

Increasing the adoption of conservation agriculture: A framed field experiment in Northern Ghana

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Abstract

Conservation agriculture techniques have the potential to increase agricultural production while decreasing CO₂ emissions, yet adoption in the developing world remains low—in part because many years of continuous adoption may be required to realize gains in production. We conduct a framed field experiment in northern Ghana to study how incentives and peer information may affect adoption. Incentives increase adoption, both while they are available and after withdrawal. There is no overall effect of peer information, but we do find evidence that information about long-term adoption increased adoption, particularly when that information shows that yield gains have been achieved.

KEYWORDS

conservation agriculture, experiment, Ghana, lab-in-the-field

JEL CLASSIFICATION

C93, O12, Q12

1 | INTRODUCTION

Climate change is a serious threat to the livelihoods of millions of smallholder farmers in developing countries, particularly in Africa, where farmers are largely dependent on rainfed agriculture and vulnerable to droughts, flooding, and seasonal rainfall pattern disruptions (UNDP, 2017). Smallholder productivity is further threatened by increasing soil degradation, which reduces land productivity over time (UNCCD, 2017). A package of practices called conservation agriculture (CA) has been proposed as one solution to the consequences of climate change and soil degradation. Proponents argue that CA combines private benefits to adopters—by increasing yields and reducing vulnerability to rainfall shocks—and public good characteristics, via carbon sequestration in soil and reduced soil

runoff into water catchment systems (Bell et al., 2018a; Hobbs, 2007).

Despite these claims about CA, adoption of its practices in developing countries remains low (Giller et al., 2009; Michler et al., 2019). One explanation is that while benefits take time to be realized, adoption costs are borne up front: yield gains from improved soil health can take up to 10 years to be realized, but adopting CA requires immediate additional investments in the form of labor and/or herbicide application for weeding (Giller et al., 2009). Poor farmers may be unwilling to take on these up-front costs for uncertain future gains. In this article, we conduct a framed field experiment with farmers in northern Ghana to test two strategies to encourage adoption by reducing this uncertainty: offering time-limited subsidies and providing information on others' adoption decisions.

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A key challenge to understanding CA adoption is the long time-horizon required for private benefits to be realized, which makes it difficult to conduct research leading to causal inference. A direct consequence is that there have been few rigorous evaluations of CA adoption. The framed field experiment we implement simulates the decision-making process to understand how participants respond under uncertainty in benefits. While the experiment does not capture all the real-world aspects of an adoption decision, framed field experiments have been used in a variety of related contexts to test how farmers respond in conditions of risk or uncertainty and to predict actual decision-making (Alpizar et al., 2011; Tjernström et al. 2021).

The experiment is designed to answer two primary research questions. First, we examine whether providing incentives to implement CA practices increases adoption both in the “short-run” (while conditional incentives are available) and the “long-run” (after incentives have been withdrawn). Second, we study whether farmers who receive information on the returns achieved by others in their community who have (or have not) adopted CA practices are as likely to adopt as those who do not receive information. We randomize farmers into an incentive treatment, a cross-randomized peer information treatment, and a control group, and study how the proportion adopting CA practices varies across groups.

In the incentive treatment, we provide a temporary subsidy conditioned on adoption of CA practices. Since private benefits (via increased yields) should persist over time, in theory farmers should continue to use CA practices even after the subsidy is withdrawn. The positive environmental externalities can justify such incentives from a policy perspective, since the incentives can be designed to be welfare-enhancing overall. A related concept has been tested in different settings, by paying people to preserve land endowments that provide ecological benefits (e.g., Alix-Garcia et al., 2018; Jayachandaran et al., 2017).¹ Therefore, the primary contribution of this article is to provide causal evidence on the potential for incentives to increase CA adoption, using the framed field experiment to study the dynamics of incentivized adoption over many “seasons.”

Our second treatment explores an alternative strategy to address uncertainty, by providing information about the experience of peers to farmers. By randomizing information in the context of our framed field experiment we can study how learning about different types of peer behavior can affect farmer choices, and how social learning might affect adoption in the context of a technology requiring a long time-horizon to be profitable. The role of individual learning is particularly important in the adoption of agricultural technologies, as observing peers can reveal information both about the profitability of a technology and information on management practices (Foster & Rosenzweig, 2010). Conley and Udry (2010) find evidence of both processes in studying pineapple adoption among farmers in Ghana: farmers adjust their own input use after observing unexpected profits (or losses) from a neighbor’s previous input allocation. Evidence suggests that peers can be just as, if not more, influential than community leaders and extension workers (Ambler et al., 2021; BenYishay & Mobarak, 2019). Of particular relevance to the technology we consider, Crane-Droesch (2018) conducts an experiment on the diffusion of information on a soil amendment technology in Kenya and finds observed variability in peer outcomes has a strong negative effect on adoption. Also related, Bell et al. (2018a) find an association between peer effects and adoption in Malawi, though they do not disentangle the way different types of information are related to adoption.²

Our results provide support for the potential of time-limited incentive payments to cause farmers to adopt CA practices in the long run. Across specifications we find a positive and statistically significant effect of assignment to the incentive treatment on the extent of CA adoption across a range of specifications. Treated participants are more likely to adopt the CA practice, maintain adoption until they achieve the private returns to choosing CA, and are less likely to return to conventional practices (CP) after choosing CA. For the information treatment, we find being told that a peer has successfully adopted CA over the long-term increases adoption, but do not find effects for other types of information, or for receiving information in general.

¹ Jayachandran et al. (2017) implement a randomized evaluation of a program in Uganda that gave households payments contingent on maintaining tree cover on their land. They found that payments were successful in reducing deforestation and that the environmental benefits compensated for the program cost. Alix-Garcia et al. (2018) use a regression discontinuity design to study the impacts of payments for participation in a land management program in Mexico. They find that payments improve land management activities and community social capital.

² Other relevant studies that examine information dissemination through peer networks and agricultural technology adoption include Beaman et al. (2021) who find that targeting “central” farmers increases adoption of pit planting in Malawi, Kondylis et al. (2017) who find minimal impacts of peer farmers on adoption of pit planting in Mozambique, Bandiera and Rasul (2006) who find an increase in adoption of a new crop as more individuals in the person’s network adopt, and Carter et al. (2021) who find large impacts on adoption of improved seeds and fertilizer with the addition of contacts who have received past subsidies for these items.

2 | BACKGROUND

Before describing the experiment design, we provide a brief description of CA and its current status in northern Ghana. CA is defined by three principles: minimal soil disturbance (MSD), permanent soil cover, and crop rotation (FAO, 2007). MSD is the replacement of traditional ploughing with direct seeding to reduce the effects of planting on the soil structure. Permanent soil cover involves leaving residues from the previous crop on plots, combined with the planting of cover crops during fallow periods. Crop rotation is the practice of planting different crops in sequential seasons to diversify nutrients available to microorganisms and create variation in the soil depth in which roots are established.

The purpose of CA practices is to increase soil organic matter to improve water and nutrient retention, which in turn allows farmers to sustainably intensify production while mitigating negative environmental impacts. While CA practices have been widely adopted in some developed economies (e.g., USDA, 2019), adoption is relatively low in low- and middle-income countries. The CA adoption literature does not yield much information about why farmers do not adopt; literature reviews find it to be highly context specific (Knowler & Bradshaw, 2007) and methodologically weak, often relying on observational data from projects aimed at promoting CA (Andersson & D'Souza, 2014). While several studies have used hypothetical choice experiments (in which decisions are not incentivized) to explore farmers' stated preferences for both financial and non-financial incentives to adopt (e.g., Marenya et al., 2014; Schaafasma et al., 2019; Ward et al., 2016), there is limited evidence which exploits exogenous variation to test how farmers respond to actual incentives. One exception is work by Bell et al. (2018b, 2018c) which uses randomized assignment to test the effects of incentives on CA adoption in Malawi, and finds an initial increase in adoption after the first season of the project. This study shows that incentives can induce on-farm adoption of CA techniques.

In northern Ghana, about 80% of land is under customary tenure (Bugri & Yeboah, 2017). In practice, households have long-standing land use rights, which are patrilineally inherited. In the sample of farmers surveyed for the framed field experiment, households both had firm use rights and cultivated 81.2% of plots enumerated, with the second most common type of plot being communally owned. Other plots were either rented-in or sharecropped. Therefore, for most plots, households should reap longer term benefits from any investments they might make in soil fertility.³

³ However, farmers generally lack credit access; according to the Ghana Statistical Service (2019), at the regional level only 5.8% to 11.6% of respondents within the four regions in the study had applied for credit.

Information on respondent and household characteristics, and comparisons by treatment status, can be found in Appendix Table S1.

3 | EXPERIMENTAL DESIGN

3.1 | Conceptual framework

In this section we describe a conceptual framework for the adoption of CA techniques that allows for interpretation and understanding of our experimental design.⁴ We begin by assuming, consistent with the agronomic literature, that if a farmer consistently applies CA techniques their land will eventually become more productive; for example the expected yield will increase for the primary crop grown on that land. We further assume that input costs are higher for implementing CA, because required labor inputs increase (Giller et al., 2009). Additionally, it is not clear how long it will take for the yield increase to occur. From a conceptual perspective, the uncertainty about the timing of the yield increase implies a risk averse farmer or one with a higher discount rate would be less likely to adopt. We further assume that using CA techniques leads to higher land productivity when weather is poor than traditional techniques, as soil begins to have better water absorption and retention properties. Finally, since farmers also have not necessarily used CA techniques (though they may have learned about them through extension workers), we assume there is some additional risk to adopting them.⁵ As a result, a risk averse farmer would require a premium over simple expectations over profits to adopt CA.

We propose that there are two ways to overcome an individual farmer's aversion to adopting CA techniques. First, an external entity could provide incentive payments to farmers for a fixed period of time (e.g., number of years) after adoption to partially compensate the farmer for extra labor inputs and the risk premium required for the farmer to adopt.⁶ This compensation need not last until the yield

⁴ While the experimental design focuses on minimum soil disturbance for simplicity, this framework speaks to CA techniques more broadly.

⁵ In northern Ghana, there have been past several CA projects, increasing the likelihood they knew of or had tried specific CA techniques. These projects included Sasakawa Global 2000 (Ito et al., 2007) which was active from 1986 to 2003 and promoted no-till farming and not burning crop residues. Other notable activities include the Savannah Resources Management Project implemented by the Ministry of Lands and Forestry (Boahen et al., 2007); work by the Center for No-Till Agriculture which is sponsored by the Howard Buffett Foundation and provides training to farmers on CA techniques; and the World Bank's Sustainable Land and Water Management Practice Project (SLWMP) which was active around the Kulpawn-Sissili and Red Volta watersheds.

⁶ Implicit in this argument is the idea that CA adoption creates a public good. There are at least two arguments that it could do so. First, CA adop-

benefit occurred, but it would need to last long enough for the net present value of continued adoption to exceed that of abandoning CA. Second, farmers could learn more about CA techniques by observing other local farmers. If, for example, one observed a neighboring farmer using CA techniques with much higher yields in a bad year, non-adopters could be induced to adopt or try those techniques. These observations would reduce the risk of adopting CA practices by providing additional, credible information.

3.2 | Experimental procedure

The framed field experiment was designed to represent these key features of CA practices over a medium to long time horizon.⁷ Participants are asked to decide whether to adopt a single CA practice: MSD. While CA practices should be adopted jointly to realize maximum benefits, we focus on a single practice for experimental simplicity. MSD was selected because focus groups in the area suggested it was the CA practice with which participants had the least experience. Participants are asked to make an adoption decision in each of ten rounds, with each round modeled as an agricultural season.⁸ Prior to the first round, the participant receives a monetary endowment for use in the activity. Each round then proceeds as follows:

1. The participant chooses one of two technologies to adopt for that round, either MSD or CP.
2. They pay a fixed price associated with that choice from their current endowment.⁹ In the experiment, the price represents the cost of weeding associated with the chosen technology, and these costs are higher with MSD than with CP.
3. The enumerator reveals the rainfall for that season. Rainfall is determined randomly and is poor with 1/3 probability or normal with 2/3 probability.¹⁰

tion should reduce greenhouse gas emissions by the agricultural sector, both through less burning crop residue and by lowering the need for fertilizer use. Second, conservation agriculture leads to improved soil water retention, which could reduce runoff in at least some neighbors' fields, particularly those that are downhill.

⁷ The scripts used in the experiment are included in Online Appendix A.

⁸ The specific choice of ten seasons is used to present a reasonable representation of the timeframe required. Giller et al. (2009) suggest 10 years as an upper bound for the realization of yield gains. We elected to simulate ten seasons with gains occurring within 5–7 seasons to observe behaviour after their (potential) realization, while limiting the time commitment required of participants. Participants knew in advance of any decisions that the total length of the experiment would be ten rounds.

⁹ The experiment was structured such that the participant always had sufficient funds to choose either practice, independent of the outcome of prior rounds.

¹⁰ To ensure consistency, all randomization was conducted in Stata prior to fieldwork and loaded into the software used for implementation. Enumerators could not change any randomized parameters as they were associated with a unique subject identifier.

4. The participant receives a payment based on their choice of technology and the rainfall realization. This payment represents the value of their harvest for that season. Payments are always higher with normal rainfall than with poor rainfall.

Before the beginning of each round, the participant was shown a choice sheet, which showed the two available choices, the price associated with each choice, and the two potential payoffs associated with each choice (four total). The choice options and associated prices were fixed throughout. The probability of each rainfall outcome was fixed and independent across rounds. The payments associated with each outcome vary by round based on if participants were assigned to the incentive treatment (described below) and on their adoption choices in the current and preceding rounds.¹¹ The choice sheets are shown in Online Appendix B.

For participants who chose MSD, if the choice was made continuously over multiple rounds, the available payments associated with that technology would increase once and remain at that higher level so long as they continued to adopt. This feature was intended to reflect the property that private benefits from CA adoption are realized over a medium to long time horizon, the length of which is not known by farmers *ex ante*. To model the uncertainty farmers face over when gains from CA adoption might occur, participants were randomly assigned with equal probability to receive the production increase with 5, 6, or 7 rounds of continuous adoption.¹² Participants were told in the script the value of the yield gain, and that abandoning CA would 'reset' the number of rounds of MSD adoption required. Hence the only two aspects of the experiment which were unknown to the farmer (and the enumerator) prior to realization were the weather outcome for each round, and the exact round in which the yield gain would occur.

Both prices and costs 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 with the same appearance as local currency was used and exchanged for real money following the conclusion of the final round.

¹¹ Oliva et al. (2020) also study incentives for adoption of a technology with delayed payout under uncertainty, finding that offering incentives leads to increased adoption by people who are less likely to follow through.

¹² Note that adoption only needed to be continuous— a farmer could for example choose CP in the first round and still achieve the yield gain if they chose MSD for 5–7 consecutive rounds thereafter.

As described in the script (Online Appendix A) enumerators provided a full explanation of the procedures for the experiment and conducted a practice round with the participant. During this explanation, participants were asked a series of questions to check that they understood the explanations being provided. Enumerators recorded their first response to each question and provided additional explanations if the participant misunderstood something. Overall, participants had a good understanding of the features of the experiment (Appendix Table S2).¹³

3.3 | Incentive treatment

The incentives treatment was designed to represent a subsidy payment to farmers adopting MSD. Participants were randomized into a group receiving incentives and a control group. Randomization was done at the individual level, stratified by farmer-based organization (FBO) and information treatment status.¹⁴ The probability of being assigned to the incentives treatment was 2/3, with 1/3 assigned to the control. The reason for treating a larger portion was to ensure sufficient variation among treated individuals in the number of consecutive rounds required to achieve increased production.

If assigned to receive incentives, the participant was eligible to receive an additional payment conditional on choosing MSD in any of the first four rounds of the experiment, which they received immediately after making their choice in each round. The amount of the incentive was fixed, and no incentives were available after the fourth round. The incentive was not conditioned on decisions in any previous round, so a treated individual choosing CP in rounds 1–3 would still be able to receive a payment if they chose MSD in round 4.

3.4 | Peer information treatment

Participants were also cross-randomized with equal probability into either a group assigned to receive information

¹³ Limiting our sample to participants who answered all the comprehension questions correctly first time does not affect our main results (Appendix Table S3).

¹⁴ Our pre-analysis plan (<https://www.socialscisearch.org/trials/3973>) anticipated stratifying by gender. Unfortunately, accurate administrative data including participant gender was not available prior to the start of fieldwork, hence the stratification was not implemented. We test for heterogeneity by gender in Appendix Table S4. Since groups were not of uniform size, individuals did not always evenly divide into treatment groups within a stratum (i.e., a group of twenty people cannot be divided into thirds). For the additional ‘misfit’ observations we randomly allocate individuals independently across strata, using the procedure and associated *randtreat* command described in Carril (2017).

about a generic peer farmer or a control group.¹⁵ Participants assigned to the information treatment were read a short vignette about an unnamed, hypothetical peer farmer before making their decision during the first four rounds of the experiment. Vignettes were used to exogenously vary information received by the participant. The four texts used were as follows:

- Last year they used CP on their plots, they have always used CP.
- Last year they used MSD on their plots. They had not used this technique before.
- Last year they used MSD on their plots. They have been using MSD for the last 10 years.
- Last year they used CP on their plots. They had used MSD before but decided to go back to CP.

Along with each 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.¹⁶ As a result there were eight possible variations of the information provided. The vignettes are representative of all possible payoffs and adoption histories (i.e., never adopted, early adoption, achieved yield gain, dis-adoption). For a given prior rainfall outcome, each variation was chosen via an independent random draw. 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.

3.5 | Payoffs

The payoffs were calibrated to model the features of CA technologies, scaled to a reasonable budget for the project. Participants were paid a fixed fee of 5 cedis (.93 USD) which was approximately the local wage for a day of agricultural labor at the time of the experiment, and could earn an additional 3–10 cedis over the course of the experiment.¹⁷ The total payout therefore ranged from 8 to 15 cedis (1.49–2.80 USD). The mean payout for the experiment was 12.6 cedis (2.36 USD). Table 1 presents the available payouts in the experiment for CP and MSD.

Since the values for CP are fixed, there are three possible comparisons: CP versus MSD without incentives; CP

¹⁵ Assignment followed the same procedure as for the incentives treatment, but the treatment and control groups were of equal size.

¹⁶ For round 1, participants in the information treatment were randomly assigned a rainfall outcome for the (hypothetical) preceding season.

¹⁷ Participants started with an initial endowment of 1 cedi, and could earn .2–.9 cedis per round.

TABLE 1 Payoff amounts per round, by practice choice & scenario.

Technology	CP	MSD	MSD	MSD	
Incentive treatment?	–	No	Yes	No	
Production increase	–	No	No	Yes	
Scenario (choice sheet)	A/B/C	A	B	C	
Possible rounds	1–10	1–10	1–4	5–10	
Normal rainfall	A. Price of choice	10	30	30	30
	B. Incentive payment	0	0	20	0
	C. Production payment	100	100	100	120
	Net payoff (C+B-A)	90	70	90	90
Poor rainfall	A. Price of choice	10	30	30	30
	B. Incentive payment	0	0	20	0
	C. Production payment	30	50	50	60
	Net payoff (C+B-A)	20	20	40	30
	Expected value	66.7	53.3	70	73.3

Notes: 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).

versus MSD with incentives; and CP versus MSD without incentives but with a yield gain realized.¹⁸ Weeding costs are held constant throughout the experiment, and cost 10 pesewas for CP and 30 for MSD.¹⁹ The incentive payment is 20 pesewas in the rounds in which it is offered, covering the difference between the cost of implementing CP and MSD. Production payments vary by rainfall and whether the yield gain has been achieved. Initially, in normal years, CP and MSD both pay 100 pesewa (not considering the weeding costs or incentives). After the yield gain has been achieved, the payment for MSD increases to 120 pesewas in normal years. In poor years, MSD always pays more, reflecting CA's resilience properties. Prior to the yield gain, in poor rainfall years CP pays 30 pesewas and MSD pays 50. After the gain, MSD pays 60 pesewas in poor years.

Comparing these three scenarios we can observe some straightforward features of the experiment: for a given round without incentives a risk-neutral participant will strictly prefer CP, since the payouts are higher than MSD under a normal rainfall outcome and equal to MSD under poor rainfall. With incentives, the reverse is true: MSD has equal returns under normal rainfall and better returns under poor rainfall.

Combining payouts across rounds, the expected value of choosing CP across all rounds is $66.7 \times 10 = 667$ pesewas.

¹⁸ Note that since the incentives were only available in rounds 1–4, and the yield gain took at least 5 rounds to be realized, there is no scenario in which the participant could receive the incentives and the gain in the same round.

¹⁹ While CA adoption could require a range of additional costs in terms of farm labor (including changes in land preparation and time required to do mulching) it was clear from discussions with participants in piloting that the largest and most salient cost for farmers was weeding. We therefore chose weeding to represent the increase in input costs associated with CA which in actual implementation would occur across a range of activities.

Without incentives, the earliest stage at which the yield gain could be achieved is round 5. For this case, the highest possible expected value of always choosing MSD is therefore the expected value of MSD from scenario (A) for four rounds, plus the expected value of MSD from scenario (B) for six rounds. Hence: $53.3 \times 4 + 70 \times 6 = 633.2$ pesewas. As a result, a risk neutral participant always chooses CP over MSD without incentives.

With incentives, the situation is reversed. For a participant always choosing MSD, the latest round in which the yield gain can be realized is round 7. Therefore, the lowest expected payoff from choosing MSD with incentives will be the total of the expected value from scenario (B) (rounds 1–4), plus the expected value from scenario (A) (rounds 5–6), plus the expected value from scenario (C) (rounds 7–10): $70 \times 4 + 53.3 \times 2 + 73.3 \times 4 = 679.8$. Hence the lowest possible expected payoff for continuous MSD adoption in the incentive scenario exceeds the expected payoff for continuously choosing CP.

This parameterization implies that for risk neutral individuals, it is preferable for individuals to choose CP when in the control group. However, if individuals are risk averse, preferences depend upon their degree of risk aversion. In other words, for some individuals it becomes preferable to select MSD over CP during all ten rounds. If we consider the constant relative risk aversion utility function, a risk averse individual who expected the MSD bonus to occur in round 6 would be neutral between choosing CP and MSD for a risk aversion coefficient of approximately .687.²⁰

²⁰ Assuming a constant relative risk aversion function of the form $U(C) = C^{1-\theta} / (1-\theta)$ when $\theta \neq 1$ and $U(C) = \ln(C)$ if $\theta = 1$. MSD is preferable under this utility function for individuals with values of q between .687 and 1.

3.6 | Limitations

The goal of the framed field experiment is to model real-life adoption decisions over the long time-horizon needed for the benefits of CA to be fully realized. There are four principal ways in which our experiment must necessarily deviate from the parameters of real-life CA adoption decisions. The first is the role of time discounting. When considering payoffs from land preparation decisions that may materialize over 10 years, farmers will discount that income differently than payouts to be made over the course of a 90-min experiment. Specifically, we may expect farmers to be more present-biased in their actual decisions, and this will be a key additional element to explore in future research. Second, although not trivial for participants, the stakes in the experiment are much lower than those around actual planting decisions for a primary crop. This may affect their decisions, in particular their willingness to take risks.²¹

Third, we assume farmers do not face credit constraints in the experiment: they always have sufficient capital to choose either practice, and neither choice affects their ability to make other investments. Fourth, the income earned in the experiment is a windfall, whereas real-life planting decisions are made with regular income, and evidence has shown that windfall and regular income are often spent in different ways (Arkes et al., 1994; Milkman & Beshears, 2009).

Fourth, participants' choices may have been influenced by the experimenter demand effects, particularly if they believed that providing the "right" answer might influence the likelihood that they would receive additional services in future. These effects may account for the relatively high level of CA adoption in the control, though the level effect should not bias our estimation of treatment effects. We are further able to test for heterogeneity in results among those who had or had not recently received training which may have encompassed CA principles and do not find differences in the effect of incentives between farmers who had or had not been sensitized (Appendix Table S7).

4 | DATA AND ESTIMATION

4.1 | Sample

This project was conducted in partnership with the Ghana Agricultural Sector Investment Programme (GASIP), a national initiative which aims to support the develop-

ment of agricultural value chains within Ghana. As part of its activities, GASIP promotes CA principles as well as increased access to improved inputs such as certified seed and machinery. We obtained a list of 66 FBOs created by GASIP for their activities in four northern regions of Ghana.²² Field staff visited each group in the second quarter of 2019 and obtained a listing of all current members. The FBOs joined GASIP in waves, with some groups joining in 2018 and others in 2019. The 2018 FBOs had exposure to 1 year of GASIP extension information (including CA and other techniques) at the time of the experiment, while implementation had not yet begun for the 2019 FBOs. The 66 FBOs enrolled were the universe of all FBOs exposed to GASIP CA activities in the north in 2018 or 2019.

The sample was composed of current FBO members: 1328 individuals across 66 FBOs.²³ Each member was visited to confirm their sample status and conduct a household survey, with a separate team of enumerators returning a few days later to conduct the experiment. If the listed individual was not available within 1 week of the scheduled household interview a replacement was used. Replacements were required to be adults within the same household who were also involved in farming. Overall, 1324 individuals were interviewed, of whom 38 were replacements.²⁴ Field work was conducted from April to June 2019.

Table 2 presents the share of respondents reporting knowledge and use of CA practices (MSD, cover-cropping, applying crop residues, not burning, and crop rotation). In general, most farmers are familiar with CA techniques, with the share somewhat higher in farmer groups which were targeted by GASIP in 2018, compared to 2019 FBOs.²⁵ For most practices, fewer than half of participants report applying them in the most recent agricultural season. The exceptions to this were the related practices of using residues for soil cover, and not burning residues.²⁶ Overall, individuals in the sample can be said to be somewhat

²² These are Northern, Upper East, Upper West, and Brong Ahafo. Farmer groups are located in twelve districts within these regions. Note that the groups comprise speakers of six languages, spread over a large geographical area, suggesting that the potential for spillovers was limited.

²³ There are 30 2018 FBOs and 36 2019 FBOs. The average group size was 20 members. One FBO was substantially larger than the others, with 37 members. For this group we randomly sampled 20 members.

²⁴ There were cases where participants were members of the same household, so the total household survey sample is 1117. For some cases, the field team was unable to match household data to individuals, as a result there are 25 experiment participants for whom we do not have a full set of controls for regression specifications. We retain these individuals and include indicator variables for the relevant missing data. Excluding these individuals does not substantively affect results.

²⁵ The main results do not vary by 2018 and 2019 FBOs (Appendix Table S7).

²⁶ The use of fire to remove residues is actively discouraged by the government of Ghana. As a result, this measure may be over-reported.

²¹ We test for heterogeneity based on self-reported measures of risk and time preferences (Appendix Tables S5 and S6) and do not find that our effects are driven by more risk-averse or impatient participants.

TABLE 2 Adoption and knowledge of CA techniques, by timing of FBO entry to GASIP.

	Old FBOs	New FBOs
Heard of...		
Conservation agriculture	.95	.79
Minimal soil disturbance	.78	.55
Cover cropping	.79	.54
Using residues	.93	.81
No burning	.97	.84
Crop rotation	.84	.73
Adopted last season...		
Conservation agriculture	.89	.83
Minimal soil disturbance	.33	.20
Cover cropping	.36	.27
Using residues	.70	.61
No burning	.81	.70
Crop rotation	.48	.41

Notes: 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 “Adopted” the overall CA proportion is an indicator for responding “Yes” to one or more sub-categories.

sensitized to CA techniques, though very few people have adopted all of them.

4.2 | Empirical strategy

To evaluate the impacts of the respective treatments on adoption of MSD in the experiment, we estimate three primary specifications using ordinary least squares at the participant level, following our pre-analysis plan. To address multiple hypothesis testing, we control for the false discovery rate (FDR) by calculating sharpened q-values (Anderson, 2008; Benjamini et al., 2006).²⁷ Our first specification is as follows:

$$Y_{ij} = \alpha + \beta_1 Incentive_{ij} + \beta_2 Information_{ij} + \beta_3 R6_{ij} + \beta_4 R7_{ij} + \gamma X_{ij} + \delta_j + \epsilon_{ij} \quad (1)$$

where Y is one of three outcome variables (the number of rounds in which MSD was adopted; a binary indicator for whether the yield gain was realized; and a binary indicator for whether the respondent ever stopped choos-

ing MSD after adopting). *Incentive* and *Information* are indicator variables for the respective treatments, and $R6$ and $R7$ are indicators for being in the groups that could realize the yield gain after choosing MSD for 6 and 7 consecutive seasons respectively (with 5 seasons as the omitted category).²⁸ X is a vector of control variables, δ represents stratification cell fixed effects (farmer group dummies, with j denoting group membership), and ϵ is an error term robust to heteroskedasticity, computed using the HC3 method (Davidson & MacKinnon, 1993).²⁹ Since the experiment is individually randomized and we cover the universe of FBOs formed before the end of 2019 for the CA component of GASIP, there is no need to cluster standard errors (Abadie et al., 2023). 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. Because the treatments are orthogonal, including them in the same regression does not change the results. We therefore present results in a combined regression for simplicity of presentation.

We then estimate the same specification adding an interaction term:

$$Y_{ij} = \alpha + \beta_1 Incentive_{ij} + \beta_2 Information_{ij} + \beta_3 IncentiveXInformation_{ij} + \beta_4 R6s_{ij} + \beta_5 R7s_{ij} + \gamma X_i + \delta_j + \epsilon_{ij} \quad (2)$$

To analyze the impact of the type of information received in the information treatment, among those who received the information treatment we estimate an alternative regression. This regression is estimated at the participant-round level for the first four rounds, and includes interactions with the rainfall realization in the previous round as it is referenced in the information:

$$Y_{ijr} = \alpha + \beta_1 InfoB_{ijr} + \beta_2 InfoC_{ijr} + \beta_3 InfoD_{ijr} + \beta_4 PoorRainfall_{ijr} + \beta_5 InfoB_{ijr}XPoorRainfall_{ijr}$$

²⁸ Due to some enumerator errors (as a result of conducting an experiment using an incorrect ID on the tablet computer) 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 assignment for the sample. We use the assigned status throughout, but the results of the analysis are not meaningfully altered by using actual assignment.

²⁹ 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.

²⁷ The FDR accounts for the percentage of false positives among rejected null hypotheses. The sharpened q-value is the expected proportion of false positive within a family of outcomes if the coefficient in question is assumed to be significant. All main results are robust to using sharpened q-values.

TABLE 3 Average outcomes in the experiment, by treatment and gain round.

	Incentive treatment		Information treatment		Gain round		
	No incentives	Incentives	No	Yes	Round 5	Round 6	Round 7
No. rounds MSD chosen	7.83	8.41	8.25	8.18	8.42	8.09	8.14
Achieved gain	.68	.75	.73	.73	.78	.72	.69
Abandoned MSD	.34	.27	.29	.29	.26	.30	.31

Note: Columns represent the mean for each group.

$$\begin{aligned}
 &+ \beta_5 \text{InfoC}_{ijr} X \text{PoorRainfall}_{ijr} \\
 &+ \beta_5 \text{InfoD}_{ijr} X \text{PoorRainfall}_{ijr} \\
 &+ \gamma X_{ijr} + \delta_j + \rho_r + \mu_{ijr}
 \end{aligned} \quad (3)$$

Here the outcome variable Y_{ijr} is an indicator variable which takes the value 1 if participant i chose MSD in round r , and 0 if they chose CP. We include a round fixed effect ρ , and indicator variables representing the information received for a given round:

InfoA: Neighbor used CP (which is the omitted category);
InfoB: Neighbor used MSD for the first time;
InfoC: Neighbor used MSD for the last 10 years; and
InfoD: 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.

This specification did not include the interaction terms in the pre-analysis plan, and we include both in the results table, but they are included here to help explain whether certain types of information are effective only when observing a positive or negative peer outcome.

5 | RESULTS

Table 3 shows the mean of each of the three main participant-level outcomes for each of the two randomized treatments as well as the randomized yield gain round. The share choosing MSD is high overall, with individuals in the no incentive group choosing it 7.83 rounds out of ten on average, but higher in the incentive group, chosen 8.4 times out of ten on average. 68% of non-incentivized individuals realized the yield gain, compared to 78% of incentivized individuals. Those in the incentives group were also less likely to abandon MSD after they had chosen it: 27% of them did so, relative to 34%

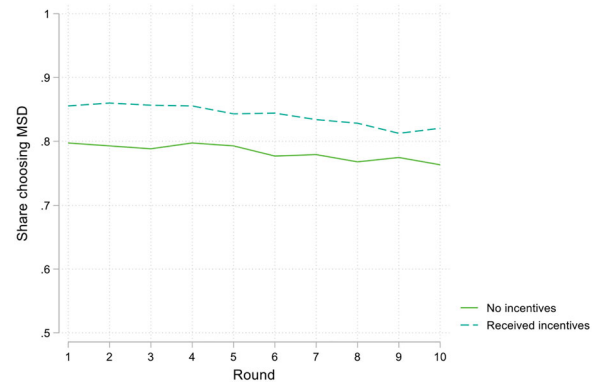
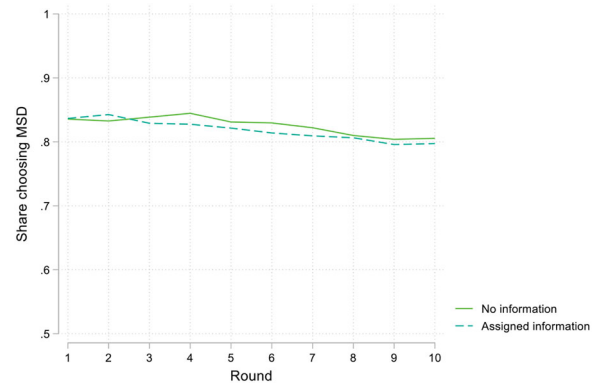
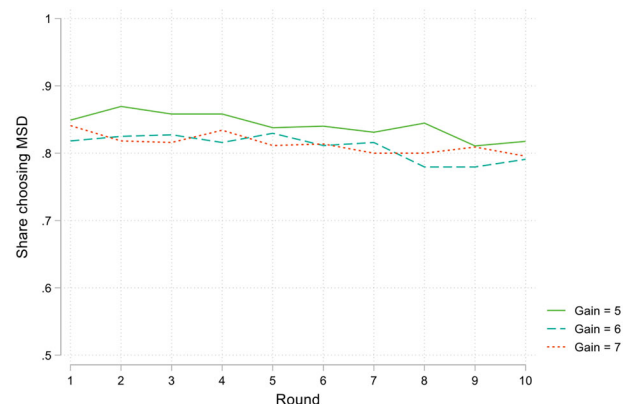

FIGURE 1 Share choosing MSD, by incentive treatment status.

FIGURE 2 Share choosing MSD, by information treatment status.

FIGURE 3 Share of Participants choosing MSD, by gain round.

TABLE 4 Impact of incentive and information treatments on MSD adoption.

	(1) Number rounds MSD	(2) Achieved gain	(3) Abandoned MSD
Incentive treatment	.598***	.083***	-.074***
Standard error	(.182)	(.026)	(.027)
P-value	.001	.002	.007
Sharpened q-value	.003	.003	.003
Information treatment	-.049	.004	-.009
Standard error	(.162)	(.024)	(.025)
P-value	.763	.856	.713
Sharpened q-value	1.000	1.000	1.000
Gain round: 6	-.222	-.038	.022
Standard error	(.199)	(.029)	(.030)
P-value	.265	.191	.476
Sharpened q-value	.661	.661	.661
Gain round: 7	-.275	-.090***	.044
Standard error	(.192)	(.029)	(.030)
P-value	.153	.002	.147
Sharpened q-value	.114	.006	.114
Mean: No incentives	7.829	.677	.337
Mean: No information	8.253	.727	.293
Mean: Gain Round = 5	8.417	.777	.261
Adjusted R-squared	.119	.114	.083
Observations	1324	1324	1324

Notes: Ordinary least squares regression, with stratification-cell (FBO) fixed effects. Heteroskedasticity robust (HC3) standard errors are reported in parentheses. The following control variables are included, but not reported: household size; respondent is female; self-reported risk preference; self-reported time preference; value of all household assets; number of CA practices reported last season; estimated value of all crops last season; number of GASIP-promoted crops grown; has electric light; has toilet; has cement walls; has cement floors; has metal roof; grew tubers; has any missing crop value data; has any missing individual data; has any missing risk or time preference data; received poor rainfall in practice round.

*, **, and *** indicate significance at the 10%, 5%, and 1% level respectively.

of non-incentivized individuals. Conversely, there is no evidence of a difference in behavior by information treatment. The means by gain round group do suggest that the earlier the gain is achieved, the more likely it is the participant reaches that point. For example, 7% percent of those who received the gain after the fourth round achieved it, compared to 69% among those receiving it after round 6.³⁰

We also examine how behavior may have changed over the course of the experiment. Figure 1 shows adoption by round and incentive treatment status. Across rounds,

³⁰ We can also describe behaviour in the experiment in other ways. For example, 66.8 (57.4) percent of incentivized (non-incentivized) participants choose MSD in every round. 5.4 (6.2) percent choose CP in every round. It is uncommon for participants to switch once between technologies: 1.2 (2.7) percent of participants switch once from CP to MSD and 3.2 (1.6) percent of participants switch once from MSD to CP. Multiple switches are more common, 23.4 (32.1) percent of participants make multiple switches over the course of the experiment.

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 declines slightly. Note that the level of adoption in the control group is higher than the actual level reported by farmers in FBOs which had been previously sensitized to MSD (33%). As described in Section 3.5, this finding may reflect an experimenter demand effect or local constraints (lack of availability of seed drills or potential gaps in implementation knowledge) which are not accounted for in our experiment. In either case, these should all be equal across treatment groups and thus do not threaten the internal validity of the experiment.

Figure 2 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 shows adoption by round separately by gain round group assignment. We do not observe

TABLE 5 Impact of treatments on MSD adoption, interacted treatments.

	(1) Number rounds MSD	(2) Achieved gain	(3) Abandoned MSD
Assigned incentives	.871***	.123***	-.084**
Standard error	(.260)	(.038)	(.040)
P-value	.001	.001	.034
Sharpened q-value	.003	.003	.012
Assigned information	.310	.057	-.022
Standard error	(.306)	(.045)	(.047)
P-value	.310	.205	.636
Sharpened q-value	.871	.871	.871
Assigned incentives X Assigned information	-.536	-.079	.019
Standard error	(.361)	(.054)	(.056)
P-value	.138	.146	.730
Sharpened q-value	.281	.281	.322
Gain round = 6	-.209	-.036	.021
Standard error	(.198)	(.029)	(.031)
P-value	.291	.213	.487
Sharpened q-value	.775	.775	.775
Gain round = 7	-.276	-.090***	.044
Standard error	(.193)	(.029)	(.030)
P-value	.152	.002	.147
Sharpened q-value	.113	.006	.113
Mean: No treatments	7.685	.653	.338
Mean: Gain Round = 5	8.417	.777	.261
Adjusted R-squared	.120	.115	.082
Observations	1324	1324	1324

Notes: Ordinary least squares regression, with stratification-cell (FBO) fixed effects. Heteroskedasticity robust (HC3) standard errors are reported in parentheses. Control variables are included in the specification, but not reported (see Table 4 note).

*, **, and *** indicate significance at the 10%, 5%, and 1% level respectively.

divergence among adoption rates by gain round assignment, though there is some variation in the initial share of participants choosing MSD.

We next turn to the main regression analysis, beginning with the participant-level analysis. Table 4 shows the results of estimating Equation (1), including all three treatment randomizations. The incentive treatment results in an economically and statistically significant impact on adoption. Participants in the incentive group choose MSD on average in .6 more rounds, an increase of 7.6% relative to the control group. They were 8.3 percentage points more likely to achieve the yield gain (12% increase) and were 7.4 percentage points less likely to abandon MSD once they had chosen it (22% decrease).³¹

³¹ Following our pre-analysis plan, Appendix Tables S4–S6 test for heterogeneity by respondent gender, self-reported risk aversion, and self-reported time preferences. Appendix Table S7 tests for heterogeneity by

There is some evidence that those who received the gain after rounds 6 or 7 adopt MSD less overall, but the coefficient estimates are not typically statistically significant. 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 5.³²

wave in which the community was enrolled in the GASIP program. We do not find that the effects of treatments varied significantly for any of these groupings.

³² In Appendix Table S8 we find a statistically significant negative differential effect for gain round 6, which is sufficiently large to cancel the incentive effect for this group of farmers. To further examine this result, we plot the impact of the incentive treatment for each incentive-gain round combination, separately by round (Appendix Figure S1). Across rounds, this effect is similar for gain rounds 5 and 7, and lower for gain round 6. Note the effect for gain round 6 is stable across rounds, including rounds 1–4 at which point none of the participants had discovered

The estimates of the impact of the information treatment are close to zero in this specification.³³

Table 5 reports the estimation of regression specification (2) and examines the impact of interacting the incentives and information treatments. The results remain suggestive that the information treatment did not have an impact and are not indicative of any complementary effects between the two. The coefficient estimates for the information treatment alone have the expected signs for each dependent variable, but are not statistically significantly different from zero. Similarly, estimated coefficients on the interaction term are not statistically across the three dependent variables.

Finally, we estimate Equation (3) to study the different types of information that were offered, using the subset of individuals who received an information treatment (Table 6). Without interactions between rainfall and information (column 1), we find evidence of an effect of being told the neighbor had used MSD for at least 10 seasons. The coefficient is 3.7 percentage points, corresponding to a 4.6% increase relative to being told your neighbor had used CP. The coefficient estimates on the other forms of information are not statistically significant, and we can reject that the effect of being told a neighbor used MSD for the last 10 years is equal to being told that the neighbor abandoned MSD. We cannot however reject that this effect is equal to being told the neighbor used MSD for the first time.

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 neighbor earned. Recall that a pay-off differential under poor rainfall is evident for all those choosing MSD (information groups B and C), but the pay-off differential for choosing MSD in normal years is only evident for information group C (neighbor used MSD for 10 years). Because the information treatment occurred in the first four rounds of the experiment, this outcome is the only one participants could not have experienced for them-

which gain round value they had been assigned. These results suggest the group of individuals assigned to gain round 6 within the incentives treatment were somewhat less likely to pick MSD *ex ante* than other individuals in the sample. These individuals are similar in terms of observable characteristics (Appendix Table S9), so this finding appears likely to be a statistical artefact.

³³ In Appendix Table S10 we present our main specifications in long form (with one observation per participant-round and round-level fixed effects), as indicated in the pre-analysis plan with the addition of a control for rainfall in the preceding round. We report our results separately by rounds 1–4 and rounds 5–10 in columns (3)–(6) to examine whether treatments impact the choice of MSD in each round. The results are similar, and there is no evidence that the impact of the incentives falls off after removing the incentive.

TABLE 6 Impact of different information types on MSD adoption.

	(1)	(2)
	Dependent variable: Chose MSD	
Info B: Used MSD (first time)	.030	.024
Standard error	(.020)	(.024)
P-value	.142	.334
Sharpened q-value	.397	.397
Info C: Used MSD (last 10 years)	.037*	.051**
Standard error	(.020)	(.024)
P-value	.068	.035
Sharpened q-value	.074	.074
Info D: Abandoned MSD	−.001	.004
Standard error	(.021)	(.025)
P-value	.952	.886
Sharpened q-value	1.000	1.000
Poor rainfall last round	−.012	−.002
Standard error	(.015)	(.031)
P-value	.424	.943
Sharpened q-value	1.000	1.000
Info B x Poor rainfall		.018
Standard error		(.041)
P-value		.657
Sharpened q-value		.49
Info C x Poor rainfall		−.042
Standard error		(.044)
P-value		.336
Sharpened q-value		.202
Info D x Poor rainfall		−.015
Standard error		(.045)
P-value		.743
Sharpened q-value		.591
P-value: Info B = Info C	.688	.233
P-value: Info B = Info D	.119	.401
P-value: Info C = Info D	.054	.043
P-value: Info B + Info B x Prev Rainfall		.218
P-value: Info C + Info C x Prev Rainfall		.087
P-value: Info D + Info D x Prev Rainfall		.594
Mean: No information, previous normal	.837	.837
Adjusted R-squared	.106	.105
Observations	2644	2644

Notes: Sample restricted to information treatment group. Heteroskedasticity robust (HC3) standard errors are reported in parentheses. Observations are at the participant-round level, rounds 1–4. Ordinary least squares regression, with stratification-cell (FBO) and round fixed effects. Control variables are included in the specification, but not reported (see Table 4 note).

*, **, and *** indicate significance at the 10%, 5%, and 1% level respectively.

selves, and as such, information group C in normal years may be the mostly likely to affect behavior.

When the rainfall is normal, the effect of being told your neighbor had used MSD for 10 seasons (and therefore received the production bonus) is now 5 percentage points, and statistically significant at the 5% level. The corresponding interaction term for poor rainfall is -4.2 percentage points, though not statistically different from zero. Regardless, it implies the total effect of group C information is near zero when rainfall is poor. This pattern is not repeated for those receiving the information that the neighbor used MSD for the first time (group B). These results are in line with the discussion above, that those who received information about the MSD yield gain in normal years were receiving new information and updating their behavior accordingly. Overall, the evidence suggests that when promoting a technology like CA where there are deferred benefits, observing peers who have experienced those benefits can be useful for promoting adoption. It should be noted that the peer information provided was limited, and did not help to reduce farmer uncertainty about when the yield gain would occur. As such, a peer information intervention in the field could have larger impacts.

6 | CONCLUSION

Agronomists have argued that CA makes for more efficient use of natural resources than traditional farming methods in developing countries (e.g., Hobbs, 2007). However, the long-time frame associated with private gains to adoption, and uncertainty regarding these gains, contribute to adoption rates well below what would be socially optimal. Using a framed field experiment, this study finds that incentives for adoption might be an effective tool for increasing adoption of CA techniques prior to the point when they become privately profitable. Though there is no overall effect of information, we do find some evidence that being given positive information about neighbors experiencing the deferred benefits of CA increases adoption. While our results are limited to the experimental environment, they suggest that investing in pilot tests of these policy solutions would be worthwhile.

These findings point to both incentives and information campaigns that emphasize outcomes from early adopters as policy options for governments and other actors that want to increase the adoption of CA techniques, and also speak more generally to the promotion of technologies with deferred benefits. In considering how to design incentive and information programs, there are several points to consider. The form of incentives is important. In focus groups conducted in formative research farmers suggested that fertilizers or herbicides would be preferred to cash;

such in-kind incentives could not be reflected in an experiment such as this one. It is also important to consider the way that farmers conceive of CA. Bell et al. (2018b) find that in Malawi farmers think of choices about adopting CA as distinct decisions for each technique. However, the agronomic evidence that exists on yields concerns adoption of the entire package, rather than just pieces of it. Therefore, effective policy would either need to consider ways to ensure that farmers were using the entire suite of CA techniques, or would need to also build evidence on the impacts of partial adoption of CA techniques.

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SUPPORTING INFORMATION

Additional supporting information can be found online in the Supporting Information section at the end of this article.

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