Pre-Analysis Plan:

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

1 Research Questions

This study seeks to provide insight into the best ways to increase adoption of conversation agriculture(CA) techniques. (CA) provides benefits to farmers by (1) immediately mitigating the negative yield shocks caused by bad weather shocks, and (2) increasing average yields in average and good years after a number of seasons of continued adoption. The challenge with adoption of CA techniques is that farmers must commit to continued adoption while possibly seeing no benefits or even slightly reduced expected yields over several years, complicating the incentives for adoption. Moreover, CA practices may induce additional costs, in the form of additional requirements for weeding, herbicides, or pest control.

A potential solution for overcoming these challenges is to provide incentives to farmers for adoption, reducing or eliminating the negative consequences for household well-being that adoption can cause in the short and even medium term. However, a study would need to follow and incentivize farmers for a number of years, since the benefits from adoption of conservation agriculture take time to materialize as soil fertility is restored. As such, setting up a randomized or non-randomized evaluation of the effects of such incentives on adoption of such techniques is challenging, and if too short it may lead to incorrect conclusions about adoption rates. Further, it may be difficult to ensure that the control group is not contaminated at some point in the research process.

A framed field experiment can allow for researchers to simulate the required time horizon, and therefore inform the best design of such incentives, meaning that a more cost-effective design can be found before attempting to implement it in the field (if one exists). In this study, we plan to analyze data from a baseline survey, in combination with data from a framed field experiment. This will allow us to understand the answers to research questions below, and can contribute to the design of a randomized control trial to provide a full impact evaluation of these techniques, and study both adoption and impact. The research questions to be addressed during this pilot work are as follows:

- Does offering monetary incentives improve the adoption of a CA technology that may not pay off for several seasons?
- How does the uncertainty over when the technology pays off affect adoption? If the incentive is discontinued before benefits appear, does that affect adoption?
- Is learning from peers an important component of the adoption decision? In other words, does knowledge of peer adoption decisions and production outcomes impact adoption decisions?
- Does the impact of incentives and peer learning vary by risk and time preferences?

2 Research Strategy

This pilot study will employ a framed field experiment to answer the above research questions.

2.1 Sampling

2.1.1 Sampling Frame

Because this formative work is intended to inform a full evaluation of adoption incentives for conservation agriculture in the context of the GASIP program implemented by the government of Ghana, the study sample will be drawn from farmers who are or who are targeted to become GASIP beneficiaries in 2019. The conservation agriculture component of the GASIP program began initial work in 30 villages in 2018, among a group of approximately 20 beneficiary farmers in each village. This study will work with these farmers and add additional villages and farmers that are targeted for inclusion in the GASIP program in 2019. As part of the recruitment for this study, the research team will work with GASIP to define the targeted farmers for 2019. GASIP plans to add an additional 720 farmers in 2019, for a total of 1,320 farmers.

The defining characteristic of this population is then that they are farmers who have chosen to participate in these initial GASIP activities, which include attending trainings on CA and carrying out land preparation on a plot on which they will implement CA practices. As a result, they differ from the rest of the population in that they were chosen to participate and agreed to do so.

The expected sample size is approximately 1,320 farmers for the baseline survey.

2.1.2 Assignment to Treatment

This study will encompass three cross-randomized treatments. The first level of randomization will consist of two arms: a control group of no incentives (1/3) and a group that receives the same incentive for four seasons within the experiment (2/3). The second randomization will determine the number of seasons after which the technology adoption will provide a positive production boost. Incentives will be given for four seasons, and benefits will appear after 4, 5, or 6 seasons. This variation will allow us to study whether adoption will continue after the incentives end and before the benefits kick in, to simulate an environment in which the time frame for payoff is uncertain and/or government or NGO budgets may cause incentives to end prematurely. The final treatment will consist of a control group and a group that receives information, following each “season,” about peer choices and outcomes in that season.

- Randomization for each cross-randomized treatment will be done at the individual level. Randomization will be stratified on village, assignment to other treatment, and farmer gender.
  - We additionally intend may stratify treatment on risk and time preferences but doing so will depend on whether field logistics allow for the baseline measures to be conducted and quickly analyzed for stratification prior to the framed field experiment.

- Stratification for risk and time preferences will be conducted through the use of the risk and time preference survey questions (see Appendix). Piloting will determine which version of the questions (a simple survey measure with scales or a hypothetical lab-in-the-field measure) perform better. Based on the distribution of the data, respondents will be allocated to two roughly equal sized groups (risk averse/not risk averse and patient/impatient). These groups will be used for stratification.

- Within the peer treatment, we will randomly assign the information that farmers receive about their peers. That is the adoption decision and adoption history for the peer/neighbor farmer. That randomization will be conducted on farmer-season level.

2.1.3 Attrition from the Sample

Because this is a framed field experiment that is conducted in one interaction, attrition is not expected to be a major concern. However, there may be attrition from the sampling frame. Our sampling frame is determined by the farmer lists provided by GASIP. We will randomize over this list of farmers. It is expected that some farmers may not be available or refuse to participate. Given the connection the farmers have with the GASIP project, and because the farmers will earn money for participating, we expect, based on previous experience, that attrition will be quite low at no more than 5 to 10 (at the absolute high end) percent of the targeted farmers. Farmers will not know which group they are assigned to prior agreeing to participate, so concerns about differential attrition are unwarranted, beyond any that may occur by chance. 5 percent attrition from the targeted sample would be 1,254 farmers. 10 percent attrition would be 1,188 farmers. Power calculations account for the expected attrition which is not expected to be a major concern.

The framed field experiment and the baseline will take place in two separate interactions, so there may be some farmers who participate in the baseline, and not in the experiment and vice versa.

2.1.4 Statistical Power

As noted below, all principal outcome variables are indicator variables (0/1). The power calculations make standard assumptions about alpha (0.05) and power (0.8). Randomization will be conducted at the individual level, simplifying the discussion of statistical power. The below tables show the minimum detectable effect size, expressed as the percentage point difference between the control group and a treatment group (or groups) for a range of different rates of control group adoption, over three different sample sizes: the full sample of approximately 1,320 farmers, sample including approximately 5 percent attrition from the baseline (1,254), and a conservative sample that includes roughly 10 percent attrition (1,188). The below assumes a control group of 440 farmers (one third) and a treatment group of 880 farmers (two thirds) for the incentive treatment. Additionally, the full sample will be cross randomized into three groups: those who receive benefits from CA after 4 seasons, those after 5 seasons, and those after 6. In addition to the overall effect of the incentive treatment, we would like to examine the impact of the

treatment, interacted with the year in which benefits accrue.

This chart clearly shows that in this range of number of observations, we are well powered to detect impacts between 4 and 7 percentage points. These are reasonable effect sizes, because given the very low adoption of conservation agriculture, the goal of this project would be to increase adoption substantially.

Detectable percentage point difference between treatment and control: Incentive treatment

		Total sample size		
		1188	1254	1320
Reference group mean	0.1	4.62	4.49	4.37
	0.2	5.92	5.75	5.6
	0.3	6.63	6.45	6.28
	0.4	6.97	6.79	6.61
	0.5	7.02	6.83	6.66
	0.6	6.78	6.6	6.44
	0.7	6.24	6.08	5.93
	0.8	5.33	5.19	5.07
	0.9	3.83	3.74	3.66

We also show the minimal detectable effect for distinguishing between the year in which benefits accrue, within the incentive group. In this analysis we are powered to detect differences between 7 and 11 percentage points. These are reasonable effect sizes, as we are most interested in larger impacts on adoption if there are indeed differences when benefits do not accrue until after the incentives expire.

Detectable percentage point difference between years in which benefits accrue among those receiving incentives

		Total sample size		
		1188	1254	1320
Reference group mean	0.1	7.34	7.13	6.92
	0.2	9.15	8.89	8.65
	0.3	10.1	9.83	9.57
	0.4	10.5	10.23	9.97
	0.5	10.46	10.2	9.94
	0.6	10.01	9.76	9.52
	0.7	9.11	8.89	8.67
	0.8	7.66	7.48	7.31
	0.9	5.36	5.24	5.13

When considering the detectable impact for the peer information treatment, with only two treatment groups, we are able to 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.

Detectable percentage point difference between treatment and control: Peer information treatment

		Total sample size		
		1188	1254	1320
Reference group mean	0.1	5.41	5.25	5.11
	0.2	6.88	6.69	6.51
	0.3	7.68	7.47	7.28
	0.4	8.06	7.85	7.65
	0.5	8.09	7.88	7.68
	0.6	7.8	7.6	7.41
	0.7	7.16	6.98	6.81
	0.8	6.09	5.94	5.8
	0.9	4.36	4.26	4.16

Finally, we present the detectable effect sizes for the interaction of the incentive and peer information treatments. We do not expect to be able to detect differences when interacting all three treatments. We show tables for two comparisons: Those who receive information without incentives compared to those who receive information and incentives, and those who receive incentives without information compared to those who receive incentives and information. In the former comparison we can detect differences between 8 and 12 percentage points and in the latter between 6 and 10 percentage points.

Detectable percentage point difference between those who receive Information and No Incentives and those who receive Information and Incentives

		Total sample size		
		1188	1254	1320
Reference group mean	0.1	8.65	8.39	8.15
	0.2	10.67	10.36	10.08
	0.3	11.72	11.39	11.1
	0.4	12.14	11.81	11.51
	0.5	12.05	11.73	11.44
	0.6	11.48	11.19	10.92

0.7	10.4	10.15	9.91
0.8	8.7	8.49	8.3
0.9	6.02	5.89	5.77

Detectable percentage point difference between those who receive Incentives and No Information and those who receive Incentives and Information

		Total sample size		
		1188	1254	1320
Reference group mean	0.1	6.77	6.57	6.38
	0.2	8.52	8.28	8.06
	0.3	9.47	9.21	8.97
	0.4	9.89	9.62	9.38
	0.5	9.89	9.63	9.39
	0.6	9.5	9.25	9.03
	0.7	8.68	8.46	8.26
	0.8	7.35	7.17	7
	0.9	5.2	5.08	4.97

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.

2.2 Fieldwork

2.2.1 Instruments

- The qualitative component of this study has already been completed, and substantially affected the design of the baseline and the framed field experiment.
- This study will include a baseline survey and the implementation of the framed field experiment, conducted in two separate visits. The framed field experiment will be conducted with the use of a detailed script developed by the research team.
- The baseline survey will cover basic household demographic indicators, household labor supply, detailed information on agricultural production, household assets, land holdings, and housing. We additionally will collect information regarding knowledge and use of conservation agriculture techniques, perceptions of use of conservation agriculture by neighbors and other peers, risk and time preferences, and experiences with other government agricultural programs.
  - This instrument has been developed by the researchers with the aim of providing a baseline for the framed field experiment, as well as a subsequent evaluation of

GASIP activities.

  - While the instrument has not been used before, most questions have been adapted from other surveys implemented either at a large scale in northern Ghana or by the research team. The survey instrument will be piloted several times prior to implementation.

- The script to run the framed field experiment will be a detailed step by step guide that the field staff will read to respondents word for word when conducting the experiment. It will be programmed into a tablet to ensure that randomization is automatically implemented and respondent choices recorded, and subsequent payouts correctly calculated.
  - This script has not been used before because it is developed specifically for the research questions to be addressed here, but the research team has experience implementing similar experiments in other settings and is drawing upon that experience in developing this script. Further, the team has looked at scripts that have been used in previous framed field experiments examining conservation agriculture, though from different angles than in this project.
  - The script has been carefully piloted in similar communities in the study area in order to ensure that the language used and descriptions of the experiment are understandable to participants. Piloting was also used to establish that the values of parameters used were consistent with the local context and using local currency units.

3 Empirical Analysis

3.1 Variables

The goal of this framed field experiment is to study how two different interventions, incentives and information about peer adoption, combined with uncertainty about the timing of the technology's payoffs, affects one's own adoption of CA practices. Therefore, the outcome variables of interest are the choice made by each farmer in each round of whether or not to adopt CA practices. The specific variables we will analyze are as follows:

- Adoption in each season.
- Adoption in consecutive seasons (ie 2 consecutive seasons, 3 consecutive seasons, etc). Special attention will be paid to adoption in 4, 5, or 6 consecutive seasons as those are the seasons in which continual adoption should lead to yield growth (depending on the treatment group). These are indicator variables for each level of consecutive seasons. We will also analyze an indicator for "adoption long enough to see production gains."
- "Disadoption," defined as choosing not to adopt after adopting.

- Farmers will also be classified as risk averse and patient/impatient based on responses to specifically designed questions in the baseline survey. These will be defined as described above for randomization stratification.

3.2 Balancing Checks

- We will check balance between treatment and control groups by selecting a set of baseline variables and testing whether the means of these variables are statistically different in each group, as well as conducting a joint test across all variables
- The specification used will mirror the specifications that we will employ for our main analysis. The outcome variable (left hand side variable) is the baseline variable of interest, with indicator variables for the treatment groups on the right-hand side, as well as stratification cell fixed effects. Because randomization will occur at the individual level, we will use robust, non-clustered standard errors. This specification will allow us to test for pairwise equality between each of the treatment groups, and to also perform a test for joint equality across groups.
  - In testing for joint significance of the baseline indicators, we will include treatment status as the dependent variable and all selected baseline variables on the right-hand side, in addition to stratification fixed effects. We then test for joint significance of the selected baseline variables.
  - The selected baseline variables will include: household size, gender of participant, value of household assets, total area cultivated, implementation of conservation agricultural practices, total value of crop production, growth of crops amenable to conversation agriculture, risk aversion, patience, and housing characteristics.
- As previously described, attrition is not expected to be a major concern. Particularly, because no one will know the treatments prior to participating in the study, we do not expect differential attrition that could affect the internal validity of the study.
- Because we will not have any demographic information on households that are GASIP participants but that do not complete the baseline survey, we cannot check balance between these groups.
- We will check balance between those that complete the baseline survey but do not participate in the framed field experiment and those that do. The analyses will mirror the variable-by-variable and overall tests described above, with the treatment variable replaced with a variable for attrition. We will also check for attrition that is differential by treatment by including treatment as a right-hand side variable.

3.3 Treatment Effects

3.3.1 Intent to Treat

We will estimate the impact of the treatments (offer of either type of incentive, or provision of peer information) on the choice to adopt conservation agriculture. The outcome variables are described in more detail above. The specification for the incentive treatment is as follows:

$$Y_i = \alpha + \beta_1 Incentive + X_i + \delta_i + \varepsilon$$

where $Incentive$ is an indicator for being in the incentive treatment group. X_i is a vector of baseline control variables, and δ_i are stratification cell fixed effects. The OLS regression includes robust standard errors, but with individual level randomization, clustered standard errors are not necessary. β_1 is the mean difference between the outcomes in the incentive treatment and the control group.

- Because of the randomization, control variables are not strictly necessary, but we will run specifications that both include and do not include these variables. The inclusion of these control variables may increase precision of our estimates and will provide some descriptive knowledge about what household characteristics influence adoption.
- These control variables will include: household size, gender of participant, value of household assets, total area cultivated, implementation of conservation agricultural practices, total value of crop production, growth of crops amenable to conversation agriculture, risk aversion, patience, and housing characteristics.

- We will additionally study whether the amount of time it takes for the production benefits to accrue affects adoption behavior, by interacting the randomly determined number of seasons needed for the production increase with the incentive treatment as follows:

$$Y_i = \alpha + \beta_1 Incentive + \beta_2 5seasons + \beta_3 6seasons + \beta_4 IncentiveX5seasons + \beta_5 IncentiveX6seasons + X_i + \delta_i + \varepsilon$$

- The second treatment (provision of peer information) will be analyzed in a similar fashion, but with only one treatment group the specification will look as follows:

$$Y_i = \alpha + \beta_1 PeerInfo_1 + X_i + \delta_i + \varepsilon$$

- Finally, we can analyze the interaction of the peer information treatment and the incentive treatment to understand whether receiving both incentives and peer information increases adoption more than just receiving one or the other. We expect that this regression may lack statistical power at smaller effect sizes, but we will include for completeness.

$$Y_i = \alpha + \beta_1 Incentive_1 + \beta_2 PeerInfo_1 + \beta_3 Incentive_1 X PeerInfo_1 + X_i + \delta_i + \varepsilon$$

- The majority of our analyses will be run on the farmer/household level. However, we will also include one specification in which each observation will be the adoption decision in each round and here we will additionally include round fixed effects.
- To more deeply understand the impact of the peer information, we will also conduct a separate analysis, using only farmers in the peer information group. The outcome variable is the adoption decision in each round, and the treatment variable would be the randomly selected information that the farmer received at the end of the previous round.

3.4 Heterogeneous Effects

- The adoption of conservation agriculture implies risk because it is a costly and unknown new technology, and it also requires patience because it does not pay off immediately following adoption. As such, the primary heterogeneity analysis we plan to run involves studying the different impacts of the treatment by risk and time preferences. Given logistical difficulties, we may not be able to stratify randomization on these characteristics, but will conduct the heterogeneity analysis using the variables as defined in the randomization section.
- We will also conduct heterogeneous treatment analysis based on gender of the farmer, an element we do plan to use to stratify the randomization.

3.4.1 Intent to Treat

- The heterogeneous effects of the treatments by risk and time preferences and gender will be estimated by including the indicators of these preferences in the main regressions and interacting them with the treatment variables. The analysis will be conducted by interacting treatment with the indicators for risk aversion and impatience, defined as described above for randomization stratification.

3.5 Standard Error Adjustments

- Because we will perform an individual level randomization, we do not plan to cluster the standard errors in our analysis.
- To address concerns related to multiple hypothesis testing, we will calculate sharpened q-values for related outcomes.

Appendix: Risk and Time Preference Survey Questions

	I1	On a scale from 1 to 10 where 1 is not at all willing to take risks and 10 is very willing to take risks, how do you see yourself: are you generally a person who is fully prepared to take risks or do you try to avoid taking risks?			
	I2	On a scale from 1 to 10 where 1 is very patient and 10 is very impatient, how do you see yourself: are you generally a person who is patient and willing to wait to do or get something, or are you generally a person who is very impatient and want to do or get things right away?			
	Respondent	I8	I9	I10	Tablet automatically increments by 10 cedi until respondent switches or amount reaches 300 / 0 cedi
		What would you prefer, a 50% chance of winning 300 cedi and a 50% chance of winning nothing, or would you rather have 150 cedi as a sure payment?	What would you prefer, a 50% chance of winning 300 cedi and a 50% chance of winning nothing, or would you rather have 140 cedi as a sure payment?	What would you prefer, a 50% chance of winning 300 cedi and a 50% chance of winning nothing, or would you rather have 160 cedi as a sure payment?	
		I2	I2	I2	
			I18=1	**I18=2**	
1	Household Head				
2	Spouse of head				
	Respondent	I11	I12	I13	Tablet automatically increments by 10 cedi until respondent switches or amount reaches 100 cedi
		Would you rather receive 50 cedi today, or 100 cedi one month from now?	Would you rather receive 100 cedi today, or [AMOUNT]+10 cedi one month from now?	Would you rather receive 100 cedi today, or [AMOUNT]-10 cedi one month from now?	
		I3	I3	I3	
			I11=1	**I11=2**	
1	Household Head				
2	Spouse of head				