)	(2)	(3)	(4)	(5)
Paiement 80-20	-0.138***	0.006	-0.052	0.068	0.010
	(0.043)	(0.054)	(0.067)	(0.067)	(0.038)
Control Mean	0.702	0.459	0.421	0.298	0.308
Observations	821	821	821	821	821
Survey Month FEs	Yes	Yes	Yes	Yes	Yes

Notes: This Table shows results of testing for differential (intended) use of payments among peer farmers across treatment groups. We regress binary indicators that are one, in case a farmer cited the respective expense group at the time of the payment disbursement and zero otherwise. All regressions include survey month- and region-fixed effects. Standard errors are clustered at the commune-level. The disbursement of the payments took place between July and November 2019. Farmers were asked to name up to three expenditure groups they intended to use the payments for. In the last column, we combine expenses on clothes, cosmetics, maintenance of the mode of transport, other family expenses as well as charitable contribution.

Table A9: Treatment effects on hired labor use and expenditure.

	Share of HH hiring labor (1)	**Conditional IHS. transf. of hired labor expenditure** (2)
Payment treatment	-0.041	-0.027
	(0.037)	(0.097)
Observations	1574	846
Observations	1574	846
Adjusted R^2	0.242	0.165
Baseline Outcome	No	No
Covariates Included	Yes	Yes
Region fixed effects	Yes	Yes
Control mean	0.577	10.549
Control std.dev.	0.494	0.950
Unit	Share	FCFA

Notes: Column (1) presents the intention-to-treat estimates of payments on the share of peer farmers who hired labour during the *current* (2019) agricultural season via OLS regression. Column (2) presents the intention-to-treat effects on the inverse hyperbolic sine of expenditures on hired labour conditional of having hired labour in the *current* agricultural season; the coefficient can thus be interpreted as a percentage change. Standard errors, clustered at the commune level, are presented in parentheses. For the vector of covariates, see Table 5.

A.5 Robustness checks

We implement three types of robustness checks. We first test whether our results are robust to re-estimating our OLS regressions using probit and count models. Next, we adjust our statistical inferences of the main treatment effect estimates to take advantage of the randomized treatment assignment in the experiment. In the final step, we adjust our statistical inferences for multiple hypothesis testing.

A.5.1 Non-linear models for count and binary outcomes

We first evaluate the extent to which our intention-to-treat impact estimates are sensitive to relaxing the continuous outcome variable assumption. This assumption of the OLS regression does not hold when our outcome variable is the number of practices adopted by farmers, or when it is a binary variable. We use negative binomial regression for count variables and probit regression for binary outcome variables to re-estimate the effects presented in Tables 5, 7, 10 and 11. The results are presented in Tables A10–A13; coefficients presented in these tables are marginal effects.

Overall, the results of the negative binomial and probit regression models are very similar to those obtained using OLS – in terms of signs, significance, and size. The results in Tables A11 and A12 are qualitatively (and even quantitatively) identical to those in Tables 7 and 10, respectively. Comparing Tables 5 and A10, the only difference between the two is for the impact on stone and earth bund construction (see the fourth column in both tables); while the coefficient for this practice remains by and large unchanged in size, it just fails to be statistically significant in Table A10 (with $p = 0.102$). The only substantive difference caused by using probit or negative binomial estimation is with respect to the role of leading by example in the comparison between the two payment sub-treatments; compare column (4) in Tables 11 and A13. While using negative binomial estimation reduces the size of the coefficient on the spilt payment indicator in Table A13, the difference becomes statistically significant ($p = 0.06$). This does not really affect our conclusion of the initial allocation not affecting outcomes, as all other results regarding the impact of changing the initial payment allocation remain unaffected.

Table A10: Robustness of treatment effects on SLMPs adoption using negative binomial and probit estimation.

		Agronomy SLMPs			Agro-sylvo-pastoral SLMPs			Agroforestry SLMPs		
	# SLMPs adopted (1)	**Zai** (2)	**Heap and pit composting** (3)	**Stone and earth bunds** (4)	**Mowing and cons. of natu. fodder** (5)	**Forage crop cultivation** (6)	**Use of agr. and wood by-products** (7)	**Controlled clearing** (8)	**Assisted natural regeneration** (9)	**Living hedges** (10)
Payment treatment	0.421*	0.001	0.101	0.104	0.136***	0.012	-0.068	-0.011	0.136*	0.079***
	(0.223)	(0.002)	(0.067)	(0.063)	(0.053)	(0.021)	(0.069)	(0.109)	(0.071)	(0.020)
Observations	1574	1574	1574	1574	1574	1574	1574	1574	1574	1574
Baseline	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region FE-s	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	2.784	0.070	0.564	0.352	0.148	0.084	0.619	0.617	0.268	0.060
Control std.dev.	1.670	0.256	0.496	0.478	0.355	0.277	0.486	0.486	0.444	0.238
Effect size (in std.dev.)	0.252	0.003	0.203	0.217	0.383	0.045	-0.139	-0.022	0.307	0.330
Unit	#	#	#	#	#	#	#	#	#	#

Notes: This table replicates column (1) of Table 5 using negative binomial regression, and columns (2)–(10) of that table using probit. All coefficients are marginal effects. Standard errors, presented in parentheses, are clustered at the commune level. For the vector of covariates, see Table 5.

Table A11: Robustness of treatment effects on input use using negative binomial and probit estimation.

	# **Manually sowed plots**	**Household labor**
	(1)	(2)
Payment treatment	0.003	-0.715
	(0.020)	(0.492)
Observations	1560	1555
Baseline	Yes	No
Covariates	Yes	Yes
Region FE-s	Yes	Yes
Control mean	1.641	3.634
Control std.dev.	0.825	5.187
Effect size (in std.dev.)	0.004	-0.138
Unit	Share	# HH member

Notes: This table re-estimates columns (2) and (3) of Table 7 using, respectively, probit and negative binomial estimation. Coefficients are marginal effects. Standard errors, presented in parentheses, are clustered at the commune level. For the vector of covariates, see Table 5.

Table A12: Robustness of treatment effects on knowledge exchange using negative binomial and probit estimation.

	PF asked for advice (1)	**Farmers discuss SLMPs** (2)	**# SLMPs adopted by CF** (3)	**CF monitor PF adoption** (4)	**% of SLMPs not adopted due to lack of knowledge** (5)
Payment treatment	0.121^{*}	0.133^{**}	0.343	0.110^{*}	-0.254^{***}
	(0.068)	(0.065)	(0.216)	(0.067)	(0.068)
Observations	1573	1573	315	1573	1571
Baseline	Yes	Yes	Yes	Yes	No
Covariates	Yes	Yes	Yes	Yes	Yes
Region FE-s	Yes	Yes	Yes	Yes	Yes
Control mean	0.356	0.408	3.900	0.336	0.231
Control std.dev.	0.479	0.492	1.746	0.473	0.263
Effect size (in std.dev.)	0.253	0.271	0.196	0.232	-0.965
Unit	share	share	#	share	share

Notes: This table re-estimates columns (1), (2), (4) and (5) of Table 10 using probit estimation, and column (3) of that table using negative binomial estimation. Coefficients are marginal effects. Standard errors, presented in parentheses, are clustered at the commune level. For the vector of covariates, see Table 5.

Table A13: Robustness of treatment effects of offering direct financial incentives for information dissemination to the contact farmers using negative binomial and probit estimation.

	# SLMPs adopt. by PFs (1)	**PF asked for advice** (2)	**Farmers discuss SLMPs** (3)	**# SLMPs adopted by CF** (4)	**CF monitor PF adoption** (5)	**% of SLMPs not adopted due to lack of knowledge** (6)
Split payment group	0.073	0.070	0.035	0.463*	0.075	-0.037
	(0.224)	(0.069)	(0.081)	(0.246)	(0.094)	(0.099)
Observations	978	978	978	195	978	976
Baseline	Yes	Yes	Yes	Yes	Yes	No
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Region FE-s	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	3.307	0.435	0.521	4.061	0.415	0.136
Control std.dev.	1.925	0.496	0.500	1.963	0.493	0.218
Effect size (in std.dev.)	0.038	0.141	0.070	0.236	0.153	-0.167
Unit	#	share	share	#	share	share

Notes: This table re-estimates columns (1) and (4) of Table 11 using negative binomial estimation, and columns (2), (3), (5) and (6) using probit estimation. All coefficients are marginal effects. Standard errors, presented in parentheses, are clustered at the commune level. For the vector of covariates, see Table 5.

A.5.2 Randomized inference

So far, we have tested our treatment effects with conventional standard errors which are based on the asymptotic distribution of the treatment effects. However, one might question the use of asymptotically consistent standard errors given the relatively limited number of clusters in our experiment (32 communes). To address this concern, we use Fisherian randomized inference to test the sharp null of no treatment effects following Athey and Imbens (2017) and Young (2018).[17] We conduct Fisherian inference tests using a software presented by Heß (2017). We consider treatment assignment permutations that are stratified at the region level and clustered at the commune level. We calculate p-values based on randomized inference for four intention-to-treat effects of conditional payments (see equation (1)) that summarize our main findings: on the number of practices adopted by peer farmers (column (1) of Table 5), on the IHS-transformed agricultural incomes of peer farmers (column (1) of Table 8), on the peer farmers' agricultural productivity index (column (6) of Table 8), and on the knowledge dissemination index (column (6) of Table 10). We present the results from randomized inference in Table A14. Columns (1) and (2) of Table A14 show the OLS point estimates and the corresponding conventional standard errors. The conventional p-values are presented in column (3) and indicate that our treatment effect estimates are significant at the 5% level, or better. The p-values from randomized inference are presented in column (4). Overall they are larger than those in column (3), but the effects are still significant: those on crop productivity and on knowledge exchange are significant at the 5% level whereas the effects on agricultural income and peer farmer SLMPs uptake are significant at the 10% level. Our results are therefore robust to Fisherian exact tests.

[17] Randomization inference generates the exact distribution of treatment effect estimates by taking different permutations of treatment allocation over the whole sample and re-estimating the point estimates for each permutation.

Table A14: Randomized inference tests of the main treatment effect estimates.

	Coefficients	Std. Err.	Conventional p-val	RI p-val
# SLMPs adopted	0.507	0.232	0.036	0.086
Agricultural income	0.221	0.089	0.018	0.052
Productivity index	0.255	0.071	0.001	0.009
Knowledge exch.	0.268	0.095	0.008	0.035

Notes: Results are from applying randomization inference on each regression using 10,000 permutations of treatment assignment. In each permutation, the treatment assignment was stratified on regions and was randomized at the commune (cluster) level.

A.5.3 Multiple hypothesis testing

Finally, we show the robustness of our results to adjustments for multiple hypothesis testing. We already addressed this issue (at least partially) by combining individual outcome variables into summary indices within outcome groups following Kling *et al.* (2007) and Anderson (2008), thus reducing the number of implemented tests. In this appendix we assess the consequences of multiple hypothesis testing using two types of adjustments - Family-Wise Error Rate (FWER) adjustments, and False Detection Rate (FDR) adjustments. We apply these adjustments to our four of key tests; see also Section (Appendix A.5.2).

First, we adjust for family-wise error rate (FWER) using the Bonferroni-Holm step-down procedure (implemented in Jones *et al.* (2019)) and the Westfall-Young free step-down method using re-randomization. Both adjust p-values upwards with the probability of making any kind of false rejections, while the Westfall-Young approach controls for potential correlation between these outcome groups via re-randomization (Anderson, 2008; Young, 2018). In Table A15, we compare the unadjusted p-values (column (3)) to those adjusted with the Bonferroni-Holm procedure (column (4)) and with the Westfall-Young procedure (column (5)). The four estimated effects survive the Bonferroni-Holm adjustment process at the 5% significance level and the Westfall-Young process at the 10% level. Re-randomization in the Westfall-Young approach also allows us to jointly test the null of no overall effect of the experiment (bottom of column (5)) which we can reject at the 5% level.

To the extent that FWER adjustments are conservative because they control for any kind of false rejection, we also implement a false detection rate (FDR) adjustment. Instead of controlling for the probability of any false detection, FDR adjusts for the expected share of false rejections (Anderson, 2008). The procedure calculates sharpened q-values which represent the share of false rejections if one were to reject the hypothesis at hand and all the other hypotheses with lower q-values. We present these sharpened q-values in column (6) of Table A15. Controlling for FDR at $q = .05$ or $q = .10$ (conventional levels used by Anderson (2008) and Banerjee *et al.* (2015)), our treatment effects remain significant.[18] The estimated effects of payments therefore survive both types of multiple hypothesis testing adjustments.

[18] More precisely, controlling for FDR at 0.05 level, we can reject all null hypothesis and the expected false discovery rate will not be larger than 1.8% or 0.72 hypotheses.

Table A15: Adjustments for multiple hypothesis testing.

	(1)	(2)	(3)	(4)	(5)	(6)
				FWER adjustments		FDR adjustment
	Coefficients	**Std.err.**	**Regular p-value**	**Bonferroni-Holm free-step down**	**Westfall-Young (re-randomization)**	**Anderson's sharpened q-s**
# SLMPs adopted	0.507	0.232	0.036	0.036	0.086	0.019
Agricultural income	0.221	0.089	0.018	0.036	0.086	0.018
Productivity index	0.255	0.071	0.001	0.004	0.025	0.005
Knowledge exch.	0.268	0.095	0.008	0.025	0.086	0.013
Joint	.	.	.	.	0.032	.

Notes: The table presents adjusted p-values in column (4)-(8). The Bonferroni-Holm adjustment does not use resampling nor treatment allocation permutations. We implement the Westfall-Young method with random permutations of treatment allocation (re-randomization, Monte Carlo simulations) instead of resampling (bootstrapped). Adjustment is based on 10000 random permutation. The joint test for all effects in the randomization based Westfall-Young algorithm is based on randomization-t statistics of Young (2018).