Conservation Agriculture Evaluation Project in Northern Ghana: a formative evaluation using a framed field experiment

Kate Ambler
International Food Policy Research Institute

Alan de Brauw
International Food Policy Research Institute

Nicole Gargano
Innovations for Poverty Action

Mike Murphy
International Food Policy Research Institute

Usamatu Salifu
Innovations for Poverty Action

Formative evaluation report

Accepted by 3ie: July 2020

About 3ie

About this formative study

This formative evaluation was submitted in partial fulfilment of the requirements of grant TW4.1008 awarded under Agricultural Innovation Evidence Programme. This version of the report is technically sound and 3ie is making it available to the public in this final report version as it was received. No further work has been done.

The 3ie technical quality assurance team comprises Mark Engelbert, Deeksha Ahuja, Diana Milena Lopez Avila, Sayak Khatua, Stuti Tripathi with overall technical supervision by Marie Gaarder.

3ie received funding for the Agricultural Innovation Evidence Programme from Alliance for Green Revolution in Africa, the Bill & Melinda Gates Foundation, the International Fund for Agricultural Development and the UK Department for International Development. A complete listing of all of 3ie's donors is available on the 3ie website.

Suggested citation: Ambler, K, Brauw, A, Gargano, N, Murphy, M and Salifu, U, 2020. *Conservation Agriculture Evaluation Project in Northern Ghana: a formative evaluation using a framed field experiment*, 3ie Formative Evaluation Report. New Delhi: International Initiative for Impact Evaluation (3ie). Available at: http://doi.org/10.23846/10.23846/TW4FE14

Acknowledgements

The authors would like to acknowledge 3ie for funding, technical review, and support throughout the study. The authors would also like to thank the Policies, Institutions, and Markets CGIAR Research Program for additional funding, and Alessandra Garbero, Luabeya Kapiamba, Esther Kasalu-Coffin, John Manful, Dean Karlan, Chris Udry, Aslihan Arslan, Shashidhara Kolavalli, Federica di Battista, Kofi Boa, Salifau Amadu, Hassan Moomin, Madeleen Husselman, Mubarak Yakubu, and David Spielman, among others, for help designing and conducting the research herein.

Executive summary

Climate change is an increasingly important consideration in sub-Saharan Africa, where a large proportion of the population is partially or fully employed in agriculture. The adoption of conservation agriculture (CA) techniques is thought to be a way to mitigate and adapt to climate change outcomes in agriculture and involves significant public benefits. CA can also increase farm yields and allow farmers to become more resilient to unpredictable weather patterns. However, increased yields are not immediately realized after adoption; continued adoption is necessary over several seasons. Additionally, CA methods increase weed growth and imply increased upfront costs to mitigate that growth. As such, farmers can perceive or be exposed to negative outcomes when adopting CA, making them hesitant to adopt or quick to abandon them. As a result, CA adoption remains relatively low in much of sub-Saharan Africa. Given the significant potential public benefit to CA adoption at scale, and potential private benefits to CA adoption in the medium term, incentivizing farmers to adopt CA techniques is a natural policy option for governments.

In this research project, Innovations for Poverty Action (IPA), International Food Policy Research Institute (IFPRI), and the Ghana Agricultural Sector Investment Programme (GASIP) collaborated to design, implement, and test methods to encourage farmers to adopt CA techniques. Given the long time-horizon necessary for measuring the continued take up of CA techniques and the ultimate impact on yields, this formative evaluation focuses on understanding whether adoption can be increased through either well-designed incentives or through information regarding peer adoption. Given the long time horizon needed to realize the benefits of CA adoption, this evaluation uses a framed field experiment (FFE) to simulate choices over several seasons and provide initial evidence regarding the effectiveness of these strategies.

Incentives and peer information are both methods that could help overcome barriers to CA adoption. Incentives should reduce farmers' perceived risk of CA adoption, and can provide a bridge to expected yield improvements from CA. Though many questions about the most effective form of incentives must be answered before large scale implementation, in this study we examine the primary question of whether farmers respond to incentives at all. Farmers also receive information from peers that may influence their adoption decisions. The field experiment therefore investigated whether farmers respond to information about peer adoption, and importantly examined how farmers differentially respond to different types of information.

The evaluation had two phases. The first phase included a review of previous CA programmes in Ghana, focusing on the northern parts of the country, and a series of focus groups discussions with farmers. The focus groups were designed to understand which CA techniques are well known and which ones are less known and used, perceived and actual barriers to adopting CA techniques, and how to encourage farmers to adopt CA techniques.

The second phase used the results of focus group discussions to design a FFE that simulates farmers' decisions about whether to adopt a certain CA technique (minimum soil disturbance) over several seasons. The FFE randomly allocated some farmers to receive incentives for adopting minimum soil disturbance, and some to receive

information about a neighbours' experience with adopting CA. It allowed the research team to simulate adoption decisions across many agricultural seasons, an important and otherwise difficult element of research on CA.

The focus groups and field experiment both indicate that incentives could improve CA adoption. In the second round of focus groups, farmers generally agreed that in-kind incentives would induce them to try out CA. In the field experiment, farmers exposed to incentives were 7.6 percent more likely to choose MSD, and about 8 percent more likely to achieve a production gain. Whereas farmers did not generally respond to peer information, information did positively affect adoption when that information was about peers who had successfully adopted CA over a long time period and had positive outcomes. Therefore, demonstration plots maintaining CA over a longer time period could effectively catalyse adoption.

Contents

List of figures and tables

Abbreviations and acronyms

CA	Conservation agriculture
CAgE	Conservation Agriculture Evaluation Project
CNTA	Center for No-Till Agriculture
CP	Conventional practices
FBO	Farmer based organization
FSIN	Food Security Information Network
GASIP	Ghana Agricultural Sector Investment Programme
IFAD	International Fund for Agricultural Development
IFPRI	International Food Policy Research Institute
IPA	Innovations for Poverty Action
MoFA	Ghanaian Ministry of Food & Agriculture
MSD	Minimal soil disturbance
NRGP	National Rural Growth Programme
SOM	Soil Organic Matter
UNCCD	United Nations Convention to Combat Desertification
ZT	Zero tillage

1. Introduction

Climate change presents a serious threat to smallholder farmers, especially those dependent on rain-fed crop cultivation, who lack adequate capacity to mitigate or adapt to adverse weather events, particularly droughts, floods or irregularly timed rainfall (UNDP, 2017). At the same time, farmers face a long-run threat to the productivity of agricultural land due to soil degradation, with Africa likely to face the most severe effects (UNCCD, 2017). The adoption of Conservation Agriculture (CA) practices is thought to be a way to respond to both challenges.

CA refers to three main principles (FAO, 2007). The first is minimum soil disturbance, which implies no tillage and that seeding should occur directly. Second, soil should be covered permanently, either with cover crops or crop residue. Third, crop rotation should occur, which allows for crops to root at different soil depths, and so that different sets of nutrients can either be added to or taken from the soil.

While there is consensus that CA provides public benefits (Knowler & Bradshaw, 2007) (Palm, et al., 2014), adopting CA does not necessarily provide immediate private benefits to farmers. Since it takes time for organic matter to build up in the soil, productivity gains are not immediately realized. At the same time, both not ploughing and applying mulch can increase weed growth, imposing immediate costs on farmers either for herbicide application (if available) or increased labour to weed their plots. Farmers adopting CA therefore typically face an immediate cost for a potentially uncertain future benefit, and therefore not surprisingly adoption rates in Africa have been low (Giller, et al., 2009).

Given that there is a public good component to CA and potential private benefits to adopters in the medium-term, one method of reducing farmers' perceived risk of CA techniques is to provide incentives for adopting CA. To date, there has only been limited rigorous study about whether farmers would respond to incentives for adoption, and study of this question is complicated by the relatively long time-horizon it takes for CA benefits to occur.

IPA and IFPRI collaborated with The Ghana Agricultural Sector Investment Programme (GASIP), a programme within Ghana's Ministry of Food and Agriculture (MoFA), to design and implement a framed field experiment (FFE) which incentivized farmers to take up CA practices. Given the long time-horizon necessary for measuring continued take-up of CA techniques and realizing yield benefits, the FFE enabled this formative evaluation to explore incentive design and the role of peer information in promoting adoption.

The evaluation had two phases. The first phase included a review of previous CA programmes in Ghana, focusing on the northern parts of the country, and a series of focus group discussions with farmers. The focus groups were designed to understand which CA techniques are well known and used, perceived and actual barriers to adopting CA techniques, and how to encourage farmers to adopt CA techniques.

The second phase consisted of a baseline survey paired with the FFE designed to simulate farmers' decisions about whether to adopt a certain CA technique (minimum soil disturbance) over several seasons. The FFE was informed by the qualitative component and included incentive and information treatments to understand how both may affect adoption decisions.

The results from the FFE are intended to provide guidance regarding incentive and information interventions that can be evaluated in the field as part of future programming. A small RCT was conducted by GASIP in late 2019 and early 2020 which can provide some information regarding short term adoption in the same communities. GASIP is slated to end in 2020, and as such the results are also meant to help inform policies set by MoFA locally and IFAD more generally in considering designs for future programmes incorporating CA, and/or the design of full, multi-year RCT evaluations.

2. Context

2.1 Literature review

The case in favour of adopting CA practices is fundamentally an argument related to soil management. The first principle, minimizing the extent to which the soil is disturbed, appears counter intuitive. The plough is generally considered a foundational technology whose refinement played a pivotal role in economic development historically (Andersen, et al., 2016). However, increased awareness of the long-run environmental impacts of top-soil erosion, and the resultant increased demand for increased use of chemical inputs have led researchers to pursue alternative approaches in order to limit soil disturbance and increase soil organic matter (SOM) (Lal, et al., 2007).

Proponents of CA argue that traditional tillage systems expose soils leaving them vulnerable to erosion and reducing water infiltration leading to increased run-off and overuse of irrigation water and chemical inputs (Hobbs, 2007). By combining limited or zero tillage with the use of crop residues to cover the area to be planted (increasing SOM over time) and rotating crops (varying the root depth and limiting compaction) CA is argued to improve yields, ultimately individually benefitting farmers, while providing common environmental benefits to communities. The hypothesis that adopting CA practices increases yields has been the subject of a substantial body of research within the agronomic literature, much of it highly context specific. To evaluate the evidence for this claim, we focus here on review studies relevant to our context that synthesize these findings.

One key study by Giller et al. (2009) notes that since SOM takes time to accumulate, yield benefits are likely to be slow to be realized. They conclude that yield gains can be achieved over the long-run (up to ten years) but that in the short term observed yield impacts are inconsistent, since the benefits of increased short-term water retention may be offset by poor germination and increases in weeds, pests and disease, though they do note that there is some evidence for positive effects under rain-fed agriculture where precipitation is limited.

Subsequent meta-analyses which draw on a larger pool of studies generally confirm these conclusions. Rusinamhodzi et al. (2011) focus on assessing long-term effects of adopting CA principles on maize under rain-fed conditions. They note that yield benefits associated with zero tillage take ten seasons to materialize. They also show heterogeneity in adoption benefits with respect to rainfall. They report a positive long-term trend associated with CA take-up where annual rainfall is less than 600mm, with no clear trend in sites within a normal rainfall range, and indeed a negative trend in areas with high rainfall levels. These results are similar to the results estimated by Brouder &

Gomez Macpherson (2014) who, focusing specifically on zero tillage (ZT), find negative short term yield effects in the first two years of adoption for major crops but a positive trend for maize over the long term. Pittelkow et al. (2015) also focusing exclusively on no-till agriculture, find similar evidence of a yield penalty associated with no-till adoption, estimating a yield gain of 7.3% when no-till is adopted in drier climates as part of a broader CA package.

While yields are an important concern, they are not of course the sole factor determining the profitability of CA practices. Costs are an important consideration, particularly the likelihood of increased weed infiltration (Giller, et al., 2009). Improvements in SOM are conducive to weeds as wells as crops, and by not practicing traditional tillage, farmers lose the benefit of reducing weeds during land preparation. If access to herbicides is limited or constrained, farmers must substitute manual labour to remove weeds. The cost is likely non-trivial: in one study in Zimbabwe, Nyamangara and co-authors (2014) note that the demand for labour more than doubled for reduced tillage plots as a result of increased weeding requirements, a significant upfront investment for resource constrained farmers (Pannell, et al., 2013).

A separate stream of literature examines the determinants of farmer adoption of CA practices. Andersson & D'Souza (2014) review the literature on CA adoption among smallholder farmers in southern Africa, arguing that current research in their context suffers from methodological concerns, and lacks common standards on how to even define CA adoption.[1] Their review is consistent with an earlier review by Knowler & Bradshaw (2007), which finds the literature to that point had not been successful in identifying common determinants.

Several studies involve discrete choice experiments to explore farmers' preferences for financial and non-financial incentives (ie. Marenya et al. (2014); Ward et al. (2016); Schaafsma et al. (2019)). While these studies are useful in examining farmers' stated preferences for different policy options, participant choices are not incentivized, resulting in a risk that responses may be driven by experimenter demand effects.

Overall, if one accepts the notion that CA is a public good, there is a need for research about whether and at what level incentives can lead to adoption of CA practices using robust research designs with exogenous variation in key policy variables. One exception is ongoing work by Bell et al. (2018) in Malawi which used randomized assignment to explore the role of incentives in CA adoption, finding an increase in adoption after one season as a result of an incentives treatment. Our FFE builds on this result, making the long time horizon explicit while controlling uncertainty in rainfall and payoffs. Interestingly, Bell et al. (2018) also find having a neighbour adopt CA is associated with adoption increases similar in size to the incentive treatment. We attempt to expand on this finding by randomly assigning information about peer adoption.

[1] Feder et al. (1985) and Foster & Rosenzweig (2010) review the larger literature related to the general adoption of agricultural technologies. Magruder (2018) explores the recent rise of the use of field experiments to test hypotheses related to adoption, focusing on studies concerning credit constraints, insurance, and information diffusion.

2.2 Conservation agriculture in Ghana

Several programmes have incorporated agricultural extension around conservation agriculture techniques in northern Ghana in the past few decades. However, most studies have not been evaluated, so it is not clear if they have been effective in stimulating CA adoption. The first such activity was Sasakawa Global 2000, which has been active in 15 sub-Saharan African countries over the past 30 years and was active in Ghana between 1986 and 2003. The broader Sasakawa Global 2000 programme focused on catalysing the adoption of improved seed technologies. The Ghana programme also promoted no till farming and a reduction in field burning and may have been the first programme related to CA in sub-Saharan Africa (Ito et al. 2007). The programme used demonstration plots and cooperated with local agro-dealers to increase input accessibility. An evaluation suggested that farmers reported difficulties adopting many such techniques, including weed control and plant survival after planting (Ekboir, et al., 2002).

A second activity that ended in the previous decade is the Savannah Resources Management Project, sponsored by Danida and implemented by the Ministry of Lands and Forestry. The activity focused on developing sustainable land management systems promoting permanent soil cover (Boahen, et al., 2007). In general, its activities appear to have been poorly documented, as other sources of information on this programme were not available.

An important institution related to CA in Ghana is the Center for No-Till Agriculture (CNTA) located in Nkawie-Toase in the Ashanti region. It which was established by Dr. Kofi Boa in 2012 with support from the Howard Buffett Foundation, and it is both a working farm and a demonstration center; it regularly hosts farmers from elsewhere in Ghana to train them in hands-on in CA techniques, and also holds external community-based training events. Scientifically, the CNTA has shown using local crops and crop rotations that minimal tillage increases nutrients in the soil (Davies et al., 2014). Future satellite centers are planned for Kumbungu (Northern Region), Zebilla (Upper East Region), and Tanina (Upper West Region).

More recently, the USAID Feed the Future Agricultural Technology Transfer programme, included a CA approach as one of its activities (IFDC, 2016). Specifically, the programme carried out community level sensitization activities about CA principles, emphasizing intercropping with cover crops and conserving crop residues by collaborating with herders to find designated grazing areas. It also set up nine participatory adaptive trials to allow farmers to test different farming practices based on their interests. These plots also served to train farmers on CA principles, practices, and technologies.[2]

Finally, CA plays a role in the on-going World Bank funded Sustainable Land and Water Management Practice Project (SLWMP). The SLWMP, which began in 2010, aims to reduce land degradation and enhance maintenance of biodiversity. Programme activities aim to promote efficient soil and water management practices in the farming system and empower smallholder farmers to diversify their farms through integration of trees and

[2] The programme ended in 2018.

value-addition through post-harvest management support. Appropriate SLWM practices for northern Ghana were identified through consultation with stakeholders and based on local conditions; CA is among the practices identified.

One of the districts in which the SLWMP operates overlaps with a district under GASIP in the Upper East Region (West Mamprusi). In 2017, the research team met with the West Mamprusi DDA staff in Wale Wale (one of the targeted districts), to discuss preliminary feedback on how the SLWMP was received. They reported that while some of the practices were very well received (for example, crop rotation), farmers considered other techniques as more tedious, and so adoption was limited. The main concern expressed by the DDA was the limited time that the programme spends in each community it engages with (i.e. one year), is not sufficient to efficiently promote adoption. According to DDA staff, farmers need to observe impact of the practices to be convinced, and one year is not enough to convince them.

In sum, there have been scattered attempts to disseminate CA over the past two decades in northern Ghana, with a distinct lack of evaluation associated with any of the programmes. However, due to the lack of both rigorous evaluation and transparent monitoring, even adoption rates for any of these programmes are effectively unknown. In addition to the primary causal research question, this study will also provide descriptive data on farmers' uptake of CA practices in northern Ghana.

2.3 Quantitative Sample Characteristics

The sample consists of farmers who were GASIP beneficiaries in communities incorporated into the project for its CA subcomponent in 2018 or 2019.[3] Table 1 lists the regions and districts where each of the communities was located.

Table 1: Study regions and districts

Region	**District**	**Number of communities**	
		2018	**2019**
Northern	Yendi	3	3
	West Mamprusi	3	3
	Kumbungu	3	3
	Saboba	3	3
	Kpandai	3	3
Upper East	Bolgatanga	3	3
	Bongo	3	3
	Builsa	3	3
Upper West	Jirapa	3	3
	Wa West	3	3
Brong Ahafo	Kintampo North	0	3
	Banda	0	3

[3] We discuss the GASIP context in more detail in the following section.

Within each community, GASIP began by organizing approximately 20 beneficiary farmers and one lead farmer in each community into a CA Farmer Based Organization (FBO).[4] The lead farmer in each FBO was elected to represent the group by other community members in the FBO. To facilitate this research, GASIP provided administrative lists for each FBO. IPA field staff then visited each community shortly prior to data collection and worked with the lead farmer to verify and update the lists.

The combined list of farmers included 1,328 GASIP members across the 66 communities. The field team completed data collection for 1,324 members, an attrition rate of less than one per cent. One important attribute of the sample was that in some cases more than one member of a household were members of the FBO. As a result, the quantitative sample comprises 1,324 GASIP members from 1,117 households. When more than one FBO member was present in a household, one member acted as the main respondent for the baseline survey.[5] Where possible, individual level questions were answered by the (adult) individual in question, and specifically the risk and time modules in the baseline survey were individually asked of each FBO member. There were a small number of cases where the field team was unable to match secondary individuals within a household survey to experiment participants.[6] As a result, there are 6 individuals in the sample for whom we lack demographic data, and a separate 14 for whom we lack data on risk and time preferences (and one individual who lacks both). We retain these individuals in the analysis regressions (and incorporate control variables for missing individual and risk data respectively).

Table 2 provides some descriptive statistics about the GASIP members in our sample (Panel A) and the households in which they live (Panel B).

[4] The size of member lists varied from 15-27 individuals, with an average size of 20 members. There was one community outside this range, which reported 37 current members. To avoid over-representing members from this community, we randomly sampled 20 members from the administrative data provided by GASIP.

[5] The field team protocol was to use the GASIP member with the lowest individual ID value as the main respondent for the baseline survey, exploiting the fact that ID numbers were assigned randomly within communities. However, if this person was not available at the time of the survey visit, enumerators were instructed to interview the available member.

[6] Challenges in matching occurred as a result of participants who went by multiple names within a community, leading to difficulties matching secondary members to household roster lists.

Table 2: Summary statistics of sample GASIP FBO members and households

Panel A: FBO Member Characteristics					
	Mean	**SD**	**Min**	**Max**	**Obs.**
Age	41.20	12.38	13	98	1303
Is female	0.53	0.50	0	1	1303
Received no schooling	0.65	0.48	0	1	1303
Received some primary education	0.11	0.31	0	1	1303
Completed primary school	0.03	0.17	0	1	1303
Received some secondary education	0.12	0.32	0	1	1303
Completed secondary school or higher	0.09	0.28	0	1	1303
Primary activity: Household farm work	0.89	0.31	0	1	1302
Reports secondary activity	0.50	0.50	0	1	1302
Reports any work off-farm	0.29	0.46	0	1	1302
Panel B: Household Characteristics					
	Mean	**SD**	**Min**	**Max**	**Obs.**
Region: Northern	0.44	0.50	0	1	1117
Region: Upper East	0.30	0.46	0	1	1117
Region: Upper West	0.16	0.37	0	1	1117
Region: Brong Ahafo	0.09	0.29	0	1	1117
Household size	9.40	5.26	1	45	1117
Members currently present	8.71	5.01	1	41	1117
Number of adults (14+)	5.38	2.91	1	22	1114
Number of children (<14)	3.93	2.90	0	21	1114
Household reports polygamy	0.27	0.45	0	1	1117
Religion: Catholic	0.18	0.38	0	1	1117
Religion: Other christian	0.34	0.47	0	1	1117
Religion: Muslim	0.30	0.46	0	1	1117
Religion: Traditional/animist	0.18	0.38	0	1	1117
Language: Buli	0.10	0.31	0	1	1117
Language: Dagbani	0.15	0.36	0	1	1117
Language: Frafra/Gruni	0.20	0.40	0	1	1117
Language: Likpakpa	0.21	0.41	0	1	1117
Language: Other	0.25	0.44	0	1	1117

Source: Baseline survey data.

The sample is 53 percent female, with an average age of 41. FBO members have a low level of education, as 65 percent have no education at all and only 9 percent have completed secondary school. 90 percent are involved in household farm work as their primary activity, but approximately half report an additional activity. The average household size is 9.4, with a large range; in some study areas there are very large households, with a maximum of 45 members. Tables 1 and 2 in Online Appendix A provide further descriptive statistics from the baseline survey.

Table 3: Adoption and knowledge of CA techniques, by community type

	Old communities	New communities
Heard of…		
Conservation agriculture	0.95	0.79
Minimal soil disturbance	0.78	0.55
Cover cropping	0.79	0.54
Using residues	0.93	0.81
No burning	0.97	0.84
Crop rotation	0.84	0.73
Ever adopted…		
Conservation agriculture	0.92	0.85
Minimal soil disturbance	0.38	0.23
Cover cropping	0.42	0.30
Using residues	0.73	0.64
No burning	0.84	0.73
Crop rotation	0.52	0.47
Adopted last season…		
Conservation agriculture	0.89	0.83
Minimal soil disturbance	0.33	0.20
Cover cropping	0.36	0.27
Using residues	0.70	0.61
No burning	0.81	0.70
Crop rotation	0.48	0.41
Peer used last season…		
Conservation agriculture	0.71	0.71
Minimal soil disturbance	0.25	0.13
Cover cropping	0.27	0.21
Using residues	0.53	0.49
No burning	0.62	0.59
Crop rotation	0.35	0.32
Plan to adopt next season		
Conservation agriculture	0.90	0.85
Minimal soil disturbance	0.35	0.22
Cover cropping	0.40	0.29
Using residues	0.72	0.63
No burning	0.84	0.72
Crop rotation	0.51	0.46

Note: Columns show mean proportion of baseline survey respondents responding "Yes" for each category. For "Heard of" the overall "Conservation Agriculture" category was asked separately from the sub-categories. For other questions the overall CA proportion is an indicator for responding "Yes" to one or more sub-categories.
Source: Baseline survey data, 2019.

Table 3 further reports information related to the knowledge and use of five concepts related to the three CA techniques: i) minimal soil disturbance; ii) cover cropping; iii) using residues; iv) no burning (the latter three all relate to permanent soil cover); and v) crop rotation. We further show an indicator for CA overall. Respondents are asked whether they heard of CA separately, but the other categories are derived from the responses regarding the five techniques. Responses are shown separately for the “old” communities that received GASIP services in 2018, and the “new” communities, which were slated to begin services following the baseline and FFE.

In general, farmers claim overall knowledge of both CA and its techniques. Overall knowledge is higher, as would be expected, in the 2018 communities than the 2019 communities. Measures of adoption (ever, last season, and plans for next season) are lower, but still generally high for both use of crop residues and no burning. Adoption of crop rotation and cover cropping is lower, and minimal soil disturbance is the least adopted technology across all measures. In general, respondents report lower peer use from last season, but the ordering is similar to own adoption. Across the board, old communities report higher adoption than new communities.

Finally, it is important to note that sample farmers have chosen to participate in the GASIP CA programme, which includes farming a demonstration plot, and they may differ from the rest of the population in that there may be observable or unobservable characteristics about these farmers that drove them to be chosen to participate and to agree to participation.

3. Intervention description, intervention logic, monitoring plan and the theory of change

3.1 The GASIP Conservation Agriculture programme

3.1.1 Objectives of the GASIP programme

The Ghana Agriculture Sector Investment Programme(GASIP) is a Government of Ghana Programme financed by the Government of Ghana and the International Fund for Agricultural Development (IFAD). It is being implemented by the Ministry of Food and Agriculture (MOFA). The overall goal of GASIP is to help contribute to reduced poverty in rural Ghana; the programme is meant to work in 180 districts by its end, representing a substantial increase in coverage over its predecessor, the Northern Rural Growth Programme (NRGP). The development objective of GASIP is to meet the goal by improving value chains for multiple crops grown by smallholders so that they can enhance their profitability and increase climate change resilience. The programme was designed with three main components: 1) Value Chain Development; 2) Value Chain Infrastructure Improvement; and 3) Knowledge Management, Policy Support, and Coordination.

The value chain development component of GASIP is further broken down into three subcomponents, which aim to work together to help improve smallholder profitability and climate change resilience. The subcomponents are agribusiness linkage development, value chain financing, and climate change resilience. CA extension falls under the climate change resilience subcomponent but is meant to work with other activities to build profitability and resilience.

3.1.2 GASIP Climate Change Resilience Subcomponent

GASIP's Climate Change Resilience subcomponent includes three activities: the promotion of CA through extension, efficient water-use techniques in irrigation, and enhanced awareness of climate change resilience by public and private value chain actors (IFAD 2019). The main activities of this sub-component have been trainings and workshops conducted with beneficiary FBOs and lead farmers on climate change practices and benefits. The concept of CA as promoted by GASIP follows from the literature (e.g. FAO 2007). GASIP's extension plan is therefore meant to teach the three main principles—minimum soil disturbance, permanent soil cover, and crop rotation—through extension to FBOs, some of which would have been established through the NRGP.

3.1.3 Implementation activities – GASIP CA Programme

Initial work on CA extension under GASIP was delayed and did not start until 2018. Prior to the 2018 farming season, CA demonstration sites were established in 30 selected communities, across 3 regions and 10 districts. An additional 36 communities were added for the 2019 farming season. The demonstration sites were expected to act as both training sites and reference points for the promotion of technologies.

Intended beneficiaries of the CA extension are smallholders organized in FBOs participating in GASIP. Extension workers with knowledge of CA techniques collaborating with the CNTA sites are charged with disseminating CA knowledge. For each GASIP FBO, a lead farmer is selected by FBO members to represent the group, and that farmer is invited to attend a CA event at the CNTA in Kumasi. The baseline survey discussed below and the FFE were conducted with all 66 FBOs in which GASIP worked during the 2019 agricultural season (the 2018 and 2019 groups).

Community and individual selection: GASIP undertakes the following steps in selecting communities to work with: 1) Analysis of vulnerability to climate change, 2) Discussion with district members on the programme and coordination for each district to provide GASIP a list of communities and farmers, 3) Verification visits to the list of communities and farmers provided by the district in order to confirm suitability.

The first step in selecting the communities consists of meeting the community leaders organizing a community meeting. During this community meeting, a GASIP representative introduces CA and emphasizes its potential. Together with community leaders, the GASIP representative explains the approach and how beneficiaries are selected. Each community must include at least 50 percent women, 20 percent aged 15-24 years, and 30 percent aged 25-34 years). The GASIP representative finally conducts a needs assessment of the community in order to justify the selection of the community into the programme (GASIP, 2018)

Site selection within a community: GASIP selects a site within a community for a CA demonstration plot. Minimum requirements for the site include accessibility by vehicle and close to a road (preferably the main road). Once a site location has been determined a GASIP representative returns to the community and collects data on characteristics of the land on which the CA demonstration site is set: 1) GPS positions, 2) altitude and topography, 3) soil type (texture), 4) cultivation history, 5) erosion features, and 6) fertility status.

The GASIP CA programme partnered with the CNTA to reinforce learning and promotion of CA practices. A lead farmer from each community is invited to visit the centre in order to facilitate proper implementation of CA practices at the demonstration sites within each community. Following the visit to the CNTA, farmers are encouraged to teach others in their community and nearby communities the processes behind no-till farming.

3.2 Intervention

A primary goal of the climate resilience subcomponent of the GASIP programme is to promote CA by educating farmers on how to best implement CA practices and encouraging the uptake of CA techniques. This study uses an FFE to study interventions designed to increase uptake. We test whether incentives or information about peers affects the adoption of MSD, given that the first research phase found that farmers already practice permanent soil cover and crop rotation to some degree.

In the FFE, farmers make decisions about adopting MSD over ten rounds representing agricultural seasons.[7] Each "season" is framed in three stages: a first stage in which the farmer decides whether to adopt conventional practices or MSD for that season, and pays an associated cost for weeding; a second stage in which they receive a random draw that determines whether the rainfall for that season is "normal" or "poor"; and a third stage in which they receive a payout amount for their "harvest" for that season. Their net payout for each round is determined by the amount they received from production (determined by their choice of practice and the rainfall outcome) minus the amount they paid for weeding (determined by their choice of practice).

In the initial scenario, conventional practices (CP) return the same amount from production in normal rainfall seasons as MSD, but a lower amount in poor rainfall seasons. However, since the cost of weeding is higher for MSD, on net CP returns a higher payoff than MSD in normal rainfall seasons and the same payoff as MSD in poor rainfall seasons. Hence- all else equal- participants should favour CP under this initial set-up. The scenario is then modified by introducing a gain to production associated with MSD, realized between the 5^{th}-7^{th} season of continuous adoption, such that the net payoff for MSD then equals CP for normal rainfall, and is higher for poor rainfall.[8]

Participants are randomly assigned two independent treatments.[9] The first treatment is an incentive treatment. Under this treatment, farmers receive a bonus payment for each season of the first four seasons in which they adopt MSD, with the idea of subsidizing adoption until the point (or close to the point) where production gains can be realized. For the second treatment, they are selected to receive information about a generic peer's adoption history and output for the first four seasons, to test whether information about peers will affect their adoption decisions.

---

[7] The choice of ten rounds was to strike a balance between having enough rounds to represent benefits to MSD as long-run, but not so long as to lead to participant fatigue.

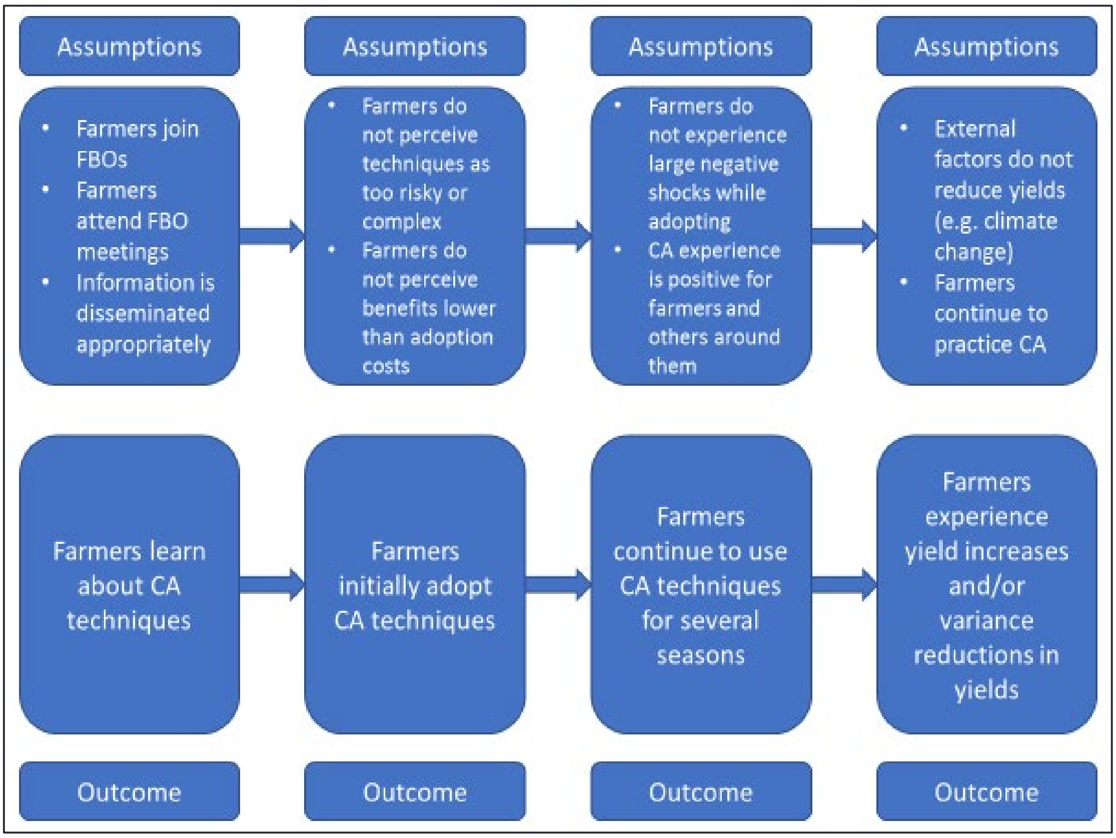
[8] The timing of the gain is unknown to participants until it is realized, though they know it occurs between seasons 5 and 7. Participants were randomly assigned to values of 5,6 or 7 rounds with equal probability.

[9] In other words, farmers can receive one of the treatments, both, or neither.

3.3 Theory of change

Though the intervention is focused on MSD, the theory of change we discuss relates to CA in general. The adoption of CA for improved farming outcomes can be illustrated relatively simply (Figure 1). We assume farmers may have information available about CA techniques through extension workers and from participating in demonstrations, but might not adopt it on their land due to uncertain initial returns and the fact that it takes a number of seasons of continuous adoption for productivity gains to be realized.

Figure 1: Illustration of theory of change for CA adoption



Having access to information about no-tillage techniques is necessary for farmers to adopt, however it is not sufficient. Farmers will only utilize the knowledge resources and adopt MSD if expected benefits of adoption exceed the costs. Moreover, farmers are likely to be risk averse, so adoption costs should include a risk premium. Finally, assuming farmers do know of CA techniques and adopt, farmers might have a negative shock in one of the first years of working with CA, reducing output and faith in CA, leading to dis-adoption in future years.

Incentives could catalyse CA adoption through two main mechanisms: (1) Incentives could act as a nudge to access available information, which will improve knowledge of CA techniques and reduce uncertainty surrounding adoption, and (2) Incentives may provide the financial compensation that will allow the financial benefits of transitioning to CA agriculture to outweigh its cost. Indirectly, it could lead other farmers in a village to adopt; if farmers observe fields that look more productive, they may also consider taking up the CA techniques. This knowledge can reduce the risk of adoption by providing additional, credible information.

4. Formative study evaluation questions and primary outcomes

The focus of the study is to evaluate the best means of promoting adoption of the CA technique of MSD. The key benefits of MSD are a reduction in soil evaporation and increased water infiltration- mitigating yield losses from low rainfall in the short term- and a reduction in soil erosion, increasing yields under normal or beneficial conditions with continuous adoption. Therefore, it is important to study factors helping or hindering adoption over a relatively long-time horizon. Incentive and information-based interventions to promote adoption have shown promise, but the need for long term study complicates impact evaluations. Formative evaluations such as this one, conducted prior to full impact evaluations are therefore crucial for proper design of such evaluations to maximize success.

The theory of change described in the previous section results in two main research hypotheses. First, if farmers receive incentives for adopting CA techniques promoted through extension agents, their adoption of CA techniques will increase. Second, if farmers observe farmers in their community or surrounding area practicing CA techniques, they will become more likely to also adopt CA. These information effects should be strongest when the observed farmer has adopted for a long enough period to realize production gains. Empirically, the null associated with the first research hypothesis is that the proportion of farmers choosing MSD when receiving incentives is no different than the proportion among farmers not receiving incentives. The second null hypothesis is that the proportion of farmers receiving information about adopting MSD is no different from the proportion of farmers adopting who did not receive the information.

The two samples are cross-randomized in the data, so whether they are complementary can be tested as well. We further randomize the information received regarding the behaviour and outcomes of the peer farmer. As such, among those who received information, we can examine the null hypothesis that the proportion of those choosing MSD is equal across the different types of information received.

The primary outcomes of interest are the number of times farmers chose MSD across all periods, whether they chose MSD long enough to receive the production bonus, and whether they ever chose conventional practices after having chosen MSD in a prior round.

5. Formative study evaluation design and methods

The interventions tested in this FFE could also be tested in a larger scale impact evaluation. The FFE differs in that participants make incentivized choices between representations of certain tasks (in our case CA practices) whereas in a project with an RCT incentivizing CA they would be expected to actually carry out those tasks (i.e.. implement the practices). The experiment is “framed” in that the task and information provided to participants is contextualized- thus in our case participants choose between different options labelled as agricultural practices, rather than abstract choices (i.e.. “Option A” or “Option B”). It additionally differs from a typical lab experiment in that the subject pool is from a non-academic context, in contrast to many university-based economic experiments (Harrison & List, 2004).

The FFE provides insights into the barriers to adoption and the drivers of dis-adoption. The key driver behind our choice to use an FFE as the project pilot is that it allows us to simulate the long time horizon needed to maximize the benefits of MSD in a way that would not be otherwise possible. The goal is to provide evidence about farmer behaviour, generated from the FFE, that would reflect the actual decisions that farmers must make around the adoption of a new practice.

5.1 Ethical considerations

All study components were reviewed by IFPRI's Institutional Review Board (IRB) and approved for implementation. Authorization for IPA staff was obtained through a reliance agreement signed between the IFPRI and IPA IRBs. The application contained copies of data collection materials and informed consent forms, as well as a detailed description of how data collection was to be implemented. All staff listed in the IRB submissions also provided evidence of having completed appropriate training for research with human subjects. Following completion of piloting of the experiment in December 2018, the script and other materials were revised, and updated materials were re-submitted to the IRB along with a description of planned changes, which the IRB reviewed and accepted.

5.2 Qualitative component design and data collection

The first stage of the research undertaken was to conduct a qualitative study to understand the feasibility of providing incentives for adopting CA techniques and to inform the baseline survey instrument design. The research was conducted in districts that GASIP was targeting and in villages in which there was some familiarity with CA among farmers from previous programmes, which would help in understanding the constraints to adoption. Interviews were carried out in Tinguri and Sagadugu communities, in West Mamprusi where they had been recent experience with the SLWMP program. Focus groups were split into groups of men and women in both communities.

Data collection occurred in two rounds. The first round of interviews was conducted between September and October 2017 and focused on the impressions of farmers of CA practices with emphasis on the challenges faced. Data from the first round was analysed and used to design incentives that were discussed with farmers during the second round conducted between November and December 2017. In total, 8 FGDs and 20 in-depth interviews were conducted with farmers in these two communities.

5.3 Framed field experiment design

5.3.1 Overview: Experiment and Procedure

The experiment was divided into 10 rounds to simulate 10 agricultural seasons. Based on results from the qualitative work, the FFE focused on the implementation of MSD, however the features of the technology are similar to the choices farmers face when implementing any CA technique. The beginning of the experiment consisted of an explanation of MSD and the way in which it impacts production costs and outputs. Then the experiment itself was described to the participant and a practice round was conducted.[10]

[10] A certain rainfall outcome was used in the practice round and this outcome was randomly determined on the individual level and stratified by community.

Prior to the first round, the participant received an endowment for use in the activity. Each round proceeded as follows: 1) The participant chooses a practice and pays the input costs associated with that practice; 2) The enumerator reveals the rainfall event for that period; and 3) The enumerator pays the participant the value of their production for that round.

At the beginning of each round, the enumerator provided the player with a choice sheet, showing the two choices and their associated payoffs for that round. The choice was always between two practices: conventional practices or minimal soil disturbance.

After choosing a practice, the participant provided the enumerator the money required to adopt that practice for the given round. The payment was taken from their endowment and any funds received from previous rounds. The experiment was structured such that the participant always had sufficient funds to choose either practice, irrespective of the outcome of prior rounds. The costs for both were displayed on the choice sheet.

The amount the participant received each round was determined by their choice of practice, their choice history, and the rainfall event. There are two possible rainfall outcomes: 'normal' and 'poor'. 'Poor' rainfall was intended to represent a negative shock and resulted in a lower production payout. The rainfall outcome for each round was individually randomized for each participant-round by the research team, with a probability of 2/3 assigned to normal rainfall, but the rainfall outcome was neither known to the enumerator nor the player prior to being realized.[11]

After revealing the rainfall outcome, the enumerator will provide the participant with money equivalent to the production payout, concluding the round. The net payoffs for any round were always positive, hence the participant always ended a round with more than they had at the outset of that round.

The payoffs were represented in pesewas, which are the sub-unit of the Ghanaian cedi. Images of coins and notes were used on visual aids showing payoffs, so participants could easily recognize the amounts involved. To prevent potential adverse issues during the experiment, play money was used during the FFE, and exchanged for real money following the conclusion of the final round. The play money used had the same appearance as local currency.

5.3.2 Incentive treatment

The incentive treatment consisted of one treatment arm and a control group. The treatment group received a bonus payment for each round in which they adopted MSD in any of the first four rounds of the experiment. The amount of the bonus was fixed at twenty pesewas, hence individuals in the incentive treatment could receive up to an additional eighty pesewas. The bonus was available independent of decisions in preceding rounds, so a treated individual choosing CP in Rounds 1-3, would still receive the bonus if they selected MSD in Round 4. This information was all conveyed to participants and the bonus amount included on the visual aid used for Rounds 1-4 for treated individuals. The only information which was not known to participants at the outset were rainfall outcomes and the timing of the production gain round (see 5.3.4 below).

[11] Once realized, the outcome could not be changed to prevent the enumerator from re-running the random process to benefit the participant.

The bonus was paid each round following the confirmation of the farmer's adoption decision. Two thirds of participants were randomized into the treatment group and one third into a control group that did not receive incentives. A larger portion were assigned to receive incentives to create sufficient variation across individuals with different values for the round in which they received the gain. Randomization was done at the individual level and stratified by community.[12]

5.3.3 Peer information treatment

The information treatment consisted of an additional prompt about the adoption decision and production outcome of a generic peer-farmer in the previous round, prior to making their decision for the current round. The prompt consisted of a short sentence or two which was read to treated individuals immediately before they made their decision. The information described a generic peer farmer and their choice for the previous 'season' or round within the experiment. There were four possible vignettes:

- Last year they used conventional practices on their plots, they have always used conventional practices.
- Last year they used minimal soil disturbance on their plots. They had not used this technique before.
- Last year they used minimal soil disturbance on their plots. They have been using minimal soil disturbance for the last ten years.
- Last year they used conventional practices on their plots. They had used minimal soil disturbance before but decided to go back to conventional practices.

Along with the vignette, they received information on the peer farmer earnings, which were calculated in the same way as for the participant, based on the realization of the rainfall variable in the previous round.[13] Since rainfall could take two values, there were eight possible variations of the information provided.

The peer information treatment was cross-randomized at the individual level with the incentive treatment evenly across one treatment and control group, stratified by community. Individuals receiving the information treatment were randomly assigned one vignette with equal probability (25 per cent) for each of the first four rounds. These draws were independent and not stratified, so an individual could receive the same vignette in different rounds, and the assignment for a given round did not affect the probability of assignment in other rounds.

5.3.4 Production gain round

One element of CA techniques that affects adoption is not only that production gains do not occur immediately, but that it is also not possible to know exactly when those gains will occur, making it difficult to properly design incentives for farmers to offset some of the

---

[12] Since communities were not of uniform size, individuals did not always evenly divide into treatment groups within a stratum (i.e. a community of twenty-one people cannot be divided into two equally size groups). For the additional 'misfit' observations we randomly allocate misfits independently across strata, using the procedure and associated *randtreat* command described in Carril (2017).

[13] For Round 1, participants in the information treatment were randomly assigned a rainfall outcome for the hypothetical preceding season.

increased cost. To model this in the FFE we randomize the round in which production gains may occur: in the fifth, sixth or seventh round of continuous adoption. In other words, participants would need to select MSD for five, six, or seven consecutive rounds before receiving the production bonus. Participants were randomized into one of these three groups with equal probability. This treatment was also stratified within community.

5.3.5 Assignment of additional random variables
Two further components of the experiment were randomized. First, the rainfall outcome was randomized as follows.[14] For each individual, ten independent random draws were taken (eleven for information treatment individuals) which were realized as either "normal rainfall" (with 2/3 probability) or "poor rainfall" (with 1/3 probability); the rainfall outcomes were not stratified.[15] The second component was the rainfall used during the practice round, which was assigned to either normal or poor with 1/2 probability and was stratified by community.

5.3.6 Further experimental details
The experiment was carried out using a standard script for all enumerators. The script was developed by IFPRI/IPA in English and then translated into local languages by IPA field staff. Enumerators were trained to read and follow the script exactly. The script is included in Online Appendix B.

The experiment was carried out using Survey CTO on electronic tablets. All randomizations were conducted with the full sample prior to implementation and preloaded into the tablets. The program automatically updated the text displayed to the enumerator (in the appropriate local language) based on the number of rounds elapsed, the participant's treatment status, and the adoption history of the participant, and calculated the amount to be paid at the conclusion of the experiment. As a result, the enumerators were not required to keep track of previous choices, implement any random selection of variables, or calculate payoff amounts.

Each enumerator received a set of choice sheets printed on laminated paper to act as a visual aid for the participant (Online Appendix C). The choice sheets contained a simple graphical representation of each phase of the experiment. and associated payoffs. They were identified with the letters A-C, and the instructions in the tablet told the enumerator at which point to show each visual aid.[16]

The choice sheet showed all of the relevant variables and outcomes for that round: the two choices available; the input cost for each choice; and the four potential production payouts (2 possible choices x 2 possible rainfall outcomes). For individuals in the incentives treatment, the choice sheet included the bonus available for choosing MSD

---

[14] Including the 'last year' referenced in Round 1 for individuals in the information treatment
[15] The eleventh value was used in the information treatment to refer to the previous season's rainfall outcome in Round 1. For subsequent rounds, the participant's previous rainfall was referenced- reflecting the fact that a peer farmer's rainfall is likely to be highly correlated with that of the participant.
[16] The letter A denoted CP versus MSD with no incentives and no production gain; the letter B denoted CP versus MSD with no incentives and a production gain, and the letter C denoted CP versus MSD with incentives and no production gain.

which was included in the net shown (production payouts + incentive – inputs costs). Similarly, for individuals receiving the production gain, the amount was incorporated in the production payout and net payoff shown. Hence, for a given round, the player will saw all potential payment outcomes for that round, with the only source of uncertainty being the rainfall variable.[17]

5.3.7 Payoffs

Table 4 presents a summary of the payoffs associated with each possible scenario. As the values associated with conventional practices were always fixed, there are three possible comparisons: CP vs. MSD without an incentive payment; CP vs. MSD with an incentive payment; CP vs. MSD with a production gain realized.[18]

Table 4: Payoff amounts per round, by practice choice & scenario

Scenario		**(1) Conventional Practices**	**(2) MSD (No incentives)**	**(3) MSD (Incentives)**	**(4) MSD (Gain)**
Normal rainfall	A. Input Cost	10	30	30	30
	B. Bonus Amount	0	0	20	0
	C. Harvest Income	100	100	100	120
	Net Payoff (C+B-A)	90	70	90	90
Poor rainfall	A. Input Cost	10	30	30	30
	B. Bonus Amount	0	0	20	0
	C. Harvest Income	30	50	50	60
	Net Payoff (C+B-A)	20	20	40	30
Choice Sheet		All	A	C	B
Possible rounds		1-10	1-10	1-4	5-10
Expected value		66.7	53.3	73.3	70

Note: Amounts shown are in pesewas, which are a division of the Ghanaian cedi. 100 pesewas = 1 cedi (approximately $0.19 USD at current market rates).
Source: Author's calculations.
Source: Authors' calculations.

Comparing the net payoffs, we can observe some straightforward features of the experiment. For a given round, if the participant is not eligible to receive incentives and has not realized a production gain, the payoff to CP is higher, as CP yields higher net returns for normal rainfall, and equal net returns for poor rainfall. Conversely, in the incentive treatment, the MSD payoff is higher, as the net payoff for normal rainfall is the same, but the net payoff for poor rainfall is higher. Lastly, if a participant has achieved

[17] There was one exception, which was the round in which a player realized the production gain for the first time. In this case, when making their choice the participant was shown the sheet without the gain incorporated into the production and payoff amounts (Sheet A). Following the rainfall realization, they were then informed by the enumerator that their production and payoff amount for that round had increased and were shown an updated choice sheet (Sheet C) for all subsequent rounds for which they were eligible to receive the production gain.
[18] Note that since the incentives were only available in Rounds 1-4, and the production gain took at least 5 rounds to realize, there is no scenario in which the participant could receive the incentives and the gain.

the production gain, the payoff to MSD is higher. In the latter case, the net payoff for normal rain is higher, but the net payoff for poor rainfall is the same.

Building on these features of the individual rounds, we can make some observations about payoffs across the full experiment. The expected value of choosing CP in a given round is 66.7 pesewas. Since the values associated with CP do not vary, the expected value of choosing CP for all ten rounds is simply 66.7 * 10 = 667. Without incentives, the earliest stage at which the production gain could be achieved is Round 5. For this case, the expected value is therefore the expected value of MSD from scenario (2) for four rounds, plus the expected value of MSD from scenario (4) for six rounds. Hence: 53.3*4 + 70*6 = 633.2. As a result, without any incentives, always picking CP has a higher expected total payoff than always choosing MSD for all production gain assignments.[19]

With incentives, the situation is reversed. For a participant always choosing MSD, the latest round in which the production gain can be realized is in Round 7. Therefore, the lowest expected payoff from choosing MSD with incentives will be the total of the expected value from scenario (3) (Rounds 1-4), plus the expected value from scenario (2) (Rounds 5-6), plus the expected value from scenario (4) (Rounds 7-10): 73.3*4+53.3*2+70*4 = 679.8. Hence the lowest possible expected payoff for continuous MSD adoption in the incentive's scenario exceeds the expected payoff for continuously choosing CP.[20]

These features of the experiment are intended to reflect in a stylized form the belief described above that farmers do not see sufficient gains over a moderate time horizon to take up MSD (or CA practices more generally). However, in the presence of a sufficient incentive, the gap can be bridged from the initially lower returns associated with MSD to the improved equilibrium once the gains from production of successive adoption have been realized.

5.3.8 Limitations

The FFE is designed to provide information about farmer CA adoption behaviour. However, a key concern is that we do not know if decisions in this controlled environment actually reflect decisions in the real world.[21] Levitt and List (2007) provide a useful framework of potential causes of divergence in the FFE from 'real' behaviour. Three are relevant here: anonymity, stakes, and artificiality.

First, our experiment was conducted privately, so subjects were not able to observe one another's decisions. This is in contrast to actual agricultural decision-making within a community which is generally public- farmers can simply observe one another's fields. As such, it may be the case that participants were more willing to deviate from social norms around appropriate practices or try a novel technology.

---

[19] Obviously, under discounting and risk aversion, this calculation changes, though discounting should also work in favor of choosing conventional practices.

[20] Though the differences between these payoffs are small, for a moderately risk-averse individual, that continuous MSD adoption is preferable may be easily inferred from the round-level pay-outs, without the need for calculating the overall expected value.

[21] There is evidence to suggest that at least some laboratory experiments generalize well (Herbst & Mas, 2015).

Second, while the payoffs in the experiment were designed to best reflect the properties of the technologies under consideration based on current research, the stakes in the experiment were necessarily much lower for participants than actual planting decisions. There was also no possibility of loss within the experiment- participants were guaranteed to make money. As a result, they may have felt willing to be less risk averse within the experiment than they would be on their own farm, where losses are not only possible, but could have significant negative welfare consequences.

Last, we consider artificiality. It is clear that there are aspects of the FFE that are not equivalent to the real world actions a farmer must undertake. Farmers do not realize the results of their decisions in the course of a few minutes, and willingness to make a monetary payment within an experiment of course does not fully capture how an individual's may make decisions in real life.

These are real limitations, and we believe the best way to fully evaluate the effect of incentives and peer information on CA adoption would be to run a long-term randomized control trial. However, given the money and time that such a study would require, we believe FFEs are a reasonable way to learn something about adoption decisions before investing in large, multi-year projects.

5.4 Quantitative Sample

5.4.1 Sample Design

The study sample for the baseline survey and the FFE includes FBO members in the 66 communities who were direct beneficiaries of the GASIP programme in 2018 or targeted to become GASIP beneficiaries in 2019. The sample design anticipated 20 beneficiary farmers in each community.[22] To construct the sample, GASIP provided a list of farmers that GASIP had recorded to be members in each of the 66 FBOs involved in the CA programme.

To validate the farmer lists received from GASIP, IPA conducted mobilization activities in which field staff located a community on the list and verified the members within the GASIP FBO in the community. Field staff first met with relevant local officials to introduce the project. Surveyors then verified the existence of the GASIP FBO and verified and updated the list of GASIP beneficiaries within the FBO. The verified list of GASIP members formed the sample and was used for the FFE randomization.

5.4.2 Sample Size Determination: Statistical Power

All principal outcome variables in the FFE are indicator variables for various definitions of adoption (0/1).[23] Power calculations conducted prior to implementation used standard assumptions about alpha (0.05) and power (0.8). Randomization was conducted at the individual level, simplifying the discussion of statistical power. Table 3 in Online Appendix A shows the minimum detectable effect size, expressed as the percentage point difference between the control group and a treatment group (or two treatment groups) for a range of different rates of control group adoption, over two different sample

[22] The final sample frame size was 1,328 individuals, of which 1324 completed the field experiment. Refer to section 3.2 "Sample characteristics" for more details on the composition of the final sample.

[23] This discussion is repeated from the project pre-analysis plan.

sizes: the initially expected full sample of 1,410 farmers, and two more conservative samples that includes roughly 5 and 10 percent attrition or refusal to participate in the experiment, respectively. The below assumes three groups of equal size, appropriate for the incentive randomization.

This table shows that in this range of number of observations, we are well powered to detect impacts between 5 and 10 percentage points. These are reasonable effect sizes, because given the very low adoption of MSD, the goal would be to substantially increase adoption. This analysis also means that we will be able to detect similarly sized differences between the two treatment groups, based on the mean in the group with lower adoption.

When considering the detectable impact for the peer information treatment, with only two treatment groups, we can detect a somewhat smaller difference of 4 to 8 percentage points. Given that the peer information treatment may be expected to have a smaller impact, this increased power is important. This analysis is presented in Table 4 of Online Appendix A.

Finally, we present the detectable effect sizes for the interaction of the two treatments in Table 5 of Online Appendix A. This is effectively an examination of six groups, meaning the detectable effect sizes are much larger mostly between 9 and 14 percentage points. Therefore, we will be able to detect only larger differences when considering the interaction of the two treatments, but that is not the main focus of the study. The main parameter of importance is the reference group mean, and we show our estimates for a range of values. The analysis is not extremely sensitive to changes in other parameters, such as an increase in power.

5.5 Survey instruments

5.5.1 Baseline Survey

Household level survey data was collected in a baseline survey launching on April 24, 2019 and concluding on June 25, 2019. The baseline survey covered basic household demographic information, household labour supply, detailed information on agricultural production, household assets, land holdings, and housing. We additionally collected information regarding knowledge and use of conservation agriculture techniques, perceptions of use of conservation agriculture by neighbours and other peers, risk and time preferences, and experiences with other government agricultural programmes.

The target respondent for the baseline survey is the individual identified to be part of the GASIP programme. All listed GASIP members within a household were subsequently invited to participate in the FFE. Main respondents were also replaced with their spouses or other adult decision makers who were farmers within the household when the main respondent was unavailable for more than 3 weeks.

5.5.2 Framed field experiment script

The experiment was conducted within a few days of completion of the baseline survey.[24] The enumerator had a set script, translated into six relevant local languages, which was read to the participant. The script began with an introduction giving a brief summary of the experiment, describing the incentives to participate, and clarifying that both the information obtained during the experiment and the amount of money that the participant received was private and confidential. The enumerator confirmed the participant's consent to participate.

The first section following the consent provided a short description of MSD. This was followed by a detailed description of how choices are made in the experiment, including comprehension questions to ensure that the participant understood the features of the experiment. Visual aids were used to facilitate the explanation, and the enumerator showed the participant a series of example choice sheets identical to those used in the experiment proper to demonstrate the different payouts associated with certain scenarios. After this explanation, the enumerator began the experiment.

Prior to conducting the FFE for the study, an extensive pilot was conducted in the field in December 2018. During the pilot researchers tested whether the questions asked during the FFE were appropriate to the local context. Careful attention was also paid to ensure that ensure the language was clear and respondents understood the instructions and decisions. Additionally, the pilot aided in calibrating payout amounts (both levels and the relative amount of costs, bonuses, and output) and testing the effectiveness of visual aids.

5.6 Field work

Data collection was conducted in the selected districts and communities of each of the four GASIP selected regions. There were six field teams each for both the baseline survey and the FFE. The number of teams was a function of the number of languages in the GASIP implementation region. Survey launch was staggered by region to allow for field work and questionnaire related challenges to be identified and resolved before rolling out to all regions and districts. The baseline and FFE surveys were each extensively piloted. After the completion of a baseline survey, respondents were given a gift of a bar of soap. In the FFE, respondents received monetary incentives based on their choices at the end of the experiment.

At the end of the baseline survey, the baseline enumerator informed respondents of the upcoming FFE team visit and set an appointment for this visit. The FFE visits were scheduled in the community 2 days following completion of the baseline survey. The respondent was given a paper voucher to present to the FFE surveyor. All GASIP members were eligible for the FFE, even when there was more than one per household. The FFE surveyor used the paper voucher as a method of verifying the individual was the correct respondent to complete the FFE.

[24] Generally, the team aimed to complete the experiment within 3 days, though the gap was sometimes larger if the respondent was not available at the time when the FFE team visited the community.

5.6.1 Field staff selection and training

Field staff were recruited between March and April 2019 in the four target regions (Northern, Upper East, Upper West and Brong Ahafo). Enumerators were recruited separately for both the baseline survey and the FFE. The two instruments consisted of different elements, thus necessitating enumerators with different skill sets. In particular, recruitment for the FFE team emphasized experience conducting FFEs. The two teams were trained in separate training sessions. For both surveys, the surveyor trainings were in two parts – the classroom training and field practice for each team. Selection and training were done taking into consideration the different language groups summarized below.

- Northern Region: Language – Dagbani/Mampruli and Likpakpa
- Upper East Region: Language – Gruni and Buli
- Upper West Region: Language – Daagare/Waali
- Brong Ahafo Region: Language – Twi/Bono

5.6.2 Data quality control

To monitor and ensure the data collected were of the highest quality, the following checks were employed:

Accompaniments and Monitoring– At least once every two days, surveyors were accompanied by either the Team Leader, Field Supervisor, Field Manager, or RA/SC to monitor data quality. Random spot checks were also carried out to observe and assess surveyor's performance during interviews and provide feedback where necessary.

Back checks – Back checks were conducted on a random selection of 10 percent of completed surveys. The randomization was done at the surveyor level, so that all surveyors would have 10 percent of their surveys back checked by the end of baseline data collection. The backcheck survey consisted of questions from the main baseline survey. When there were discrepancies between the two forms, a third party reached out to the respondent either in person or through a follow up phone call to confirm the correct response for the question(s) involved. Feedback from back checks were meant to encourage surveyors to probe and record accurate answers, and also check the stability of the questions being asked. In the end, about 25 per cent of the full baseline surveys were back checked.

High Frequency Checks (HFCs) were run to check data quality, including surveyor performance, programming errors, and identifying responses that could be further coded. An editor was hired to review HFC outputs and reach out to surveyors to correct errors whilst data collection was still on-going.

6. Formative study timeline

The GASIP programme was launched in 2015 and activities began in 2017. In 2018, after substantial delay relative to initial timelines, GASIP activities related to CA began in 30 communities. FBOs were formed, and each FBO began to manage a demonstration plot for practicing CA. Technical assistance was provided by Agriculture Extension Agents from MOFA and the CNTA. The second phase of GASIP activities – the addition of Brong Ahafo region, selection of additional 3 communities in each of the districts, and implementation started in April 2019, and continued until the end of the farming season in September 2019.

The qualitative component of the study was conducted in late 2017. A total of 8 Focus Group Discussions and 20 in-depth interviews were conducted in two communities in West Mamprusi district.

Piloting of the framed field experiment was conducted in December 2018. In February and March 2019, planning and preparations for the launch of the baseline and FFE surveys was carried out including recruitment of field staff. In late March and early April, both the FFE and the baseline survey trainings were completed. Field data collection started in late April and lasted till the end of June.

Data cleaning, analysis, and reporting of the baseline and FFE surveys was completed in June and July 2019. Policy engagement activities are scheduled for later in 2019, and an endline survey is potentially scheduled for 2020 after GASIP ends if incentives can be distributed in a manner conducive to evaluation and funding can be obtained.

7. Analysis and findings from the formative evaluation

7.1 Qualitative study results

The goals of the qualitative research were to understand the constraints to the implementation if CA and investigate the types of incentives preferred by farmers. The first set of focus groups and interviews were largely designed to understand what parts of the CA package farmers understood well, which ones they did not understand, and which ones they felt constrained against adopting. Farmers do perceive that farming conditions are becoming more challenging. In the past, plots were left fallow to regenerate nutrients, but almost all farmers now report that they cultivate their plots every year, with only large landholders able to afford leaving plots fallow for two or three years. Also, all farmers perceived that the rainfall pattern had become more unpredictable in the past decade, and that temperatures had risen. With a short rainy season lasting for 4-5 months where timing of operations is crucial for a good harvest, these climate-related trends were noted as being negative for farmers' productivity. As a result of these two trends, farmers in general were quite receptive to the idea of adopting CA techniques.

Next, there was a focus on understanding some further techniques currently in use, and what constraints existed in general. Most farmers reported using tractors to plot land in preparation for the season (rather than ploughing by hand), although tractor scarcity and untimely availability was a problem perceived by both men and women. Farmers also reported that a lack of access to fertiliser was a constraint hindering their productivity; however, in general they reported that they faced a lack of financial resources.

Regarding CA in particular, farmers were positive about both permanent soil cover and crop rotation. Many farmers suggested they already leave crop residues on their plots, to let livestock smash them and further fertilize plots. In addition, some farmers reported bringing food residues to their plot to further add organic matter. Farmers further reported that the proportion of fields in which crop residues were burned (a traditional method of clearing plots) had been dramatically reduced in recent years, though bush fires sometimes still affected plots. Meanwhile, almost all farmers, both men and women, suggested they had already adopted crop rotation, in order to achieve continuous cultivation on those plots.

MSD, on the other hand, was not received as well as the other two practices. Some farmers reported that the extreme level of erosion of their soil makes this practice inapplicable. Others, particularly women, were concerned that practicing MSD or no-till would imply an extra round of weeding. In sum, farmers were resistant to the idea of MSD practices. In a separate individual conversation, a chief from one of the two communities also informed us that although finding no-tillage a good practice on his plot, not spending money to have his plot ploughed by tractor would have created a false impression among community members that he lacked the financial resources to do it. Given his position in the community, this perception could create social problems.

Therefore, the first round of interviews suggested that CA is potentially beneficial to farmers in northern Ghana, given that they are facing increasing land scarcity and more erratic rainfall than before. Given the points that emerged from our discussion, we believe that Conservation Agriculture is a valid alternative for farmers facing land scarcity and the need to continuously farm their land, combined with erratic rainfalls and rising temperatures. However, while permanent soil cover and crop rotation have been well received by farmers, no-tillage is usually subject to more scepticism and hence its adoption might be more in need of incentives.

Based on the first set of interviews, the second set of interviews was designed to have farmers rate different incentives against one another, and to understand whether farmers really practiced crop rotation or not. Different incentives were considered for specific activities; in other words, the focus group moderator asked farmers whether specific incentives would nudge them to adopt MSD or permanent soil cover, and then farmers were asked to collectively rank the incentives at the end of each exercise. For crop rotation, to understand the depth of its use 5 participants were chosen for additional interviews after the focus group, for a total of 20 participants. Each was asked about the crops they grew in the previous two years, what they were planning to grow in the following year, and to draw their fields and what was planted where each year.

In discussing MSD, three different potential incentives were tested. First, as a main problem with MSD is the growth of additional weeds in initial years, herbicides can reduce the increased labour intensity required for weeding. The incentive proposed to farmers was 8 litres of herbicides, or enough to cover 1 acre. A second complaint about MSD is that the soil is too depleted to not plough; a potential incentive in this case is fertilizer. The incentive proposed, then, was a coupon for fertilizer approximately equivalent in value to the herbicide. Finally, farmers were asked about cash as a third incentive, again equivalent in value, to pay for additional weeding.

The preferred incentive for MSD was fertilizer, followed by herbicides and then cash. When asked about their preferences, farmers were concerned that cash would potentially get spent in a non-productive way, whereas fertilizer and herbicide would be more clearly be only useful for their farms. Consequently, cash was the least preferred option. In general, though, farmers expressed the opinion that if they were given incentives to conduct MSD, they would do so at least on part of their land.

Note that although more farmers already reported doing some permanent soil cover, the focus groups still asked about preferred incentives for permanent soil cover. Here, the incentives differed—the FGDs were asked specifically about hand seeders, which are a tool that allow farmers to jab into covered soil relatively easily to create a hole for planting. Hand seeders are not widely available in the north but were mentioned IFAD and CNTA employees as potential incentives. Farmers were also asked about wheelbarrows, which can be used to transport residues from one part of a plot to another to retain soil cover; gumboots, which limit the potential for snake or scorpion bites in the field; and cash. The hand seeder was most preferred by farmers, as farmers could easily envision it as a labour-saving device. The second choice differed by men and women; women preferred the wheelbarrow, as it would help them cover larger distances quickly with bulky residues, while men preferred the gumboots. Again, cash was least preferred.

Finally, the drawing exercise demonstrated that farmers indeed rotate their crops around their plots. The exercise did find that women knew less about the farming activities of other household members than men, and some within household swaps took place. For example, women traded plots with men soon after giving birth so they would not have to walk as far to cultivate the field.

In general, the qualitative interviews suggested that MSD is the least adopted CA technique in northern Ghana; hence, the FFE was designed around promoting MSD. Although in the FFE cash was the only feasible incentive option, GASIP did implement in-kind incentives for farmers in the 2019 agricultural season.

7.2 Framed field experiment: Analysis strategy

The framed field experiment was designed based on the results of the qualitative evaluation, as described in Section 5. Here we outline the analytical approach used to answer the research questions posed in Section 4. The analysis is based on the research project pre-analysis plan, and we indicate where we deviate from the plan.

7.2.1 Outcome variables

All outcome variables are based on the choice that participants make in the FFE: whether to adopt CP or MSD. Analysis is performed both at the participant level and on the participant-round level. On the participant level we analyse the following outcome variables: number of rounds in which MSD was chosen (out of 10), whether or not the production gain was achieved by that participant, and a measure of dis-adoption, defined as one if the participant ever chose CP after having previously chosen MSD, and zero otherwise. In analysis performed at the participant-round level, the outcome is simply whether MSD was chosen in that round.

7.2.2 Empirical specifications

In order to study the impacts of the incentive and information treatments on the adoption of MSD, we estimate the following regressions using ordinary least squares at the participant level:

$$Y_i = \alpha + \beta_1 Incentive_i + \beta_2 Information_i + \beta_3 6s_i + \beta_4 7s_i + X_i + \delta_i + \varepsilon \quad (1)$$

Incentive and *Information* are indicators for being in the incentive and information treatment groups, and *6s* and *7s* are indicators for being in the groups that received the production gain after choosing MSD for 6 and 7 seasons respectively.[25] X is a vector of control variables, δ are stratification cell fixed effects (community dummies), and ε is a robust error term. [26] This specification differs from that listed in the pre-analysis plan only in that we initially indicated that we would show treatments in separate specifications; however, because they are randomized, there is no reason not to include them in the same regression. This specification shows the main effects of each of the treatments.

In the next specification, we study the interaction of the incentive treatment with the randomized gain round. This specification allows us to understand whether adoption behaviour and the effectiveness of the incentive is affected when the production gain does not occur immediately after the incentives expire:

$$Y_i = \alpha + \beta_1 Incentive_i + \beta_2 Information_i + \beta_3 6s_i + \beta_4 7s_i + \beta_5 Incentive_i X6s_i + \beta_6 Incentive_i X7s_i + X_i + \delta_i + \varepsilon \quad (2)$$

Third, we study the impact of receiving incentives or information only versus receiving both:

$$Y_i = \alpha + \beta_1 IncentiveOnly_i + \beta_2 InformationOnly_i + \beta_3 Incentive\&Information_i + \beta_4 6s_i + \beta_5 7s_i + X_i + \delta_i + \varepsilon \quad (3)$$

Finally, we study the interactions of these categories with the gain round. This specification was not included in the pre-analysis plan, but it is a natural extension of the analysis:

$$Y_i = \alpha + \beta_1 IncentiveOnly_i + \beta_2 InformationOnly_i + \beta_3 Incentive\&Information_i + \beta_4 6s_i + \beta_5 7s_i + \beta_6 IncentiveOnly_i X6s_i + \beta_7 IncentiveOnly_i X7s_i + \beta_8 InformationOnly_i X6s_i + \beta_9 InformationOnly X7s_i + \beta_{10} Incentive\&Information_i X6s_i + \beta_{11} Incentive\&Information X7s_i + X_i + \delta_i + \varepsilon \quad (4)$$

These regressions are all run at the participant level. We additionally analyse analogous specifications at the participant-round level, where standard errors are clustered at the participant level. We conduct these analyses in the full sample, and then in the sub-sample of rounds one to four and rounds five to seven. Since the treatments were only active in the first four rounds, the latter specification helps us explore persistent effects in rounds where treatments were no longer active.

[25] Both treatment variables and the gain variable are coded based on random assignment. Due to some enumerator errors (as a result of conducting an experiment using an incorrect ID on the tablet) there are a small number of cases (2 observations for the information treatment, 3 for the incentive & gain round assignments) where the implemented treatment did not match the assigned. The results of the analysis are not meaningfully altered by using actual assignment.

[26] Control variables include: household size, gender, age, risk and time preferences, value of assets owned, number of CA techniques used last season, value of crop production, number of GASIP crops grown, household has electric light, household has toilet access, household has cement walls, household has cement floors, household has metal roof, household grew tubers, the rainfall assigned in the practice round, and indicators for missing data.

Lastly, in order to analyse the impact of the type of information received in the information treatment, we conduct an analysis, among only those who received the information treatment, at the participant-round level, for the decisions made in the first four rounds:

$$Y_i = \alpha + \beta_1 InfoB_i + \beta_2 InfoC_i + \beta_3 InfoD_i + \beta_4 PoorRainfall_i + X_i + \delta_i + \varepsilon \quad (5)$$

The information indicators represent the following information received:

Info A: Neighbor used CP (which is the omitted category);
Info B: Neighbor used MSD for the first time;
Info C: Neighbor used MSD for the last ten years; and
Info D: Neighbor abandoned MSD (used CP after having used MSD).

PoorRainfall is equal to one if the rainfall in the previous season (i.e. the rainfall experienced by the neighbor/peer in the reported information) was poor.

We additionally estimate a specification in which we interact the types of information with the previous season's rainfall, as the information conveyed depends on the rainfall. We also examine the impact of the information types separately by incentive treatment group. This will help to understand whether certain types of information are effective only in conjunction with receiving an incentive. These specifications were not specified in the pre-analysis plan.

7.2.3 Treatment balance

While treatment was assigned randomly, it remains important to ensure that treatment and control groups do not differ in important ways in their individual characteristics. To do so,

Table 5 presents group means and balance tests for the two assigned treatments. We additionally perform the same tests across assignment to the gain round (Table 5 of Online Appendix A).

Table 5: Balance Comparison, Assigned Treatments

	Incentives (Control) (1)	Incentives (Treatment) (2)	p-value (3)	Information (Control) (4)	Information (Treatment) (5)	p-value (6)
Household size	9.98	10.04	0.736	10.14	9.90	0.391
Is female	0.49	0.55	0.047	0.51	0.55	0.083
Age	41.40	40.75	0.314	41.34	40.59	0.271
Risk preference	7.21	6.38	0.022	6.44	6.87	0.184
Time preference	5.37	4.81	0.015	4.99	5.01	0.904
Value of household assets	8797.42	8172.97	0.641	9192.15	7565.43	0.129
# CA practices last season	2.33	2.42	0.332	2.41	2.36	0.456
Crop value (estimated)	2629.72	2216.76	0.318	2274.00	2433.61	0.579
# GASIP crops last season	1.61	1.63	0.658	1.63	1.61	0.597
Has electric light	0.58	0.59	0.782	0.59	0.59	0.864
Has toilet access	0.51	0.55	0.161	0.52	0.54	0.284
Dwelling: Cement walls	0.18	0.17	0.743	0.18	0.16	0.540
Dwelling: Cement floor	0.74	0.75	0.991	0.73	0.76	0.254
Dwelling: Metal roof	0.80	0.81	0.776	0.81	0.81	0.988
Rainfall outcome in practice	0.47	0.51	0.180	0.48	0.52	0.218
Omnibus test			0.212			0.350

Note: Columns (1), (2), (4) & (5) present the mean values for each group. Columns (3) and (6) report the p-value associated with the coefficient of an indicator variable for each group, regressed on the variable of interest with community-level fixed effects.
Source: Baseline data.

In general, the treatment and control participants in the interventions do not differ in statistically significant ways for either the incentives or information treatments. Both groups report similar demographic characteristics, assets and agricultural production levels, and dwelling characteristics. The exception is for the risk and time variables in the incentives treatment, where there are statistically significant differences between treatment and control treatments (though the magnitudes of the differences is not large). In line with the design from the pre-analysis plan, we control for these variables in the regressions presented below.

7.3 Framed field experiment: Results

7.3.1 Descriptive results

We first examine adoption behaviour in the experiment descriptively. Table 6 displays the mean of each of the three main participant-level outcomes by each of three randomizations: incentives, information, and gain round. Overall adoption is high, the average number of rounds MSD is chosen ranges from 7.8 to 8.4 depending on the treatment group. The production gain is achieved in 68 to 78 percent of cases. However, non-negligible numbers of participants try MSD and then return to CP at some point.

Table 6: Experiment Outcomes

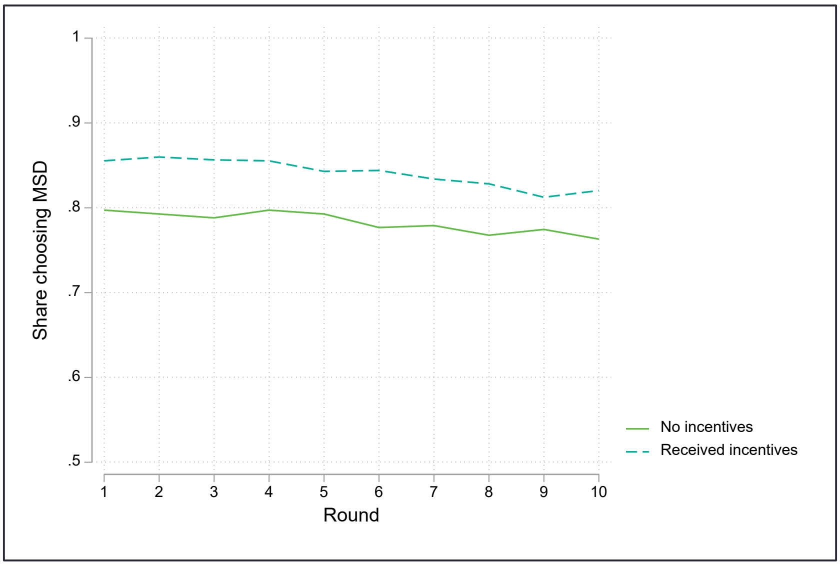
	Incentive treatment		Information treatment		Gain round		
	No incentives	Incentives	No info	Info	Round 5	Round 6	Round 7
No. rounds MSD	7.83	8.41	8.25	8.18	8.42	8.09	8.14
Achieved gain	0.68	0.75	0.73	0.73	0.78	0.72	0.69
Abandoned MSD	0.34	0.27	0.29	0.29	0.26	0.30	0.31

Note: Columns represent the mean for each group
Source: FFE data

These raw means suggest that the incentive treatment increases adoption of MSD and reduces the likelihood that someone will dis-adopt. There appears to be no impact of the information treatment in the raw means. The examination of the gain round means shows higher adoption and lower dis-adoption among those who received the gain in Round 5 versus Round 6 or 7.

It is also informative to examine adoption by round, to examine how behaviour may have changed over the course of the experiment, in particular because incentives and information were only offered for four rounds, but the production gain only occurred after five, six, or seven seasons of continuous adoption. Figure 2 shows adoption by round separately for those who received incentives and those that did not. Across rounds, adoption rates for the incentive groups are always higher than those in the no incentives group, and this difference is consistent over time. In both groups, adoption is steady across the first four rounds, and then begins to decline slightly.

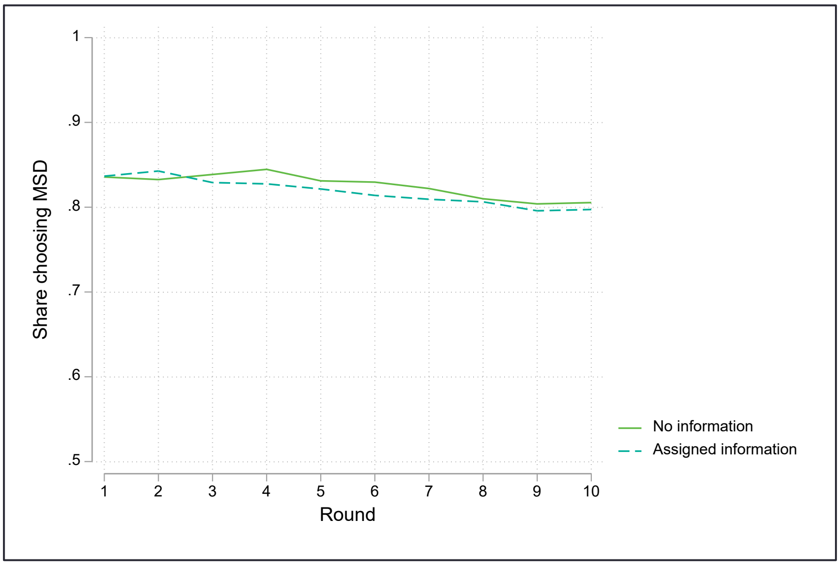
Figure 2: Share choosing MSD, by incentive treatment status



Source: FFE data

Figure 3 shows the same information separately by information treatment. The same time trend is visible, but there is little to no difference in average choices between the two treatment groups.

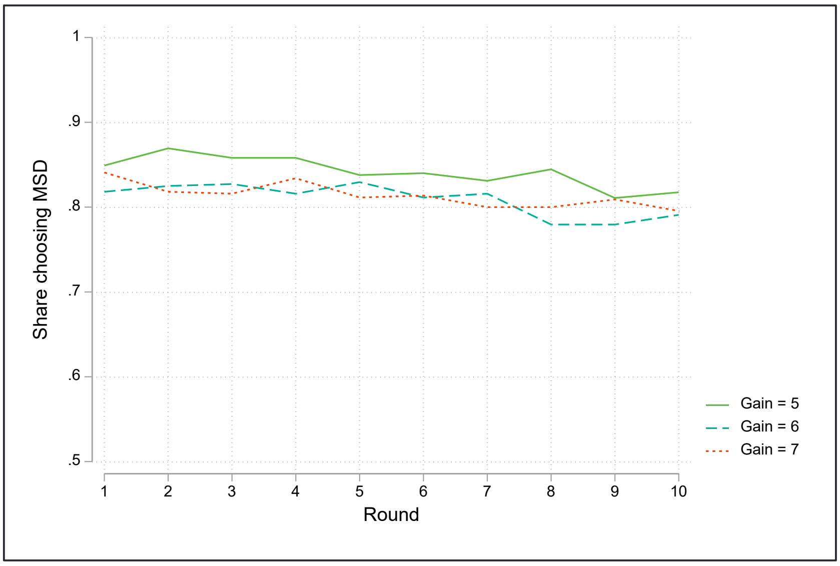
Figure 3: Share choosing MSD, by information treatment status



Source: FFE data

Finally, Figure 4 shows by-round adoption separately by the randomly assigned gain round group. No pattern attributable to the receipt of the production gain after certain seasons is immediately visible. The adoption rates for those receiving the gain after Round 5 are consistently higher than the other two groups. This is somewhat perplexing given that in the first five rounds participants would not have known which gain round group they were in. This does not appear to be readily attributable to an imbalance in a particular covariate (such as risk or time preferences), though the omnibus test included in the balance table does reject the null hypothesis that assignment is not predicted by the full set of controls (Online Appendix A, Table 5).

Figure 4: Share choosing MSD, by gain round



Source: FFE data

7.3.2 Regression results

We now turn to the regression analysis constituting our main analysis, beginning with the participant-level analysis.Table 7 shows the results of estimating regression specification (1). It displays the results of the three treatment randomizations. Table 7 of Online Appendix A reproduces these results and displays the coefficients of the related control variables. Few control variables have predictive power; however, women are less likely to achieve the gain and more likely to abandon MSD. The most robust correlation is with toilet access; households reporting toilet access are significantly more likely to adopt MSD, and the size of the coefficient is comparable to the coefficient on the incentive treatment.

Table 7: Impact of treatments on MSD adoption

	(1)	**(2)**	**(3)**
	Dependent variable:		
	Number rounds MSD	**Achieved gain**	**Abandoned MSD**
Incentive treatment	0.598***	0.083***	-0.074***
	(0.176)	(0.026)	(0.026)
Gain round: Round 6	-0.222	-0.038	0.022
	(0.192)	(0.028)	(0.029)
Gain round: Round 7	-0.275	-0.090***	0.044
	(0.186)	(0.028)	(0.029)
Information treatment	-0.049	0.004	-0.009
	(0.157)	(0.023)	(0.024)
Mean: No incentives	7.829	0.677	0.337
Mean: No information	8.253	0.727	0.293
Mean: Gain round = 5	8.417	0.777	0.261
Adjusted R-squared	0.119	0.114	0.083
Observations	1,324	1,324	1,324

Note: Ordinary least squares regression, with stratification-cell (community) fixed effects. Standard errors in parentheses. Control variables are included in the specification, but not reported. *,**,*** indicate significance at the 10%, 5% and 1% level respectively.
Source: Baseline and FFE data.

Returning to the analysis of treatment impact in Table 7, the incentive treatment results in an economically and statistically significant impact on adoption. Participants in the incentive group choose MSD on average in 0.598 more rounds, an increase of 7.6 percent relative to the control group. They were 8.3 percentage points more likely to achieve the production gain (12 percent increase) and were 7.4 percentage points less likely to abandon MSD once they had chosen it (22 percent decrease).

There is some evidence that those who received the gain after rounds 6 or 7 adopt MSD less overall, but the estimates are not statistically significant for the most part. The one significant outcome is that participants who receive the gain round in round 7 are 9 percentage points less likely to achieve the gain than those who receive it in round 5. The estimates of the impact of the information treatment are close to zero in this specification. This specification is also estimated in a participant-round format, shown in Table 8 of Online Appendix A, both for the full sample, and separately for rounds 1-4 and rounds 5-10. The patterns are similar, and consistent across early and late rounds. Interestingly, the effects of the gain rounds, though imprecise, are if anything larger in early rounds.

Table 8: Impact of incentives and gain round on MSD adoption

	(1)	**(2)**	**(3)**
	Dependent variable:		
	Number rounds MSD	**Achieved gain**	**Abandoned MSD**
Incentive treatment	0.948***	0.139***	-0.100**
	(0.293)	(0.043)	(0.046)
Gain round: Round 6	0.367	0.049	-0.016
	(0.352)	(0.051)	(0.054)
Gain round: Round 7	-0.205	-0.070	0.031
	(0.372)	(0.054)	(0.055)
Incentives x Gain Round 6	-0.903**	-0.134**	0.057
	(0.423)	(0.063)	(0.066)
Incentives x Gain Round 7	-0.113	-0.030	0.019
	(0.431)	(0.064)	(0.066)
Information treatment	-0.034	0.006	-0.010
	(0.156)	(0.023)	(0.024)
P-values:			
Incentives + Incentives x Round 6	0.882	0.909	0.351
Incentives + Incentives x Round 7	0.009	0.022	0.086
Mean: No incentives	7.829	0.677	0.337
Mean: No information	8.253	0.727	0.293
Mean: Gain round = 5	8.417	0.777	0.261
Adjusted R-squared	0.122	0.116	0.082
Observations	1,324	1,324	1,324

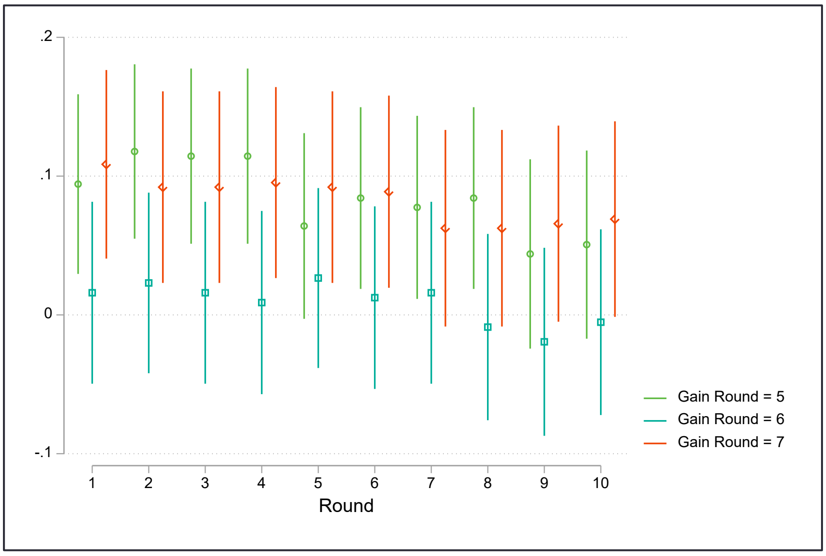
Note: Ordinary least squares regression, with stratification-cell (community) fixed effects. Standard errors in parentheses. Control variables are included in the specification, but not reported. *,**,*** indicate significance at the 10%, 5% and 1% level respectively. Source: Baseline and FFE data.

Table 8 presents the results of regression specification 2, examining whether the impact of the incentive treatment is differential by gain round group. The impact of the incentive treatment when the gain round is round 5 (incentive treatment main effect) remains positive and statistically significant. The interaction of the round 6 and round 7 gain round indicators with the incentive treatment are both of opposite sign from the main effects, and for round 6, large and statistically significant. The interaction term in round 6 is large enough that it cancels out the incentive effect completely for that group. The round 7 coefficients are smaller, and not statistically different from the main effect. The total incentive effect for gain round 7 is still statistically different from zero.

A similar pattern of results is observed in the by-round regressions (Online Appendix A, Table 9). Because the gain round was unknown to participant at the beginning of the experiment, we would not expect to see an interaction effect in rounds 1 through 4. However, the negative interaction between incentives and Round 6 is, if anything, stronger in the early rounds.

To further understand this result, we plot the impact of the incentive treatment for each incentive-gain round combination, separately by round, in Figure 5. Across rounds this effect is similar for gain rounds 5 and 7, and lower for gain round 6. The incentives appear not to have been effective among those randomly assigned to gain round 6, even in the early rounds.

Figure 5: Coefficient of assigned incentives x round, by gain round



Source: FFE data

Table 9: Impact of treatments on MSD adoption, interacted treatments

	(1) Dependent variable: Number rounds MSD	(2) Achieved gain	(3) Abandoned MSD
Incentive treatment only	0.871***	0.123***	-0.084**
	(0.251)	(0.037)	(0.038)
Information treatment only	0.310	0.057	-0.022
	(0.295)	(0.043)	(0.045)
Incentive and information	0.645**	0.101***	-0.086**
	(0.256)	(0.037)	(0.037)
Gain round after Round 6	-0.209	-0.036	0.021
	(0.191)	(0.028)	(0.030)
Gain round after Round 7	-0.276	-0.090***	0.044
	(0.186)	(0.028)	(0.029)
P-values:			
Incentives only = Information only	0.018	0.065	0.099
Incentives only = Both	0.219	0.436	0.925
Information only = Both	0.169	0.223	0.086
Mean: No treatments	7.685	0.653	0.338
Mean: Gain Round = 5	8.417	0.777	0.261
Adjusted R-squared	0.120	0.115	0.082
Observations	1,324	1,324	1,324

Note: Ordinary least squares regression, with stratification-cell (community) fixed effects. Standard errors in parentheses Control variables are included in the specification, but not reported. *,**,*** indicate significance at the 10%, 5% and 1% level respectively.
Source: Baseline and FFE data

Table 9 reports the estimation of regression specification (3) and examines the impact of receiving each treatment alone or receiving both together. The results remain suggestive of zero impact of the information treatment and are not indicative of any complementary effects of information and incentives. The coefficients in the incentives only and incentives plus information treatments are similar in magnitude, statistically different from zero, and not statistically distinguishable from each other. The information only coefficients are small and not statistically different from zero. However, we cannot reject that the coefficients in the incentives and information group are equal to the information only treatment in two of three cases. As in other cases, the by-round analysis shows similar patterns (Online Appendix A, Table 10).

Table 10: Impact of interacted treatments and gain round on MSD adoption

	(1)	**(2)**	**(3)**
	Dependent variable:		
	Number rounds MSD	**Achieved gain**	**Abandoned MSD**
Incentive treatment only	0.943**	0.178***	-0.122*
	(0.409)	(0.060)	(0.067)
Information treatment only	0.028	0.057	-0.007
	(0.493)	(0.070)	(0.076)
Incentive and information	1.001**	0.161***	-0.083
	(0.409)	(0.061)	(0.067)
Gain round after Round 6	0.104	0.054	-0.006
	(0.522)	(0.074)	(0.077)
Gain round after Round 7	-0.361	-0.076	0.049
	(0.529)	(0.076)	(0.080)
Incentives x Round 6	-0.316	-0.117	0.076
	(0.608)	(0.089)	(0.093)
Incentives x Round 7	0.139	-0.038	0.035
	(0.608)	(0.089)	(0.095)
Information x Round 6	0.559	-0.004	-0.019
	(0.697)	(0.102)	(0.108)
Information x Round 7	0.328	0.016	-0.033
	(0.759)	(0.109)	(0.112)
Both x Round 6	-0.934	-0.157*	0.019
	(0.624)	(0.090)	(0.093)
Both x Round 7	-0.074	-0.010	-0.034
	(0.617)	(0.091)	(0.096)
P-values:			
Incentives = Information	0.027	0.039	0.068
Incentives = Both	0.847	0.714	0.447
Information = Both	0.018	0.078	0.228
Mean: No treatments	7.685	0.653	0.338
Mean: Gain Round = 4	8.417	0.777	0.261
Adjusted R-squared	0.122	0.115	0.079
Observations	1,324	1,324	1,324

Note: Ordinary least squares regression, with stratification-cell (community) fixed effects. Standard errors in parentheses Control variables are included in the specification, but not reported. *,**,*** indicate significance at the 10%, 5% and 1% level respectively.
Source: Baseline and FFE data

In Table 10 we examine the interaction of the combined treatments with the gain round. We are limited in power to draw any strong conclusions here, but the largest interaction effects for the Round 6 pattern observed in Table 8 seem to be coming from those that received both incentives and information. This pattern is repeated in the by-round results (Online Appendix A, Table 11). Next, we more carefully examine the impact of the information treatment, by studying the different types of information that were offered. This is regression specification 5. The results are presented in Table 11.

Table 11: Impact of different information types on MSD adoption

	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent variable: Chose MSD					
	Information treatment		Information, no incentives		Information, incentives	
Info B: Used MSD (first time)	0.030	0.024	0.043	0.012	0.016	0.027
	(0.020)	(0.024)	(0.035)	(0.042)	(0.023)	(0.029)
Info C: Used MSD (last 10 years)	0.037*	0.051**	0.013	0.031	0.040*	0.055*
	(0.020)	(0.024)	(0.037)	(0.042)	(0.024)	(0.029)
Info D: Abandoned MSD	-0.001	0.004	-0.025	-0.035	0.003	0.015
	(0.020)	(0.025)	(0.038)	(0.044)	(0.024)	(0.029)
Poor rainfall last round	-0.012	-0.002	-0.022	-0.039	-0.012	0.018
	(0.015)	(0.031)	(0.028)	(0.058)	(0.018)	(0.036)
Info B x Poor rainfall		0.018		0.093		-0.034
		(0.041)		(0.074)		(0.049)
Info C x Poor rainfall		-0.042		-0.060		-0.046
		(0.043)		(0.085)		(0.049)
Info D x Poor rainfall		-0.015		0.030		-0.038
		(0.044)		(0.081)		(0.052)
P-values:						
Info B = Info C	0.683	0.226	0.396	0.640	0.281	0.289
Info B = Info D	0.113	0.393	0.058	0.279	0.573	0.669
Info C = Info D	0.050	0.039	0.303	0.130	0.111	0.145
Info B + Info B x Prev Rainfall = 0		0.209		0.089		0.864
Info C + Info C x Prev Rainfall = 0		0.082		0.085		0.648
Info D + Info D x Prev Rainfall = 0		0.587		0.437		0.699
Mean: A - Neighbor used CP	0.812	0.812	0.788	0.788	0.826	0.826
Adjusted R-squared	0.106	0.105	0.148	0.149	0.138	0.137
Observations	2644	2644	904	904	1740	1740

Note: Sample restricted to information treatment group. Ordinary least squares regression, with stratification-cell (community) and round fixed effects. Standard errors in parentheses Control variables are included in the specification, but not reported. *,**,*** indicate significance at the 10%, 5% and 1% level respectively.
Source: Baseline and FFE data

Among the full sample, with no interactions (column 1), there is an effect of being told that the neighbour had used MSD for at least 10 seasons. The coefficient is 3.7 percentage points, a 4.6 percent increase relative to being told your neighbour had used CP. The coefficient for the neighbour having used MSD once, is 3 percentage points, but not statistically significant.

In column 2 we interact the information with the rainfall from the previous season, because the rainfall outcome in the previous season affects what the participant was told about how much the neighbour earned. This interaction is of particular interest for information category C, since (within the experiment) the net payoff for continuous MSD adoption under poor rainfall is higher than for either CP or MSD with recent adoption. As a result, one might anticipate the interaction of category C with poor weather to be positive, reflecting this resilience aspect of MSD adoption.

When the rainfall is normal, the effect of being told your neighbour had used MSD for 10 seasons (and therefore received the production bonus) is now 5 percentage points. The interaction term for poor rainfall is -4.2 percentage points, so the total effect of this information when rainfall is poor is near zero. However, the interaction term is not statistically significant. This pattern is not repeated for the receiving the information that the neighbour uses MSD for the first time. There are no statistically significant effects, and all coefficients are smaller.

In columns three through six, we repeat this analysis, separately for those who received incentives and those who did not. While there is some evidence that the effects are stronger for those who were offered incentives, the pattern is not strong enough to draw any definitive conclusions. Overall this is evidence that when promoting a technology like CA where there are deferred benefits, observing peers who have actually experienced those benefits can be a useful tool for promoting adoption.

7.3.3 Heterogeneity analysis

We examine the differential impacts of our treatment along four different dimensions: gender, risk preferences, time preferences, and type of GASIP community (old and new). The first three were specified in the pre-analysis plan, and the last is an exploratory analysis. We believe it is important because the old GASIP communities will have had a year of exposure to the programme and education regarding CA techniques. It is useful to know how incentive and information interventions may affect participants with different levels of knowledge differently. We focus on regression specification (1) for this analysis.

Table 12: Impact of treatments on MSD adoption, by gender

	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent variable:					
	Number rounds MSD		**Achieved gain**		**Abandoned MSD**	
	Male	**Female**	**Male**	**Female**	**Male**	**Female**
Incentive treatment	0.487**	0.485*	0.075**	0.055	-0.080**	-0.059
	(0.246)	(0.268)	(0.037)	(0.038)	(0.039)	(0.038)
Gain round: Round 6	-0.330	-0.048	-0.030	-0.029	-0.018	0.028
	(0.273)	(0.288)	(0.042)	(0.042)	(0.045)	(0.042)
Gain round: Round 7	-0.434*	-0.196	-0.114***	-0.075*	0.052	0.038
	(0.254)	(0.277)	(0.040)	(0.041)	(0.043)	(0.042)
Information treatment	-0.016	-0.005	0.032	-0.009	-0.028	0.005
	(0.226)	(0.226)	(0.034)	(0.033)	(0.036)	(0.034)
Mean: No incentives	8.054	7.595	0.701	0.651	0.326	0.349
Mean: No information	8.393	8.116	0.738	0.716	0.287	0.299
Mean: Gain round = 5	8.600	8.245	0.800	0.755	0.260	0.262
Adjusted R-squared	0.135	0.114	0.104	0.120	0.062	0.108
Observations	626	698	626	698	626	698

Note: Ordinary least squares regression, with stratification-cell (community) fixed effects. Standard errors in parentheses Control variables are included in the specification, but not reported. *,**,*** indicate significance at the 10%, 5% and 1% level respectively.
Source: Baseline and FFE data

We first examine differential effects by gender in Table 12. In general, the impacts of the incentive treatment are similar for men and women, though the results for women are slightly less precise. Table 13 similarly shows the main results estimated separately by old and new GASIP communities. Again, there is little evidence of strong differences across these groups. Similar analysis is performed for risk and time preferences, shown in Tables 11 and 12 in Online Appendix A.[27] Overall these analyses do not display a strong pattern, but there is suggestive evidence that the incentives are more effective among those who are most impatient.

[27] Risk and time preferences are estimated using a "staircase" measure, as described by Falk et. al. (2016).

Table 13 – Impact of treatments on MSD adoption, by community type

	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent variable:					
	Number rounds MSD		**Achieved gain**		**Abandoned MSD**	
	Original community	**New community**	**Original community**	**New community**	**Original community**	**New community**
Incentive treatment	0.705***	0.473*	0.077**	0.079**	-0.047	-0.092**
	(0.232)	(0.252)	(0.036)	(0.036)	(0.037)	(0.037)
Gain round: Round 6	-0.093	-0.413	-0.027	-0.064	-0.001	0.057
	(0.242)	(0.287)	(0.039)	(0.040)	(0.041)	(0.041)
Gain round: Round 7	-0.123	-0.403	-0.054	-0.117***	-0.006	0.085**
	(0.237)	(0.274)	(0.039)	(0.039)	(0.042)	(0.040)
Information treatment	-0.147	0.037	-0.015	0.021	-0.009	-0.008
	(0.198)	(0.231)	(0.032)	(0.033)	(0.034)	(0.033)
Mean: No incentives	8.321	7.443	0.746	0.622	0.269	0.390
Mean: No information	8.866	7.753	0.805	0.663	0.242	0.334
Mean: Gain round = 5	8.874	8.049	0.828	0.736	0.237	0.280
Adjusted R-squared	0.046	0.111	0.056	0.109	0.071	0.071
Observations	589	735	589	735	589	735

Note: Ordinary least squares regression, with stratification-cell (community) fixed effects. Standard errors in parentheses Control variables are included in the specification, but not reported. *,**,*** indicate significance at the 10%, 5% and 1% level respectively.
Source: Baseline and FFE data.

Finally, we examine the impact of the type of information received separately according to these variables. We show the results by gender in Table 14.

Table 14: Impact of different information types on MSD adoption, by gender

	Information treatment			
	(1)	(2)	(3)	(4)
	Dependent variable = Chose MSD			
	Male	Female	Male	Female
Info B: Used MSD (first time)	0.030	0.030	0.042	0.022
	(0.028)	(0.027)	(0.035)	(0.032)
Info C: Used MSD (last 10 years)	0.086***	0.004	0.081**	0.040
	(0.027)	(0.028)	(0.033)	(0.032)
Info D: Abandoned MSD	-0.003	0.001	0.010	-0.003
	(0.028)	(0.029)	(0.033)	(0.035)
Poor rainfall last round	-0.014	-0.015	0.002	0.003
	(0.021)	(0.021)	(0.044)	(0.043)
Info B x Poor rainfall			-0.036	0.033
			(0.057)	(0.057)
Info C x Poor rainfall			0.015	-0.107*
			(0.057)	(0.062)
Info D x Poor rainfall			-0.046	0.014
			(0.064)	(0.060)
P-values:				
Info B = Info C	0.035	0.323	0.241	0.561
Info B = Info D	0.220	0.297	0.311	0.464
Info C = Info D	0.001	0.931	0.020	0.202
Info B + Info B x Prev Rainfall = 0			0.886	0.253
Info C + Info C x Prev Rainfall = 0			0.419	0.198
Info D + Info D x Prev Rainfall = 0			0.644	0.604
Mean: A - Neighbor used CP	0.823	0.803	0.823	0.803
Adjusted R-squared	0.172	0.148	0.171	0.150
Observations	1192	1452	1192	1452

Note: Sample restricted to information treatment group. Ordinary least squares regression, with stratification-cell (community) and round fixed effects. Standard errors in parentheses Control variables are included in the specification, but not reported. *,**,*** indicate significance at the 10%, 5% and 1% level respectively.
Source: Baseline and FFE data.

The results for the other variables are not shown and do not exhibit any consistent patterns. In the full sample of participants in the information treatment (columns 1 and 2), we observe that the impact of receiving information that your neighbour used MSD for 10 seasons has a large, positive impact on adoption for male participants only; the coefficient for females is close to zero. When examining the interaction with the previous season’s rainfall (columns 3 and 4), there is little difference for men, they react to the MSD information regardless of the weather. The interaction term between receiving information that a neighbour had used MSD for 10 years and poor weather for women is large and negative. The evidence is suggestive that women react positively to the information when the weather was good, and negatively when the weather was bad. However, these results are not precisely estimated. In Table 14 of Online Appendix A, we re-estimate this analysis separately for the incentive and no incentives group. The patterns in the full sample seem to be completely concentrated among those participants who also received incentives.

8. Implications of formative study findings

8.1 Implications for the intervention

The theory of change that we described at the beginning of the paper suggested that farmers first needed to learn about CA techniques, then initially adopt and continue to use CA for several seasons before CA would lead to either yield improvements or a reduction in yield variance. As the FFE was conducted in some groups that had received CA extension in 2018, we find that indeed there is reasonable knowledge about CA techniques in those communities, but the initial adoption lags behind the knowledge of techniques.

Our focus groups and the FFE both suggest that incentives could improve CA adoption. In the second round of focus groups, when we asked specifically about incentives and whether such incentives would lead farmers to try CA, farmers generally agreed that incentives would induce them to try out CA. Indeed, farmers exposed to incentives were 7.6 percent more likely to choose MSD, and about 8 percent more likely to achieve the production gain built into the experiment. As framing issues might have led to high adoption among the control group within the FFE (about 76 percent on average), it is worth noting that there was not much room for additional adoption among the incentives treatment group. We would expect control adoption to be lower in the field, and incentives could have an even larger impact.

The next step in our theory of change is for farmers to maintain adoption in CA for several seasons. Here, there are three interesting points from the FFE. First, not surprisingly incentives are effective. However, we did design the incentives over several periods, so it is not as clear how a one-time incentive like a hand-seeder or a wheelbarrow might work to maintain adoption. Second, we observe continued higher take-up even after incentives have been withdrawn, suggesting that targeting incentives over a fixed period could help farmers overcome the initial costs and uncertainty associated with adoption of CA practices. It is particularly notable that MSD adoption was sustained for farmers between the withdrawal of incentives and the point at which the production gain was realized.

Third, we note that there are no average effects overall of the information treatment. However, we do find a positive effect among treated individuals who were told that a peer had successfully adopted CA over a long time period. Therefore, whereas watching what one's peers in the FBO are doing might not be effective, the demonstration plot might be effective if it can be maintained for a long enough time period. As such our theory of change should be modified to suggest the type of long-term information that might be more relevant than contemporaneous farming outcomes among peers.

An obvious next step would be to consider the costs and benefits of taking such a program to the field. As noted, our results suggest an approximately 8 percent increase in continuous take-up; the private benefits accruing to the farmers plus the public benefits of any additional carbon sequestration would have to be weighed against the costs of incentives, delivery of incentives, and monitoring. Assuming the net benefits are positive, important decisions would need to be made about the type of incentives and how to allocate treatments. Monitoring costs for conditioning incentives should be low (as they would only need to take place once per annum) and could be organized to coincide with other GASIP programming activities.

Within a 6 to 8-year IFAD programme, one could envision using a randomized roll-out to test the provision of incentives to catalyse CA adoption. The idea would be to use lessons from initial communities in the second entry group, which would serve as the control group during the first phase of the programme. This type of study would allow research to either confirm or refute the results of the FFE described here, while providing some risk reduction for the implementing government; after all, one would know the average treatment effect before implementing the second phase, so if it were unsuccessful in catalysing adoption it could be dropped.

Within the GASIP programme, a small RCT concerning incentives for adoption was conducted in late 2019 and early 2020; however, these results will concern only short term adoption, and the qualitative evidence on the way that FBOs form and dissipate in northern Ghana might suggest that the benefits of that approach might be fleeting.

8.2 Implications for further research

Agronomists have long argued that CA makes for more efficient use of natural resources than traditional farming methods in developing countries (e.g. Hobbs, 2007). As climate change is expected to stress farming systems (e.g. Lipper and Thornton, 2014), the resilience enhancing aspects of CA, are quite attractive from several perspectives. From a economic perspective, it is plausible that the long run private gain to adopting CA is higher than continuing traditional practices for many smallholders, but farmers may not perceive all of the benefits, as research demonstrates in the short-term yields may not increase (e.g. Michler et al., 2019).

A further challenge is that adoption rates for many CA techniques, either promoted as part of a CA package (e.g. Arslan et al., 2017; Michler et al., 2019) or individually (Kondylis, Mueller, and Zhu, 2017; Beaman et al., 2018) are low. Our findings fit within a small literature studying framed field experiments revolving around CA. In an FFE, Alpizar, Carlsson, and Naranjo (2011) study decisions made by coffee farmers to reduce risk in the context of climate changes, and find that when the distribution risks are ambiguous they behave more cautiously. Related to this study, Marenya, Smith, and Nkonya (2014) conducted a choice experiment in Malawi, asking farmers to intercrop legumes with maize while also implementing MSD. Farmers were asked to choose, in pairs, between conventional practices, cash, a fertilizer subsidy, and two types of index insurance contracts. Farmers in that experiment preferred the cash incentive to insurance or the fertilizer subsidy, in part because it gave a constant return. However, experiment parameters were fixed to favour MSD and intercropping, rather than studying whether any of the incentives can catalyse CA adoption.

In this report, we find that the incentives appear to positively influence CA adoption in a repeated framework. Following this proof of concept, the next step would be to test the conclusions from this field experiment in a multi-year randomised control trial (RCT). Several design details are important to consider. The form of incentives would be important; farmers suggested that fertilizers or herbicides would be preferred to cash, which could not be reflected in an experiment such as this. It is also important to consider the way that farmers conceive of CA; Ward et al. (2018) find that in Malawi farmers think of choices about adopting CA techniques as distinct decisions rather than holistically, while the agronomic evidence that exists on yields exists about adoption of the entire package, rather than just pieces of it.

Still, the results are encouraging for the development of a carefully designed RCT following the contours of the FFE described here. As farmers in these areas are already rotating crops and some are practicing permanent soil cover, MSD could be promoted through incentives and doing so would add to the small literature that links results from field experiments to participant behaviour in RCTs (Aflagah, Bernard, and Viceisza, 2019). If such an approach were to replicate the finding of the experiment, it would also provide the opportunity to rigorously analyse the effect of CA adoption on other outcomes, such as input use, household resource allocation and agricultural productivity in an applied setting. Since the assignment of incentives would be exogenous by design, the effects of the intervention on such outcomes would be identified cleanly, absent confounding factors.

9. Major challenges and lessons learnt

Other than timing the research activity to fit in with the GASIP intervention timing, the key challenges which the research team faced can be grouped into two principle categories: challenges relating to the FFE design; and challenges related to the fieldwork implementation.

In terms of the experiment design, a key challenge was establishing exactly how to frame the experiment. At one extreme, one could consider designing an experiment in a very abstract manner, comparing two choices (“A” vs. “B” for example) with minimal framing. Doing so would reduce any potential framing effect. A more abstract design would more clearly identify economic parameters of interest and, assuming appropriate design, would have high internal validity.

However, such an approach presents two related problems. First, it is simply more challenging to explain and ensure participants have a good understanding of an experimental set-up for which they likely have a limited frame of reference. Given the diversity of ethnic groups and languages in our sample (discussed further below) this was a significant consideration. Second, while an abstract experiment may ensure its internal validity, there may be a trade-off in its external validity: the extent to which behaviour in the experiment is predictive of actual decision-making.

As a result, the FFE was framed to be as appropriate as possible for the context. Thus, the experiment was described to participants in terms of agricultural seasons, with explicit reference to specific land preparation approaches, in order to both facilitate understanding, and to attempt to make choices within the experiment represent the actual decision-making process of participants as closely as possible. The script was repeatedly reviewed, tested and updated internally, before live piloting was undertaken by IFPRI and IPA in communities close to the study area. After the live piloting, the script was again discussed and further revised.

An important concern relating to the framing is the fact that we see very high levels of MSD adoption in the control group for the experiment. The same level of adoption was observed during piloting. The script was then modified to both reduce any framing within the script that might lead participants to be more inclined to choose MSD, and the piloted parameters were revised to increase the payoffs associated with CP. The high adoption rates in the control group may represent an experimenter demand effect (where MSD was perceived as the “right” answer- despite explanations that either choice was equally valid). However, if such an effect exists, in expectation it is present for individuals assigned to the treatment and control groups, so it should not affect the internal validity of the estimated treatment effects.

In terms of implementation, since the GASIP programme operates through a lead farmer model, its staff are only directly present in communities at specific points of the year to coordinate activities. As a result, IPA had to undertake significant mobilization activities to meet with community leaders and lead farmers to validate the programme member lists and ensure that the sample was a valid and up to date representation of FBO members. Since the administrative data provided by GASIP did not indicate when individuals were from the same household (for example, a husband and wife), the field team also had to work to track and document these cases, to ensure that households were not interviewed twice, and that experiment participants were linked to the appropriate household data (and the correct individual within that data).

The diversity of languages among the study population also proved a significant challenge for the research. Participants came from six distinct language groups, requiring IPA to target hiring for both baseline and FFE teams to ensure that there were a sufficient number of speakers of each group. While initial translations were done prior to FFE training, a great deal of training time was devoted to ensuring that the translations were correct, and as uniform across languages as possible. As correct translation is particularly important for an experiment script, and a key focus of both training and monitoring was ensuring that enumerators read the FFE script as written and did not skip portions or add their own interpretation.

Online appendixes

Online appendix A: Additional tables

https://www.3ieimpact.org/sites/default/files/2020-07/TW4.1008-GASIP-Ghana-Online-appendix-A-Additional-tables.pdf

Online appendix B: Experiment script

https://www.3ieimpact.org/sites/default/files/2020-07/TW4.1008-GASIP-Ghana-Online-appendix-B-Experiment-script.pdf

Online appendix C: Visual aids

https://www.3ieimpact.org/sites/default/files/2020-07/TW4.1008-GASIP-Ghana-Online-appendix-C-Visual-aids.pdf

References

Aflagah, F. K. D., Bernard, T. & Viceisza, A., 2019. Cheap Talk and Coordination in the Lab and in the Field: Collective Commercialization in Senegal. *NBER Working Paper No. 26045.*

Alpizar, F., Carlsson, F. & Naranjo, M., 2011. The effect of ambiguous risk, and coordination on farmers' adaptation to climate change — A framed field experiment. *Ecological Economics,* Volume 70, pp. 2317-2326.

Andersen, T. B., Jensen, P. S. & Skovsgaard, C. V., 2016. The heavy plow and the agricultural revolution in Medieval Europe. *Journal of Development Economics,* Volume 118, pp. 113-149.

Andersson, J. & D'Souza, S., 2014. From adoption claims to understanding farmers and contexts: A literature review of Conservation Agriculture (CA) adoption among smallholder farmers in southern Africa. *Agriculture, Ecosystems and Environment,* Volume 187, pp. 116-132.

Arslan, A. et al., 2017. Diversification Under Climate Variability as Part of a CSA Strategy in Rural Zambia. *Journal of Develoment Studies,* 54(3), pp. 457-480.

Beaman, L., Ariel, B., Magruder, J. & Mobarak, A. M., 2018. Can Network Theory-based Targeting Increase Technology Adoption?. *Working Paper.*

Bell, A. R. et al., 2018. Transformative change through Payments for Ecosystem Services (PES): a conceptual framework and application to conservation agriculture in Malawi. *Global Sustainability,* 1(e4), pp. 1-8.

Boahen, P. et al., 2007. *Conservation Agriculture as practiced in Ghana,* s.l.: FAO.

Brouder, S. M. & Gomez-Macpherson, H., 2014. The impact of conservation agriculture on smallholder agricultural yields: A scoping review of the evidence. *Agriculture, Ecosystems and Environment,* Volume 187, pp. 11-32.

Carril, A., 2017. Dealing with misfits in random treatment assignment. *Stata Journal,* 17(3), pp. 652-667.

Davies, B. et al., 2014. Acro-Ecosystem, Tillage, and Cropping Effects on Extractable Soil Nitrogen and Organic Carbon in Ghana. *Journal of Arid Land Studies,* 24(1), pp. 203-207.

Dzinyefa Aflagah, F. K., Bernard, T. & Viceisza, A., 2019. Cheap Talk and Coordination in the Lab and in the Field: Collective Commercialization in Senegal. *NBER Working Paper No. 26045.*

Ekboir, J., Boa, K. & Dankyi, A., 2002. *Impact of No-Till Technologies in Ghana,* Mexico City: CIMMYT.

Falk, a. et al., 2016. The Preference Survey Module: A Validated Instrument for Measuring Risk, Time, and Social Preferences. *IZA Discussion Paper No. 9674.*

FAO, 2007. *The State of Food and Agriculture,* Rome: United Nations Food & Agriculture Organization.

Feder, G., Just, R. E. & Zilberman, D., 1985. Adoption of Agricultural Innovations in Developing Countries: A Survey. *Economic Development and Cultural Change,* 33(2), pp. 255-298.

Foster, A. D. & Rosenzweig, M. R., 2010. Microeconomics of Technology Adoption. *Annual Review of Economics,* Volume 2, pp. 395-424.

FSIN, 2019. *Global Report on Food Crises,* Rome: FAO.

GASIP, 2018. *Ghana Agricultural Sector Programme (GASIP), Climate Change Resilience Component: Guiding Notes for CA Farmer Selection,* Accra: Ghanaian Ministry of Food & Agriculture.

Giller, K. E., Witter, E., Corbeels, M. & Tittonell, P., 2009. Conservation agriculture and smallholder farming in Africa: The heretics' view. *Field Crops Research,* 114(1), pp. 23-34.

Harrison, G. & List, J. A., 2004. Field Experiments. *Journal of Economic Literature,* 42(4), pp. 1009-1055.

Herbst, D. & Mas, A., 2015. Peer effects on worker output in the laboratory generalize to the field. *Science,* 350(6260), pp. 545-549.

Hobbs, P., 2007. Conservation Agriculture: What Is It and Why Is It Important for Future Sustainable Food Production?. *The Journal of Agricultural Science,* pp. 127-137.

IFAD, 2014. *Ghana Agricultural Sector Investment Programme.* [Online]
Available at:
https://www.ifad.org/en/web/operations/project/id/1100001678/country/ghana
[Accessed 6 1 2020].

IFAD, 2019. *Ghana Agricultural Sector Investment Programme: Supervision Report,* Rome: IFAD.

IFDC, 2016. *USAID Cooperative Agreement No. AID-641-A-13-0001,* Washington DC: USAID.

Ito, M., Matsumoto, T. & Quinones, M. A., 2007. Conservation tillage practice in sub-Saharan Africa: The experience of Sasakawa Global 2000. *Crop Protection,* 26(3), pp. 417-426.

Jat, R. A., Sahrawat, K. L., Kassam, A. H. & Friedrich, T., 2013. Conservation Agriculture for Sustainable and Resilient Agriclutlre: Global Status, Prospects and Challenges. In: *Conservation Agriculture: Global Prospects and Challenges.* Boston: CABI.

Knowler, D. & Bradshaw, B., 2007. Farmers' adoption of conservation agriculture: A review and synthesis of recent research. *Food Policy,* Volume 32, pp. 25-48.

Kondylis, F., Mueller, V. & Zhu, S., 2017. Seeing is Believing? Evidence from an Extension Network Experiment. *Journal of Development Economics,* Volume 125, pp. 1-20.

Lal, R., Reicosky, D. & Hanson, J., 2007. Evolution of the plow over 10,000 years and the rationale for no-till farming. *Soil and Tillage Research,* 93(1), pp. 1-12.

Levitt, S. D. & List, J. A., 2007. What Do Laboratory Experiments Measuring Social Prefernces Reveal About the Real World. *Journal of Economic Perspectives,* 21(2), pp. 153-174.

Marenya, P., Smith, V. & Nkonya, E., 2014. Relative Preferences for Soil Conservation Incentives among Smallholder Farmers: Evidence from Malawi. *American Journal of Agricultural Economics,* 96(3), pp. 690-710.

Margruder, J. R., 2018. An Assessment of Experimental Evidence on Agricultural Technology Adoption in Developing Countries. *Annual Review of Resource Economics,* Volume 10, pp. 19.1-19.18.

Michler, J. D., Baylis, K., Arends-Kuenning, M. & Mazvimani, K., 2019. Conservation Agriculture and Climate Resilience. *Journal of Environmental Economics and Management,* Volume 93, pp. 148-169.

Nyamangara, J. et al., 2014. Weed growth and labor demand under hand-hoe based reduced tillage in smallholder farmers' fields in Zimbabwe. *Agriculture, Ecosystems and Environment,* Volume 187, pp. 146-154.

Palm, C. et al., 2014. Conservation agriculture and ecosystem services: An overview. *Agriculture, Ecosystems and Environment,* Volume 187, pp. 87-105.

Pannell, D. J., Llewellyn, R. S. & Corbeels, M., 2013. The farm-level economics of conservation agriculture for resource poor farmers. *Agriculture, Ecosystems and Environment,* Volume 187, pp. 52-64.

Pittelkow, C. M. et al., 2015. Productivity limits and potentials of the principles of converation agriculture. *Nature,* Volume 517, pp. 365-370.

Rusinamhodzi, L. et al., 2011. A meta-analysis of long-term effects of conservation agriculture on maize grain yield under rain-fed conditions. *Agronomy for Sustainable Development,* Volume 31, pp. 657-673.

Schaafasma, M., Ferrini, S. & Turner, R. K., 2019. Assessing smallholder preferences for incentivised climate-smart agriculture using a discrete choice experiment. *Land Use Policy,* Volume 88.

UNCCD, 2017. *Global Land Outlook,* Bonn: United Nations.

UNDP, 2017. *Annual Report,* New York: United Nations Development Programme.

Waddington, H., White, H. & Anderson, J., 2014. *Farmer field schoools: From agricultural extension to adult education,* Washington DC: 3ie.

Ward, P., Bell, A., Droppelmann, K. & Benton, T. G., 2018. Early adoption of conservation agriculture practices: Understanding partial compliance in programs with multiple adoption decisions. *Land Use Policy,* Volume 70, pp. 27-37.

Ward, P. S. et al., 2016. Heterogenous preferences and the effects of incentives in promoting conservation agriculture in Malawi. *Agriculture, Ecosystems and Environment,* Volume 222, pp. 67-79.