Testing innovative approaches to extension in Cambodia: Using technology and incentives to improve the PADEE and ASPIRE projects

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Note to readers

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Summary

Agricultural productivity and profitability are keys to improving the livelihoods of rural households and to poverty reduction. Governments of developing countries have allocated considerable resources to improve agricultural practices, encourage the use of improved intermediate outputs, and induce adoption of new available technologies among farmers. However, farmers do not always adopt new technologies, as they may not know about or how to use them. To overcome this constraint, governments and others have invested considerable resources in agricultural extension programs.

However, there is little convincing evidence that sending extension workers to farmers is a cost-effective way to improve farmer knowledge. Extension is not likely to be effective if extension workers lack adequate levels of education, training, or skills to transfer techniques and technologies. And extension workers visits are costly to monitor, due to the spatial dispersion of agriculture. Governments or others offering extension have found it difficult to design reliable mechanisms assessing the accountability and performance of extension workers.

In this report, we describe two pilot interventions using Information and Communications Technology (ICT) conducted by Cambodia's Ministry of Agriculture, Forestry, and Fisheries (MAFF) and the International Food Policy Research Institute (IFPRI) to try to overcome these constraints. The interventions were both randomized and occurred within projects designed with the International Fund for Agricultural Development (IFAD)—the Project for Agricultural Development and Economic Empowerment (PADEE) and the Agriculture Services Programme for Innovation, Resilience and Extension (ASPIRE).

Within PADEE, we additively randomized two interventions to standard PADEE extension during the 2016 growing season. First, we tested e-PADEE software, developed to provide extension agents with tablets equipped with app-based information about seed recommendations and fertilizer applications based on soil testing results, and identification and treatment of crop diseases. Second, we test whether performance-based incentives increase information dissemination through e-PADEE.

Within ASPIRE, we used a push-message based system for delivering extension messages and randomized the frequency and intensity of message receipt during the 2017 growing season. The messages involved rice, chicken, and vegetable farming, timed to the agricultural calendar. Randomization involved four groups— a "basic message" group, in which we will push basic messages to the group; "enhanced messages", in which messages were more frequent and timed to the agricultural season; "enhanced messages plus", in which we added farmers outside the farmer group to receive messages; and a control group, which received ASPIRE as usual.

We use baseline and endline surveys to understand how the interventions changed treatment groups relative to the control group. We are first interested in whether the two interventions led to increases in knowledge of agricultural practices taught through the messaging systems. Second, we test whether increased knowledge led to the adoption of new agricultural practices or increased expenditures on agricultural inputs. Adoption of new practices or increased expenditures could lead to increases in agricultural yields;

yield improvements could be reflected as increases in agricultural income and value of production per hectare. In both pilots, we look for immediate impacts; however, productivity impacts might take time to build up after repeated extension interactions.

Within the PADEE intervention, we find evidence that software use was effective at teaching farmers additional rice extension messages, some of which farmers implemented. We did not find differences between the two treatment groups. Farmers in the treatment groups were about 50 percent more likely to report receiving agricultural extension than the control group, and the percentage of farmers reporting using seed and fertilizer recommendations increased by about 5 and 12 percentage points, respectively. However, we do not find any impacts on rice production or productivity.

In the ASPIRE pilot, we find the messages were popular among farmers receiving them. Even after 12 weeks of messages, about 60 percent of farmers listened to entire messages. Farmers report finding the messages increased their production, and we find further evidence that messages spread around the village beyond call recipients. All the effects were stronger with the enhanced message groups. Calls on rice and chickens were more likely to be called useful than those on vegetables. Though we find an increase in rice fertilizer use among one of the two enhanced message groups, we do not find production or income impacts caused by the calls.

We estimate the e-PADEE intervention has average and marginal costs of at least $10.48 per farmer. The marginal costs of the ASPIRE intervention are much lower, at a maximum of $2.39 per farmer, and represents actual rather than prospective beneficiaries. As ASPIRE is spending at least $56 per prospective beneficiary, adding direct calls would appear justified on a cost effectiveness basis, even if evidence on increased productivity is only based on farmers' beliefs about their productivity. As a call-in system is now generally available in Cambodia, it would be useful to encouraging farmers to use the system to help increase their knowledge of current recommendations.

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Abbreviations and Acronyms

ANCOVA	Analysis of Covariance
ASPIRE	Agriculture Services Programme for Innovation, Resilience and Extension
BMI	Body Mass Index
CDRI	Cambodian Development Research Institute
CEW	Community Extension Workers
CGIAR	Consultative Group for International Agricultural Research
DHS	Demographic and Health Survey
FFS	Farmer Field Schools
GDA	General Directorate for Agriculture
GDP	Gross Domestic Product
Ha	Hectare
HARVEST	Helping Address Rural Vulnerabilities and Ecosystem Stability
HH	Household
ICT	Information and Communication Technologies
iDE	International Development Enterprises
IFAD	International Fund for Agricultural Development
IFPRI	International Food Policy Research Institute
IRB	Institutional Review Board
MAFF	Ministry of Agriculture, Forestry, and Fisheries
MIS	Monitoring Information System
MST	Mobile Support Team
NGO	Non-Governmental Organization
NPK	Nitrogen, Phosphorous, and Potassium
PADEE	Project for Agricultural Development and Economic Empowerment
PDA	Provincial Departments of Agriculture
RCT	Randomized Controlled Trial
SNV	Stichting Nederlandse Vrijwilligers

1 Introduction

Agricultural productivity and profitability are keys to improving the livelihoods of rural households and to poverty reduction. Governments of developing countries have allocated considerable resources to improve agricultural practices, encourage the use of improved intermediate outputs, and induce the adoption of new available technologies among farmers. However, farmers do not always adopt these technologies. One likely explanation is that farmers – with low average levels of education, living in remote and poorly connected areas – just do not know about improved practices or new production processes; even when access exists, farmers sometimes do not know how to use those technologies optimally. This notion has led governments and other agencies to invest considerable resources in agricultural extension programs.

However, convincing evidence of the causal impact of extension programs on farmers' agricultural productivity has proved to be elusive. One constraint is certainly methodological limitations of previous studies, as there are challenges related to selection, program placement bias, and potentially confounding programs, among others. But sending individuals to advise farmers directly is also expensive; if extension workers lack adequate levels of education, training, or skills to transfer innovative skills and technologies to farmers, extension is not likely to be effective even as it is costly (Feder et al 1999; Swanson and Rajalahti 2010). A further problem with extension is that visits by extension workers are costly to monitor, due to the spatial dispersion of agriculture. Governments or others offering extension have found it difficult to put in place reliable mechanisms to assess the performance and increase the accountability of extension workers.

In this project, the International Food Policy Research Institute (IFPRI) has worked with the Ministry of Agriculture, Forestry, and Fisheries (MAFF) to test methods of overcoming constraints related to information provision, cost effectiveness, and monitoring through the use of Information and Communication Technologies (ICTs) in providing timely advice on agricultural practices. During the period of the grant, MAFF was running two projects funded by the International Fund for Agricultural Development (IFAD). Through discussion with MAFF, it was decided to pilot interventions to innovate on extension in both projects. First, the Project for Agricultural Development and Economic Empowerment (PADEE) worked to improve agricultural productivity and diversification of income sources for rural households in its five target provinces. PADEE took place between 2012 and 2018, and the IFPRI pilot intervention took place in 2016 and 2017. The second project, Agriculture Services Programme for Innovation, Resilience and Extension (ASPIRE), began in 2016 and will run until 2021. ASPIRE was conceived to have two phases; the first phase is working in 5 provinces not included in PADEE (Kampong Chhnang, Pursat, Battambang, Preah Vihear and Kratie); in the second phase, beginning in 2019, the PADEE provinces will be added.

The impact evaluations conducted with PADEE and ASPIRE were timed to provide input into the second phase of ASPIRE when expansion was planned to occur. One of the primary rationales for ASPIRE is that public support for agricultural development in Cambodia is not presently efficiently delivered. Therefore, the impact evaluation was designed from the outset to test what can be considered inexpensive additions to a basic model of government extension delivery, though it is also hoping to catalyse private sector involvement in extension. The impact evaluation highlights technologies that could be used by the private sector, and at a time in which the results can be useful for scale up during the second phase of ASPIRE.

The basic model of intervention in both PADEE and ASPIRE have been farmer field schools (FFS). Two systematic reviews have examined the impacts of FFS on farmer outcomes; Waddington et al. (2014) and Waddington and White (2014) focus on understanding the systematic impacts of FFS, while Phillips et al. (2014) focus on how FFS are targeted. For the purposes of this report, Waddington et al. (2014) is more meaningful. They review 90 studies on the impacts of FFS, finding that only 15 studies can be rated less than “high risk” for bias. The review generally finds that FFS can be effective at increasing knowledge and in promoting the use of effective practices. They also find that FFS increase agricultural yields by 13 percent and profits by 19 percent. it is worth noting two points. First, none of the cited evidence is derived from randomized control trials; the only randomized control trial we could find on FFS is Guo et al. (2015), which examines the impacts of FFS on rice production, by attempting to teach farmers to use less fertilizer. They find an increase in knowledge of pest management, but no change in fertilizer use. Second, the follow-up time period varies greatly among the studies considered by Waddington et al. (2014); the shortest is less than a year, with the longest being 7 years. Therefore, impacts noted above are averaged over that time period.

Within PADEE, we randomize two potential mechanisms to attempt to address problems related to information provision and monitoring, though they are randomized in an additive fashion. First, we test e-PADEE software, that was developed to provide extension agents with tablets equipped with specialized software with information about soil testing results, seed recommendations, fertilizer applications, and identification and treatment of crop diseases. In a second arm of the trial, we test whether performance-based incentives can incentivize extension workers to make use of information available in the software to increase their effectiveness. For these purposes, we will gauge farmers’ knowledge of practices related to agricultural advice that extension services should provide. Extension workers in this arm were provided with monetary bonuses based on the percentage of correct answers of farmers in their catchment areas. The control group was defined as PADEE business as usual.

Within ASPIRE, community extension workers (CEWs) who were mobilized were all given tablets with a number of different applications on them in mid-2017, making a second test of the e-PADEE software infeasible. We therefore worked with MAFF to consider alternatives to e-PADEE rollout for randomization. We ended up focusing on using a push-message based system that used the same messages as e-PADEE for rice, and we developed further messages for vegetables and chicken rearing (in villages with a focus on chicken rearing through ASPIRE). We then pushed recorded messages to farmers’ and CEW’s phones, timed to the agricultural calendar. Randomization involved four groups, all of which are of particular interest to policy makers at MAFF and at the General Directorate for Agriculture (GDA)— a “basic message” group, in which we will push basic messages to the group; “enhanced messages”, in which messages will be more frequent and timed to the agricultural season; “enhanced messages plus”, in which we are adding farmers outside of the farmer group that received the messages; and a control group, which receives ASPIRE as usual. All of our treatments are therefore additive on top of the primary program.[1]

In the pre-analysis plan initially submitted to 3ie, we did not explicitly write out research questions; rather, we discussed the research hypotheses in the context, as described above, and specified the primary and secondary outcomes of interest. However, the pre-analysis plan filed did not cover the ASPIRE intervention. As a result, in a modification of the pre-analysis plan we recast the primary questions as follows:

1. Can ICT methods strengthen the effectiveness of traditional extension in terms of farmer learning?
2. Do ICT methods enhance the effectiveness of traditional extension on farming outcomes?
3. How cost effective are ICT-related additions at increasing farmer's learning / outcomes?
4. Can financial incentives for extension workers improve the effectiveness of extension programs in developing countries?
5. By reducing costs of information delivery and increasing the critical mass of informed farmers, can ICTs improve farmers' knowledge and adoption of agricultural practices?

Whereas the first three research questions relate to both projects, question 4 relates to the pilot within the PADEE intervention and question 5 relates to the pilot within the ASPIRE intervention. The remainder of the report is structured as follows. In the second section, we discuss the interventions in more detail, the theory of change in each case, and the research hypotheses. The third section places the study better in the context of agriculture in Cambodia, and the fourth section provides a timeline of the two interventions. The fifth section provides a detailed description of the research design and implementation. The sixth section includes information on how the interventions were implemented in practice, and the seventh section provides impact analysis and answers the key evaluation questions. The eighth section discusses the results, and the ninth section concludes with findings for policy and potential further research. In each section, because we are discussing two interventions, we first discuss the intervention within PADEE and then the one within ASPIRE.

2 Intervention, theory of change and research hypotheses

The interventions studied in this impact evaluation took place within the context of the PADEE and ASPIRE projects of MAFF. While there are differences in their implementation and mechanisms, both projects aim to increase smallholders' agricultural productivity through innovations in extension. Both PADEE and ASPIRE follow something of a train-and-visit extension model in their basic workings, with visits from extension workers.

[1] Note that an overall evaluation of ASPIRE is also taking place, and villages could not be randomized, so we decided not to include a "pure" control group as it would simply repeat the work being done by the group conducting the overall evaluation.

2.1 PADEE and the ePADEE Intervention

PADEE was launched in five southern provinces (Kampot, Takeo, Kandal, Prey Veng, and Svay Rieng) of Cambodia in mid-2012 and operated until 2017. Farmers in PADEE villages were grouped in learning groups with approximately 50 members and received agricultural extension advice from a member of the Mobile Support Teams (MSTs). MSTs are itinerant extension workers and are civil servants staffed by MAFF's District Agricultural Offices.

As part of the PADEE operations, MAFF – in collaboration with the GDA, Grameen Intel, SNV, and iDE – developed ePADEE, a specialized software for extension workers to provide them with information about seed choices, fertilizer application, and plant disease control related to rice production.[2] ePADEE includes a module to analyze individual plot soil tests to provide personalized recommendations based on each farmer's needs. Software content was developed by MAFF's specialists based on Cambodia's local agricultural conditions.

A small group of MSTs were provided with tablets loaded with ePADEE and the software was tested in a small pilot by two NGOs (the SNV Netherlands Development Organization and International Development Enterprises - IDE) throughout 2015. Each MST worked with an average of 7 farmers in the pilot, so few PADEE beneficiaries had been exposed to ePADEE in general. Furthermore, the impacts of using ePADEE could not be established from the pilot, since there was no survey and no attempt to establish a counterfactual. Consequently, it was of interest to both MAFF and Grameen Intel to help devise an evaluation of the e-PADEE product, to observe whether it would make sense to integrate it into plans for ASPIRE.

2017 was effectively the last year that PADEE was implemented, due to budget constraints that led it to be closed earlier than had been originally planned. Meanwhile, ASPIRE operations began slowly in 2016, but became more widespread by 2017. The PADEE provinces are scheduled to be added to ASPIRE in 2019, so any lessons from the pilot can be directly brought into the same geography when these provinces are added to ASPIRE.

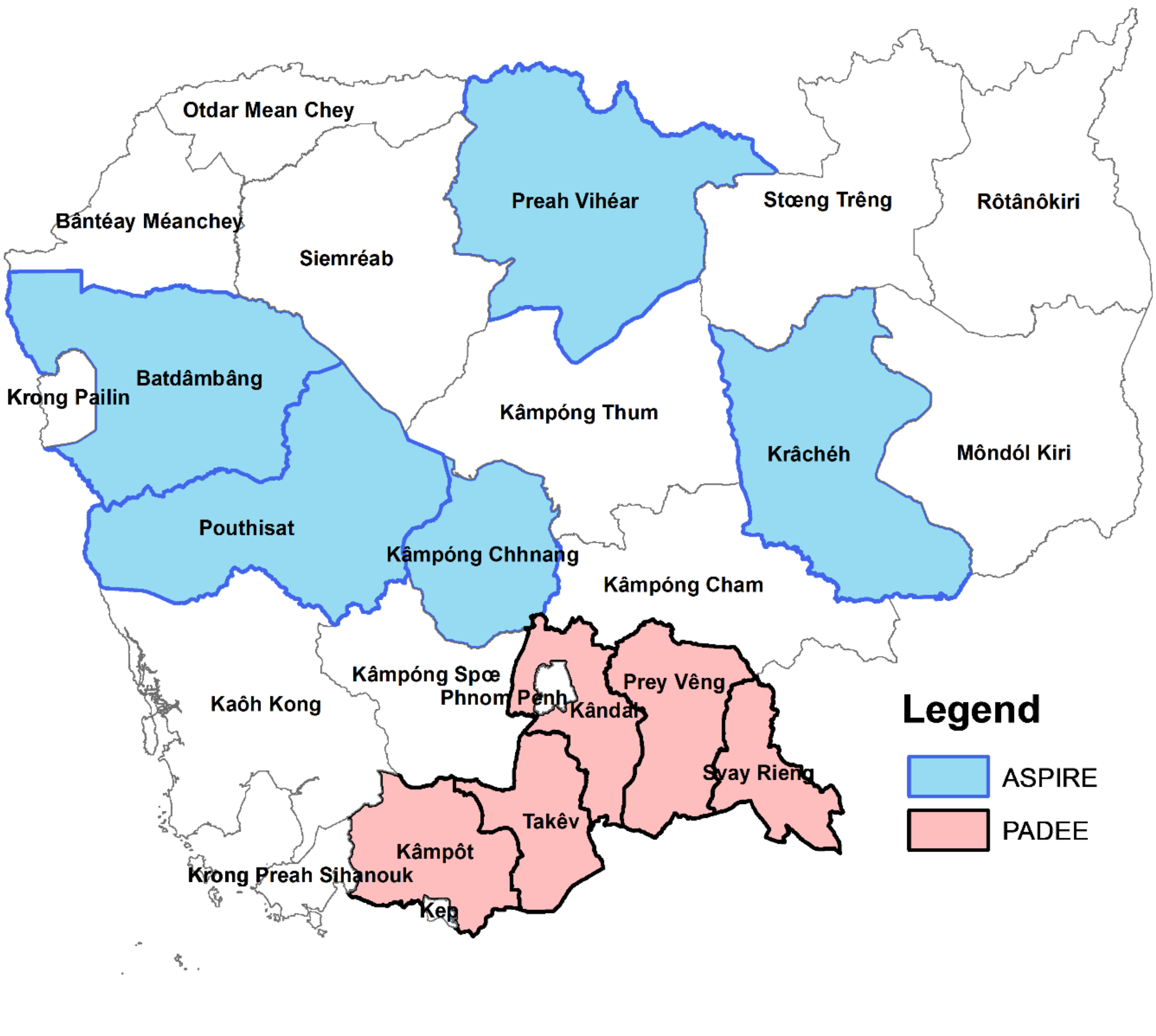
2.2 ASPIRE and Direct Calls

ASPIRE is being implemented in five provinces that were previously not targeted by PADEE (Battambang, Pursat, Kampong Chhnang, Preah Vihear, and Kratie; Figure 1). The basic extension model in ASPIRE is similar to the one in PADEE: farmers are organized in learning groups and extension workers provide them with agricultural advice. However, ASPIRE incorporates local community members that act as liaisons between extension workers and farmers. Each village has a Commune Extension Worker (CEW) to facilitate relations with farmers. The goal is that CEWs can both share information with farmers in their assigned

[2] iDE, which operates a social enterprise called Lors Thmey, continued to use ePADEE after the completion of both the pilot and the set up of the impact evaluation. In doing so, they have developed a module for vegetable farming that is meant to try to help develop a profitable business including extension. After their involvement in PADEE, however, they stopped working with the government.

villages—each CEW is assigned six villages—and to also help private actors liaise with villages who might purchase their inputs or provide specific types of outputs.

Figure 1. Map of Cambodia, with highlighted PADEE and ASPIRE Provinces



Note: Authors' Calculations, GADM database of global administrative areas (boundaries).

In early 2017, the decision was made to provide all CEWs who had begun work in 2016 with tablets that would be pre-loaded with information to potentially share with farmers. As a result, there were several difficulties with attempting to continue the e-PADEE pilot within ASPIRE provinces. First, at least in theory, CEWs will have access to a great deal of information, which would potentially swamp out the information from e-PADEE. A second concern came up discussing the Grameen Intel software going forward, which was that it did not provide for peer-to-peer information flows, and third, because of the contracting process being followed by the government, there is an additional challenge in keeping ePADEE application up-to-date as conditions, recommendations, and input availabilities change. Finally, training costs would be substantial as would additional political buy-in, since some CEWs would require the ePADEE software while others would not be allowed to have it, so separate or additional trainings would need to take place, and provincial authorities would have to approve the differences in tablets and training for some but not all CEWs. Given all these challenges, it was decided to conduct an alternative innovative test within ASPIRE.

Through discussions with IFAD and MAFF, we all agreed upon an alternative test, which was to use a push-message based system, which calls farmers on a weekly basis with basic extension messages.[3] The rice messages directly mirrored the basic rice extension messages in the ePADEE software, but farmers were also offered messages on either growing vegetables (long beans and cucumbers) or rearing chickens, based on choices made by farmer groups in their villages for 2016. The messages were delivered through a firm called VOTO Mobile (now Viamo mobile). MAFF collaborated with IFPRI on developing the messages out of their training manuals. We pushed the recorded messages to farmers' and CEW's phones, timed to the agricultural calendar.

2.3 Primary Outcomes

Our project revolves around two primary research hypotheses, one of which is tested under both projects and one of which is tested only in the PADEE intervention. First, we hypothesize that ICT based extension can be an appropriate tool for making extension more cost-effective. By effective, we mean that it can lead to additional farmer learning and better farming outcomes, including improved yields and farm revenue. Second, we hypothesize that better incentive mechanisms for extension workers can lead to improved interactions between farmers and extension workers. Our outcomes of interest are as follows:

Have a positive effect on farmers' knowledge of agricultural practices: We will collect farmers' knowledge of different "families" of practices: adequate fertilizer application, seed selection, and management of crop diseases; vegetable production techniques; chicken rearing knowledge, etc. We will create indexes for each family using the methodology outlined by Kling et al (2007), who propose creating summary measures by adding normalized outcomes.[4] We will also create an aggregate measure that will include practices in all families created for each intervention.

[3] Incidentally, in discussing text messaging as a possible delivery mechanism, we learned that many Cambodian farmers do not often text, because many of the phones do not support the Khmer script. So we did not send text messages, only pushed phone messages.

[4] Any particular outcome will be excluded when there is limited variation in the answers (e.g., everyone answers "Yes" or if everyone reports similar values).

Lead to adoption of new agricultural practices: Similarly, we will measure adoption by creating indexes for the adoption of practices across families of practices (adequate fertilizer application, seed selection, and management of crop diseases, vegetable production, chicken rearing knowledge) and an aggregate measure of the three families. This outcome will reflect the intensive measure by which farmers are adopting potentially beneficial practices.

Lead to larger expenditures on selected agricultural inputs: To measure the extensive measure of adoption of practices, we will investigate whether the interventions increase farmers' expenditures on selected agricultural inputs. This will be measured by the households' expenditures in adequate fertilizers, seeds, and recommended inputs required to treat or prevent crop diseases.

Increase in agricultural yields: We will measure agricultural yields by dividing the total harvest achieved by farmers during an agricultural season by the number of hectares allocated to crops, at least for rice. We will test both the yield as well as the logarithm of yields.

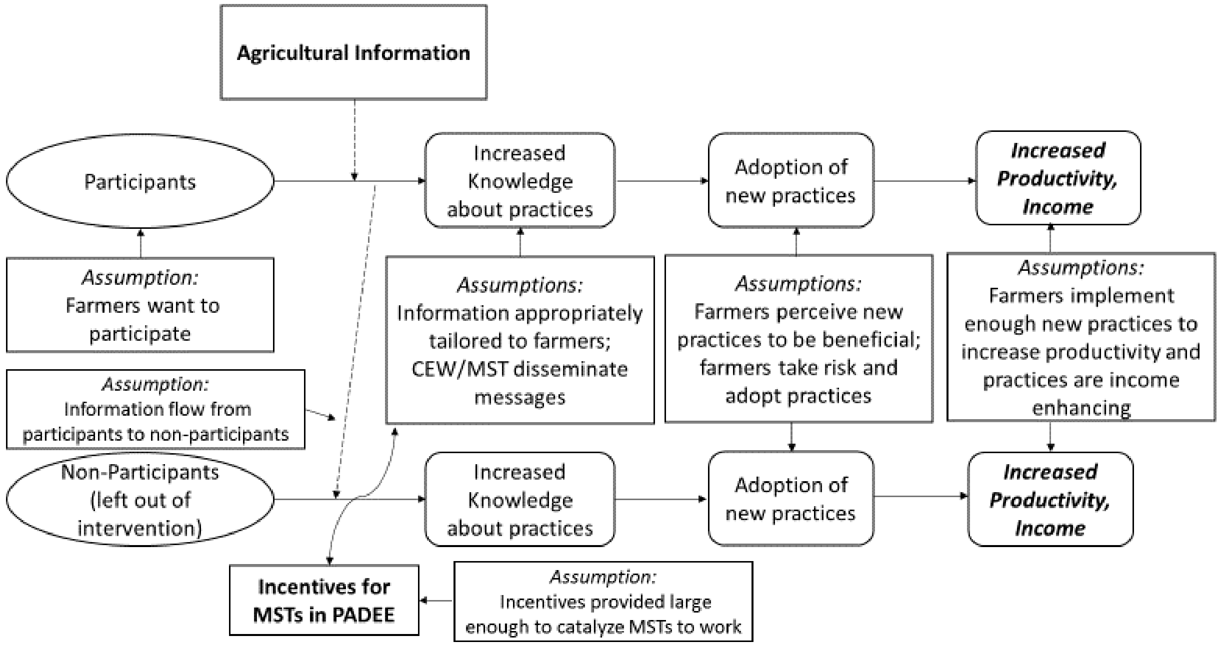
Increase farmers' agricultural income: We will measure farmers' income from agricultural activities, including cropping and poultry income. We will compute agricultural income by adding up the value of crops produced, using prices either internally generated by the survey (average prices for sold product within the household, or the median at the village level if the household did not sell) or from detailed surveys conducted by our partners, the Cambodian Development Research Institute, if not available for some rarely traded vegetables. We will then subtract input costs.

Increase value of production per hectare. Finally, we will take both gross and net values of agricultural production and divide by land size, to measure whether households increase crop income on a per land unit basis.

2.4 Theory of Change

We hypothesize a reasonably straightforward theory of change for improving extension in Cambodia through innovations (Figure 2). First, we consider the pathway through direct beneficiaries. We hypothesize that new models of agricultural extension will lead to increased awareness or knowledge of different agricultural practices or techniques. We assume direct beneficiaries want to participate in the additional extension. The techniques depend a bit upon the type of extension. For example, improved extension can lead to will lead to increased awareness of more appropriate inputs (seeds, fertilizer, pesticides, herbicides) and practices (e.g. timing of fertilizer application; placement of seeds). To ensure that the innovative extension model leads to improved knowledge, several assumptions must be met. Information must be tailored appropriately for farmers, and those in charge of delivering an extension model must actually disseminate the messages. In the case of the MSTs in PADEE, we further test whether improved incentives for delivering extension will lead to improved delivery; the assumption is that incentives are large enough to catalyse the work required.

Figure 2. Schematic of theory of change



We are also interested in potential spill-overs of information from those who participate to those who do not participate, or indirect beneficiaries. For these potential beneficiaries, the information flow must come through beneficiaries rather than through the project; we must assume that the information flow occurs and that it is accurate. If so, it would lead to increased knowledge about improved practices among those non-participants. Given that there may be less information in that flow, we may further want to assume that such flows will be weaker than flows directly through the intervention.

The remainder of the theory of change is the same for direct and indirect beneficiaries. The increase in knowledge leads to adoption of the new practices. Here, we assume that farmers perceive the new practices to be beneficial, and that they are willing to take the risk of attempting adoption. If they adopt practices, then we can observe increases in agricultural productivity, within the targeted product, and potentially agricultural income as well. For those final outcomes to occur, we must also assume that farmers implement enough new practices to increase their productivity. Here, we are using an expansive definition of productivity; we could mean yields, but we could also mean quality improvements that do not necessarily lead to higher yields but do lead to higher farmgate prices for products. It is worth noting that this process may take more time than is generally allowed for in the impact evaluation; for example, if farmers begin to experiment with techniques that can be derived from additional knowledge, then we might not observe initial impacts on productivity at the endline, but potentially could if there was more time available between rounds. Nevertheless, in the theory the increased productivity would lead to higher welfare levels, therefore increasing resilience against shocks through self-insurance.

Within each project, the assumptions that we make slightly differ. For the PADEE intervention, we posit that increases in the (correct) information through ePADEE will potentially lead to the benefits noted above. We are also specifically considering rice yields; we then assume that revenues also increase because quality does not decline (which would reduce prices).

In the ASPIRE intervention, we make the same assumption that additional information through the direct phone calls will also lead to the above benefits. We need to first assume that people will accept the phone calls, as well as have their phones electrically charged at most times to receive those calls. We must assume that the right person can listen to the calls, and then we assume that the person finds the messages useful and listens to the calls. If all of that occurs, then for impacts beyond knowledge retention the farmer must act on the knowledge from the calls and experiment either with techniques or new crops/methods of raising chickens to experience yield impacts or revenue impacts. We finally assume that the calls are interesting enough for people in villages to talk about them, at which time there can be indirect effects among non-recipients. We need not make any assumptions about CEW behaviour, since they do not participate in the intervention itself.

This discussion suggests that we broadly categorize the outcomes listed in the previous section as intermediate and final outcomes. Intermediate outcomes relate to improved knowledge. We will directly ask about knowledge of recommended agricultural practices after asking about information diffusion, so we understand whether such measures change as a result of the pilots within PADEE and ASPIRE, relative to business as usual Improved knowledge does not necessarily lead to behaviour change; in terms of behaviour change we examine self-reported measures of use of techniques after we have asked about knowledge of each technique.

Final outcomes include productivity and welfare. We will measure productivity through the household survey and we will measure welfare primarily through per capita and per adult equivalent expenditures, also through the household survey.

3 Context

Both PADEE and ASPIRE programs featured agricultural extension components. In general, both PADEE and ASPIRE selected areas with agricultural potential and vulnerability to poverty. IFAD rolled out PADEE in five Cambodian provinces in 2012. In 2015, it started the implementation of ASPIRE in five additional provinces. Upon the end of PADEE in 2017-2018, it planned to include the five PADEE provinces into the ASPIRE program. According to the 2008 population census, the ten provinces of both programs cover about 50% of the total population of Cambodia. Recently, the ASPIRE loan was expanded so that the remaining provinces could be covered in its second phase.

The study sites for the ePADEE pilot were selected randomly from a sample of all sites in which PADEE was slated to continue to work in 2016. Since we included all five provinces, we have to assume that the villages were broadly representative of the provinces in which PADEE operated. The threat to this assumption is that the final villages in which PADEE operated could have systematically differed from other villages; however, there is not an obvious data source with which we can test this assertion.

The ASPIRE provinces were chosen a bit less systematically, as we had to choose provinces in which enough activity had occurred in 2016 for our sampling purposes. All the resulting villages were in Battambang and Pursat provinces. Again, there is not an obvious data source to examine for representativeness. However, we can look at some simple statistics from the latest (2014) Demographic and Health Survey (DHS) to consider how the selected provinces differ from rural Cambodia as a whole, and then we examine some statistics from our baseline surveys against surveys conducted for evaluating the PADEE and ASPIRE programs, respectively. The DHS is well known as a high quality data source, but does not collect much economic information; we decide to measure three measures at least somewhat correlated with multidimensional measures of poverty. In particular, the last measure (the child stunting rate) is often strongly correlated with poverty.

To provide a some sense of how the selected provinces look relative to rural Cambodia in terms of poverty, we examine three indicators of nutrition from the DHS surveys: the anaemia rate and stunting rate for under 5 year olds, and the proportion of women of child bearing age with a low Body Mass Index (BMI).[5] The DHS demonstrates that the seven provinces are largely distributed around the averages for all of rural Cambodia (Table 1). Children in the PADEE provinces appear a bit better off than the rural areas of the overall country, whereas women appear approximately average in terms of low BMI. The opposite is true in the two ASPIRE provinces; whereas Pursat has quite poor child nutrition statistics, they are relatively good in Battambang; they average to being about the same as rural Cambodia as a whole. In terms of women's BMI, the two ASPIRE provinces appear slightly better off than other provinces.

Table 1. Selected Statistics at the Province Level, 2014 Cambodia Demographic and Health Survey

Province or Area	Low BMI Rate, Women	Anaemia, under 5 year olds	Stunting Rate, under 5 year olds
All Rural Cambodia	14.1	57.4	33.8
PADEE Provinces			
Kampot	17	57.3	25.2
Takeo	13.9	53.1	30.7
Kandal	17.9	58.6	28.1
Pray Veng	12.5	51.3	32.7
Svay Rieng	13.1	49.8	32.8
ASPIRE Provinces			
Battambang	11.9	49	24.9
Pursat	13.1	64.8	38.8

[5] Stunting is defined as having a height-for-age z score below -2; women are considered to be underweighted/low BMI if their BMI is below 18.5 (and they are not pregnant).

3.1 PADEE Context

PADEE's beneficiaries were determined using a two-step approach. First, PADEE selected the project's intervention communes. The Provincial Departments of Agriculture (PDAs) proposed an initial list of communes for the project, based on the following criteria: (a) high incidence of poverty and large numbers of poor households, (b) agricultural potential, (c) exposure to natural hazards, (d) food insecurity over the year, (e) vulnerability of women and children, and (d) commitment of administrative councils to work with communities and farmers. This initial list of communes was further streamlined by: (a) excluding urban communities, (b) excluding communes with less than 200 poor households or less than 500 hectares for rice cultivation; (c) excluding communes with poverty rates smaller than 19%. To maximize the cost-effectiveness of the implementation of the project, PADEE ruled out all districts with five or less communes meeting these criteria. Within the remaining districts, PADEE prioritized the poorest eight communes for their program. As a result of this selection process, the project targeted 246 communes in 36 districts.

Once the communes were determined, beneficiaries were selected following a participatory wealth ranking approach. The project convened meetings with villagers, community councils, village chiefs, and district staff. Meeting participants ranked village households based on their own perceptions and definitions of poverty. The fifty poorest households were invited to participate in the project. A final filter was applied to ensure willingness to participate and carry out responsibilities associated with the program.

To attempt to consider the external validity of our study, we compare make a few comparisons with the PADEE areas, we use statistics on four variables generated from the overall PADEE baseline that are available at the provincial level (Table 2). We find some differences between our baseline and the overall PADEE baseline. First, we find that households in our sample are somewhat larger; note, the same was found in the PADEE evaluation midline survey (SBK Research and Development, 2016). We find similar patterns of availability of mainline electricity; as in the evaluation baseline, most households in our sample have electricity in Kandal and Takeo, but fewer in Kampot and Prey Veng. In Svay Rieng, few households have electricity. Rice yields in our sample are more variable than in the evaluation baseline, but they are also based on smaller samples, so the means are more variable. Finally, we find fewer households grow fruits and vegetables, though our survey did not ask specifically about fruits, unlike the PADEE evaluation baseline, so in a relative sense our measures are biased slightly downwards. In general, between results on household size and rice yields, we believe that the external validity for the remainder of the PADEE areas is reasonable.

Table 2. Comparison of PADEE evaluation baseline with ePADEE trial baseline survey, Cambodia

	Household Size	Electricity	Rice Yields	Grew Fruits/Vegetables
Panel A: PADEE Evaluation Baseline (2013)				
Overall	4.42	62.4%	2.85	57
Kampot	4.75	48.2%	2.29	47
Kandal	4.41	90.6%	2.65	40
Prey Veng	4.27	45.4%	3.07	77
Svay Rieng	4.30	36.3%	2.52	68
Takeo	4.53	88.9%	3.15	36
Panel B: ePADEE Baseline (2016)				
Overall	4.86	68.8%	2.99	30.1
Kampot	4.43	69.1%	2.99	29.6
Kandal	5.17	98%	4.01	21
Prey Veng	4.95	66.4%	2.85	23.6
Svay Rieng	4.55	17.7%	2.06	48.6
Takeo	5.17	91.5%	3.48	29.2

Notes: PADEE evaluation baseline included ¼ of the sample outside communes targeted for PADEE; reports did not include standard deviations or standard errors. ePADEE baseline only asked specifically about types of vegetables and not about fruits.

3.2 ASPIRE Context

ASPIRE began its operations in 2016 in five additional provinces not covered under the PADEE program. These provinces were selected based on their potential to increase agricultural GDP and the capabilities of the Provincial Departments of Agriculture. Based on these criteria the program selected the following five provinces: Kratie, Preah Vihear, Battambang, Pursat, and Kampong Chhnang. ASPIRE will expand into the five PADEE provinces in 2019.

Within the initial five provinces, their targeting differed somewhat from that in PADEE. ASPIRE aimed to target communes that would maximize the cost-effectiveness of extension programs, a relatively recent view of the government. Therefore, it began targeting with communes that were believed to have considerable unrealized agricultural potential: it included those where significant production gains could be achieved, excluding those that already had high productivity levels. While ASPIRE targeted poor farmers, it also theoretically reaches out to somewhat non-poor farmers who are vulnerable to risks and shocks.

Within communes, they targeted both poor farmers and non-poor farmers. In the view expressed in the project design document, “the participation of less poor smallholders is also important from the knowledge dissemination point of view as this type of farmers are better situated to try and demonstrate new technologies to the effect that poorer and more risk averse farmers can observe the results and adopt (IFAD, 2014).”

The overall ASPIRE evaluation baseline was based on a much smaller sample than the PADEE evaluation (340 households versus 3050 households). Therefore, the report based on the baseline did not include provincial breakdowns for most variables (SBK Research and Development, 2017). Moreover, it did not ask about electricity access, so in describing means from the ASPIRE evaluation baseline against our sample to assess external validity, we replace that variable with "ID Poor" status, which is a program developed by the Cambodian government to identify poor households and target them with assistance. Note that the initial ASPIRE baseline sample is small and so likely has relatively large standard errors associated with measures; the report to the government cited above did not include standard errors.

Perhaps not surprisingly, we find that the households in our sample appear to be worse off than in the ASPIRE baseline in general (Table 3). Whereas household size is similar on average, the ID Poor rate is almost 10 percentage points higher, and almost equals to the rate of ID Poor households generally in each province (32.6 percent in Battambang according to the Ministry of Planning, and 22.8 percent in Pursat). Not surprisingly, rice yields are a bit lower in our sample than the ASPIRE evaluation baseline, but more households grow vegetables. Recall, however, that the IFPRI sample includes a majority of non-farmer group members, while in theory the ASPIRE baseline should only include farmer group members. So whereas our results will not be externally valid for ASPIRE participants, they will reflect a larger portion of the rural population of the two selected provinces.

Table 3. Comparison of ASPIRE evaluation baseline with the IFPRI impact evaluation baseline

	Household Size	ID Poor Status	Rice Yields	Grew Vegetables
ASPIRE Baseline (2016)	4.58	19%	3.24	34
IFPRI Impact Evaluation Baseline (2017)	4.52	28.4%	2.66	46.2
Battambang	4.73	32.7%	2.92	54.0
Pursat	4.26	23.0%	2.40	36.6

4 Timeline

Figure 3 shows the timeline of activities for the ICT and incentives intervention in the PADEE project. This intervention was implemented in the wet season of 2016 and focused on rice production.

Figure 3. Timeline of ICT + Incentives intervention in PADEE Project

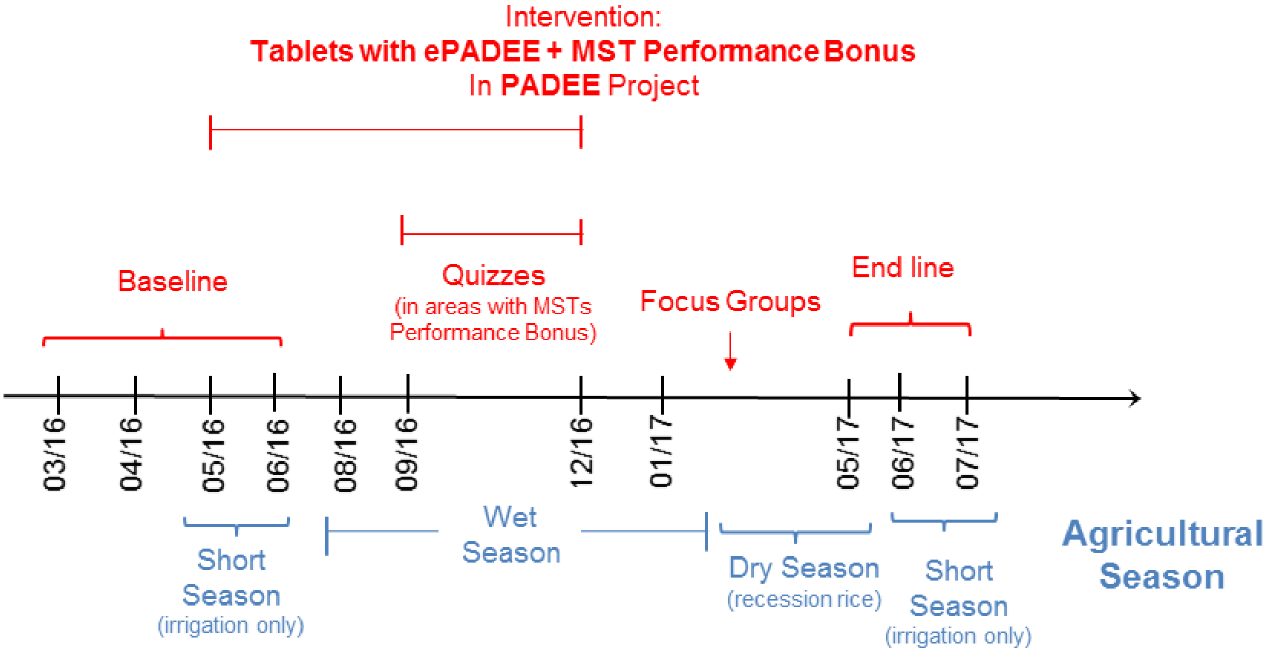
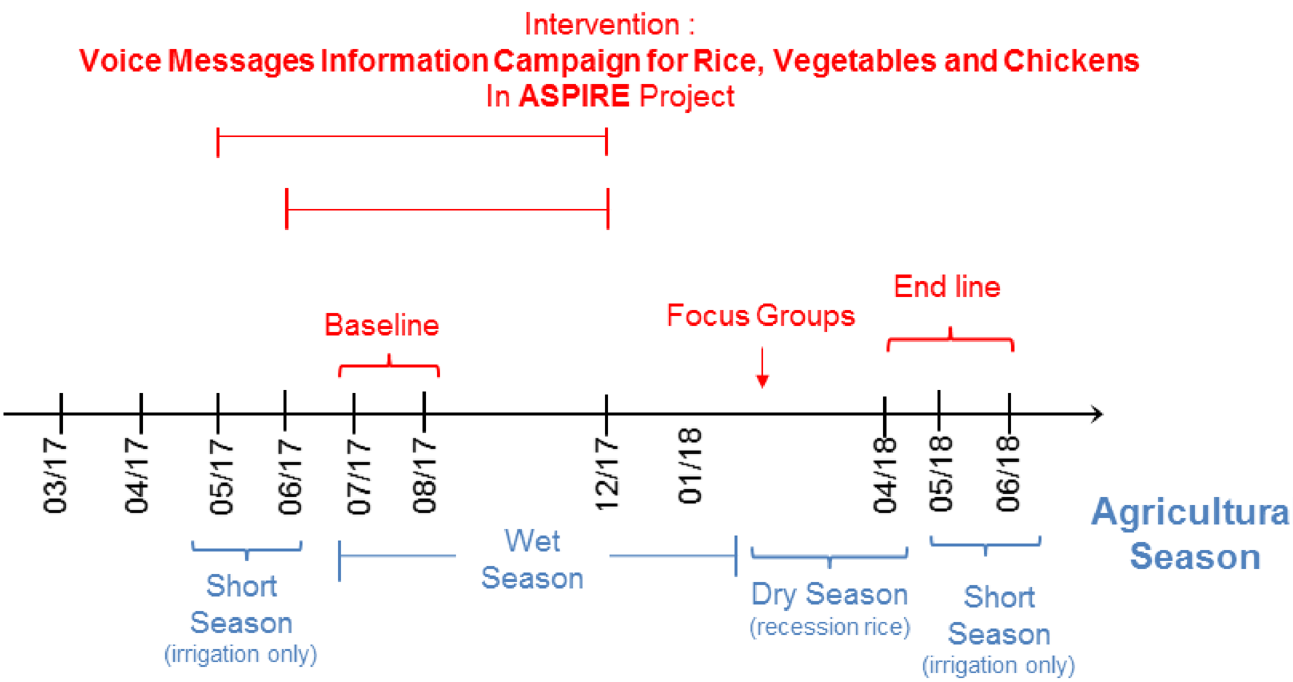


Figure 4 shows the timeline of activities for the voice messages information campaign intervention in the ASPIRE project. This intervention was implemented in the wet season of 2017 and focused on rice production, vegetable production and poultry rearing practices.

Figure 4. Timeline for voice messages information campaign in ASPIRE project



5 Evaluation: Design, methods and implementation

In this section, we describe the evaluation of the two innovations in PADEE and ASPIRE, which took place through clustered randomized control trials (RCTs). To ensure that the two RCTs were implemented ethically, the plans were reviewed by IFPRI's Institutional Review Board (IRB, which has Federal-wide Assurance no. FWA00005121). As is often common in developing countries and trials that do not involve measures of health, there is no corresponding review board in Cambodia. As plans evolved amendments were sent to the IFPRI IRB on an annual basis.

5.1 PADEE Evaluation Strategy

The evaluation of ePADEE took place through a clustered randomized control trial. The randomized control trial involved two treatments and one control group. We call the two treatment groups the ICT and ICT Plus groups, respectively. The two treatments are described in more detail below.

The **ICT** group is comprised by 20 villages where the MSTs assigned to work in these villages receive a tablet loaded with the ePADEE software. The **ICT Plus** group is comprised of 20 villages where the MSTs assigned to work in these villages receive a tablet loaded with the ePADEE software receive a bonus payment (incentive in addition to their base salary) based on their performance, as measured by the response of their assigned farmers to periodic monitoring telephone surveys. Each month, we randomly selected 6 farmers in each farmer group and test them with a ten-question quiz over their mobile phones. Three questions were "soft" questions, assessing whether the farmer had seen the MST in the village during the last month, had talked to him, and had received any useful advice. Other questions were based on knowledge of particular issues on which the MSTs should have been advising farmers. At the end of the season, each MST received a bonus payment based on the average of their performance across the season. Each MST group was monitored three times during the season, timed to specific timelines proposed in the software.

Our impact evaluation sample (i.e., the one for which baseline and endline outcomes will be collected) is comprised of 20 farmers in each learning group. A concern is that MSTs might focus on farmers that evaluation has contacted before and not in the complete learning group (i.e., MSTs might be redistributing their effort and not necessarily increasing it). For this reason, all 20 members of the learning groups were part of the performance-monitoring scheme.

For both treatment groups, IFPRI, MAFF, and Grameen Intel staff collaborated to hold workshops in June-July 2016 to both re-teach the MSTs on how to use the software and reinforce the knowledge of the MSTs in both treatment groups. Each MST will receive payments through a mobile transfer provider in Cambodia (Wing), which includes a monthly allowance to finance their visits to the farmer group. The ICT Plus group receives a marginally lower base pay than the ICT group. At the end of the agricultural season the ICT Plus group receives a lump sum payment and an explanation of the payment using the performance monitoring results.

A third group of 20 villages was assigned to the **Control** group. Villages in this group receive regular PADEE extension (non-ICT) through MAFF. These villages have MSTs assigned to work with the learning groups, but the MSTs did not receive tablets with the ePADEE software and will not be eligible to receive performance-based bonus payments. Moreover, the overall MAFF budget did not allow for many visits from MSTs in 2016.

5.2 Sample Determination in PADEE

Throughout the length of the project, PADEE aimed to cover 2,367 villages in the provinces of Kampot, Takeo, Kandal, Prey Veng, and Svay Rieng. Villages were rolled in and out of the program in different stages. For the purposes of the ePADEE pilot project, we restricted our sample to villages in which: (a) PADEE would operate in 2016 (and presumably 2017); and that (b) MAFF had confirmed MSTs assigned to them by early 2016. The resulting sampling framework consisted of 170 villages. These villages were linked to 85 MSTs (approximately 2 villages per MST).

We then dropped villages in which anyone had experience with the ePADEE pilot and a few others in which we could not match the village to PADEE lists for that commune. We were then left with 98 villages and 62 MSTs. We then randomly dropped one village per MST for those who continued to have two MSTs, leaving us with 62 MSTs and 62 villages. We finally randomly selected 60 of the 62 village-MST pairs for the intervention.

The number of villages therefore selected in each province was roughly proportional to the share of villages each province had in the overall framework. As a result, the sample for our study in the PADEE project comprises 12 villages in Kampot, 10 in Kandal, 14 in Prey Veng, 11 in Svay Rieng, and 13 in Takeo. At that point, in collaboration with provincial MAFF offices we ensured that each of the 60 villages was assigned to a different MST; despite our efforts to assign one MST to each village, in two cases, an MST covered two villages due to changed assignments within provincial offices.

At that point, MAFF provided us with lists of approximately 50 farmers participating in the learning groups in each of the 60 villages in our study. From the 50 farmers in each learning group, we randomly chose 20 farmers for our household surveys and to potentially participate in the additional intervention. Therefore, our initial PADEE sample consists of 1,200 farmers across 60 villages in these 5 provinces.

We also conducted qualitative work as part of the PADEE fieldwork. The qualitative work included two components, conducted in selected treatment villages to understand farmers' experiences with the ePADEE program and with members of the mobile support team:

1. *Focus groups with farmer groups*: Using the farmer groups in our study as a starting point, focus groups can be used to understand common issues with e-PADEE software, the level of trust for the extension system in general and actors within the agricultural support system more specifically, as well as the relative importance of extension in their production. We will also explore what types of information farmers find valuable from

extension workers and in particular what else they would like to obtain from extension workers; e.g. how to tell fertilizer quality, help with vegetable crops, etc.

2. *In-depth interviews with Mobile Support Team members*: To complement the information obtained from the focus groups, we plan to conduct in-depth interviews with MST members to understand how the e-PADEE software could be made more user friendly and how one might be able to use CEWs in the ASPIRE project to help coordinate any need for extension visits, as well as to help understand how they perceive the incentives treatment if relevant.

5.3 Assignment of Treatment in PADEE

As previously stated, treatment was assigned for the e-PADEE intervention by randomization at the cluster or farmer group level. Each farmer group was associated with an individual MST, and the randomization took place before the MST training described above so that the 40 MSTs being given refresher trainings on the ePADEE software would be chosen ahead of time. The chosen sample included 10 villages in Prey Veng, 6 villages in Kandal, and 8 in Kampot, Svay Rieng, and Takeo, respectively. Randomization into the treatment groups was done through a random number generator in Washington. As the ICT treatment was being assigned at the village level, there were no concerns about jealousies arising due to treatment status within villages.

5.4 ASPIRE Evaluation Strategy

The evaluation strategy for direct phone calls to individuals in ASPIRE villages was developed through consultation with consultants working on information technologies within ASPIRE's national coordination unit. In consultation with ASPIRE and VOTO mobile, we determined that three randomized treatments and a control group would allow us to test different methods of delivering messages. Additionally, it was attractive to both the research team and MAFF to study farmers beyond the main farmer group, which is easy to do once meetings are not required. Hence, we broke up the sampling plan into a group of members and non-members within each village, which we will describe later in the report.

Therefore, the three treatment groups were as follows. First, there was a "basic messages group," in which basic messages were pushed once to farmers. Second, there was an "enhanced messages group," in which messages were repeated close to the time that action would need to take place within the season. Third, there was an "enhanced messages plus" group, in which messages were pushed to 50 farmers outside of the main farmer group. The idea behind the third group is that we can use that group to test whether a critical mass is required for the adoption of certain practices. Finally, we have a control group as the fourth group; the control group is conducted in a "ASPIRE business as usual" fashion.

The original design was to measure spillovers from the basic and enhanced message groups into the non-farmer group members, and then to ensure that the non-farmer group members in the enhanced messages plus group received phone calls. However, as implemented, in all three treatment groups the non-members in the sample were included in phone call lists. As a result, there is little difference between the enhanced messages and enhanced messages plus groups.

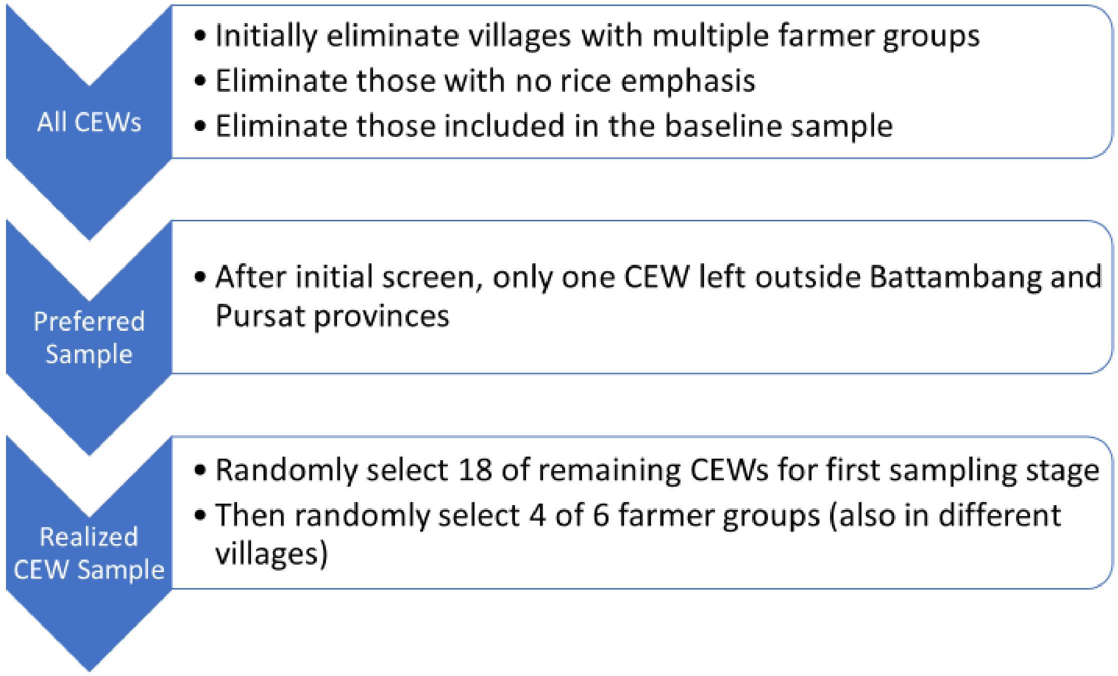
5.5 Sample Determination in ASPIRE

Beginning in 2016, IFAD and MAFF launched ASPIRE in Cambodia. In the first year of operation, the project planned to operate in 326 villages in the five provinces targeted by the project: Kratie (65), Kampong Chhnang (72), Preah Vihear (51), Battambang (70), and Pursat (68). Further villages were added to ASPIRE in 2017, but they were going to be added too late to include in the pilot project.

However, we were not able to fully confirm if the project was able to meet its first-year goal in terms of villages by the end of 2016, so as we negotiated a sample with the government, we determined where Commune Extension Workers (CEWs) had begun to work and in what villages. A further challenge also arose, as another firm conducted a baseline in some villages for an overall evaluation of ASPIRE, and we were asked to eliminate those villages from our potential sample. Finally, we realized we could not manage a sample in the same way as we did under PADEE, linking one CEW with one specific village, as had been done in the ASPIRE evaluation. After eliminating villages in which the baseline survey had occurred, there were not (nearly) enough CEWs to match each CEW with a specific village, meaning that some CEWs would need to be linked to more than one treatment.

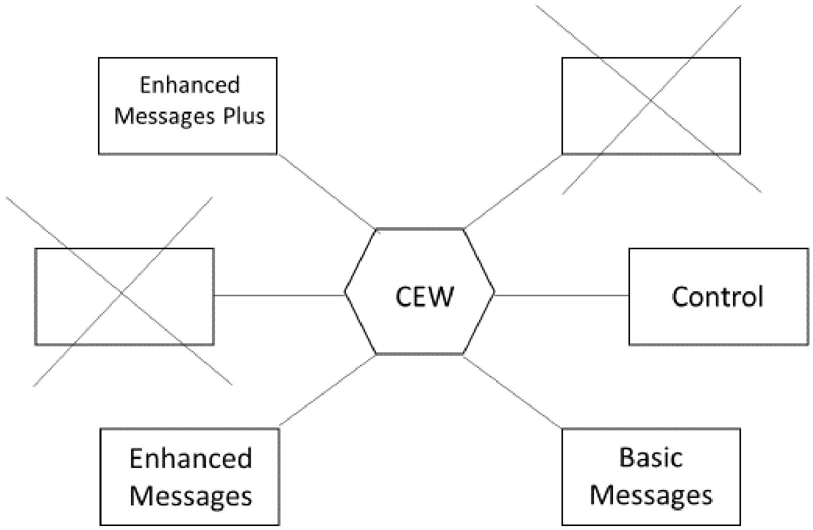
Given that the intervention that we agreed upon with MAFF did not require CEW involvement, we determined that the best method of sampling would be as follows. Each CEW works with six farmer groups; typically, the farmer groups were all in different villages. In some cases, however, CEWs worked with two farmer groups within the same village. We first screened CEWs to ensure that they worked with at least four different villages that had not been included in the overall ASPIRE baseline and were working with rice and either chicken or vegetables. After this initial screen, there was only one CEW outside of Battambang and Pursat provinces, so we decided to only randomly sample CEWs from those two provinces to save on data collection costs (see Figure 5 for an illustration of the sampling procedure). We sampled 18 CEWs from that remaining list.

Figure 5. Schematic of Overall ASPIRE Sampling Procedure for CEWs



In the second stage, we sampled 4 villages from among the up to 6 eligible villages for each selected CEW. The CEW was used as an initial stratum, so one village was selected from each treatment or control group around each CEW, so that CEW fixed effects could be used in estimation. Because the intervention does not involve the CEW, there is no concern about potential contamination. The sampling strategy at the CEW level is illustrated in Figure 6. The ASPIRE sample ended up comprised of 40 villages in Battambang and 32 villages in Pursat after 10 CEWs were selected in Battambang and 8 in Pursat, respectively.

Figure 6. Illustration of ASPIRE Sampling of Villages around strata formed by CEWs



The next challenge was to select farmer group and non-farmer group members for the sample in each village. MAFF and IFAD provided us with the lists of farmers that were organized in farmer groups to participate in ASPIRE. In each village, farmer groups were comprised of between 17 and 51 farmers. From this list, we randomly selected 10 farmers for our household surveys. In addition, we randomly selected 14 farmers not participating in ASPIRE from the village roster (see section 4 for the role of this group in the research design). Therefore, our ASPIRE sample consists of 1,728 farmers across 72 villages in these 2 provinces; 10 farmers in ASPIRE learning groups and 14 outside of the ASPIRE learning groups. Sample size was determined through power calculations detailed in Section 5.8.

The farmer group component of the sample was simple to obtain; we simply sampled 10 of the farmer group members. However, sampling farmers in the non-farmer groups was not trivial. To do so, we first came up with eligibility criteria. We needed to ensure that households included in the sample were actually farmers. Therefore, to be included in the non-farmer group samples, households had to meet one of the following three criteria:

1. Grew rice throughout a complete season (wet or dry) last year;
2. Has a home garden (where they allocate time and resources to prepare land and water the vegetables);
3. Have a chicken fence or have at least five chickens (of 0.5 kg or more) for meat consumption or egg production, so fighting cocks were excluded.

To then select farmers outside the farmer groups, field supervisors first requested the household roster from the village chief. Average village sizes in Battambang and Pursat was around 200 households. Those lists were checked against the list of farmer group households, and all farmer group households were dropped. The next step was to sample remaining households systematically, by dividing the total number of households by 14, which gives roughly equal size parts. For example, if there were 140 households remaining, they would split the list into 14 parts of 10. Then they would start from a random number (N), and pick the Nth household from each partial list, giving a total of 14 selected households. That list would be checked by the village chief, who would verify whether selected households were farmers by the above criteria or not. If not, then a list of non-farmers would be compiled, and the remaining sample would be split into parts based on the number of originally picked non-farmers. So, if four out of the 14 selected households do not farm, the remaining list of 126 households would be split into 4 parts, and every 26th household (because 126 divided by 4 is approximately 26) would be chosen, again starting from a random part of the roster. This procedure was repeated until 14 farming households were chosen. Then an additional six households were chosen using the same procedure as alternates in case the village leader had provided incorrect information, or the household was not available. To ensure that the procedure was followed properly by the enumeration team, the overall lead at CDRI checked the sample on a daily basis and supervisors conducting the randomization were required to take pictures of the rosters, so that the procedures could be replicated and checked.

5.6 Assignment of Treatment in ASPIRE

As described above, treatment assignment was done within each CEW cluster. Within each cluster, one village was assigned to either the basic model, the enhanced model, the enhanced plus model, or the control. As a result, there are 10 villages within each model in Battambang, and 8 villages within each model in Pursat. Randomization into the treatment groups was done through a random number generator at the IFPRI offices. As the ICT treatment was being assigned at the village level, there were no concerns about jealousies arising due to treatment status within villages.

5.7 Data Collection

Data collection was performed by the Cambodian Development Research Institute (CDRI) at baseline and endline for both pilot projects. CDRI translated survey instruments and provided input from a Cambodian perspective. But the research team at IFPRI was mainly responsible for the survey design and implementation plan. Before each survey round, CDRI in collaboration with IFPRI conducted a week-long enumerator training, followed by a pilot of the survey where the enumerators were further evaluated on their interviewing skills. The team of enumerators and field supervisors to participate in the survey activities that were selected based on a competitive and rigorous evaluation process during the training of the field team. Interviewers were both chosen on ability to ask questions in a neutral, similar manner, and on experience.

In each round of data collection, the field team was comprised of eight teams, which included one supervisor and five enumerators. Supervisors back-checked the data collected by households, and during the data entry process households were called by phone to clarify any discrepancies in the data. Households were not compensated for their participation in the survey, but refusal rates were extremely low.[6] The four surveys were collected as in Table 4, and in both cases enumerators made efforts to visit exactly the same households. Attrition rates, therefore, were 5.2 percent for PADEE and 2.4 percent for ASPIRE.[7] We discuss whether treatment was a determinant of attrition in Section 8.

Table 4. Timing and Size of Surveys

Survey	Timing	Number of Households
PADEE Baseline Survey	May 2016	1200
PADEE Endline Survey	April-May 2017	1127
ASPIRE Baseline Survey	June 2017	1728
ASPIRE Endline Survey	March-April 2018	1687

5.8 Sample Size Determination

In our original proposal, our sample size calculations included 4 treatments and a control group, which led to a sample of 3000 households. As we negotiated to work on PADEE and ASPIRE separately on the request of MAFF, we had to determine separate sample sizes for studying the ePADEE intervention and the VOTO mobile direct calls. Our sample sizes for the two interventions were basically determined by three factors: 1) the overall budget availability, 2) the availability of farmer groups and/or communities in which to conduct the surveys, and 3) power calculations that informed whether we could reasonably detect impacts.

For the ePADEE pilot, we were somewhat constrained by the number of farmers that each MST could reasonably work with at maximum within a farmer group, as well as statistical power. A rule of thumb in considering statistical power among continuous variables is that there is very little to gain with sample sizes beyond 20 observations within a primary sampling unit, and 20 farmers per group was an absolute maximum with whom MSTs were willing to work. As mentioned above, after dismissing villages and corresponding MSTs that had experience with ePADEE in its earlier phase, we were left with 62 possible MSTs; hence, we settled on a balanced sample of 60 MSTs (20 in each treatment arm).

Since we know the attrition rate (5.2%), we can provide here an *ex post* consideration of statistical power for both general discrete and standard normal continuous variables within our sample. The minimum detectable effect for a discrete variable with a baseline level of 10 percent (for example, a knowledge variable) is 0.112, or 11.2 percentage points.[8] Minimum detectable effects increase as the baseline level increase, to 18.4 percentage points at a 40 percent baseline

[6] Note that neither John Henry nor Hawthorne effects should play a role in affecting our results; the control group farmers should not have been aware they were in a control group and were receiving generally project benefits anyway, while Hawthorne effects should be balanced between the treatment and control groups.

[7] After observing the attrition rate for PADEE, IFPRI worked with CDRI specifically on improving their tracking procedures for data collection in the ASPIRE endline. However, as we will discuss later in the report, much of the attrition came from one province, Pray Veng, and that attrition was due to migration to Phnom Penh, which is adjacent.

[8] We continue to assume an intracluster correlation of 0.1.

level. For a standardized normal, the sample can detect a change of 0.37 standard deviations with no controls and 0.33 standard deviations if 20 percent of variation can be explained with controls.

Table 5. ePADEE power calculations based on sample of 60 villages, 20 households per village, 5.2% attrition rate

	Baseline Level	Minimum Detectable Effect	Minimum Detectable Effect, 20% Explained
Discrete Variable	10%	0.112	0.107
	20%	0.148	0.142
	30%	0.171	0.165
	40%	0.184	0.176
Standard Normal (Continuous)		0.37	0.33

For the ASPIRE intervention, we were particularly interested in understanding whether interactions could be measured between farmer group and non-farmer group members. We focused on determining a sample that both stayed within the remaining fieldwork budget and optimally split the sample between farmer group and non-farmer group members. We considered a number of different scenarios, largely from the perspective of rice yields and total value of production, in terms of different treatments (variants of the models that we used) and in terms of the split between farmer group and non-farmer group members. As noted earlier in the report, the idea was to ensure that a large enough sample of non-farmer group members might receive information from farmer group members; however, all of the non-farmer group members in all three treatments ended up receiving calls. Ensuring that treatment effects would be detectable under a reasonable amount of spillovers was our primary goal in developing the sample.

We used simulations to determine that under our budget constraint and feasibility constraints, the sample we chose from among a number of different options (3 treatments plus a control, with one calling non-members; 72 villages with 10 farmer group members and 14 non-members). We find that an increase in rice yields may be plausible to detect at around 15 percent for a direct effect and 20 percent for a plausible indirect effect, while a change in the value of output will be far more difficult to detect (Figures 7 and 8).

Figure 7. Simulated Power Calculation for Rice Yields, ASPIRE Sample

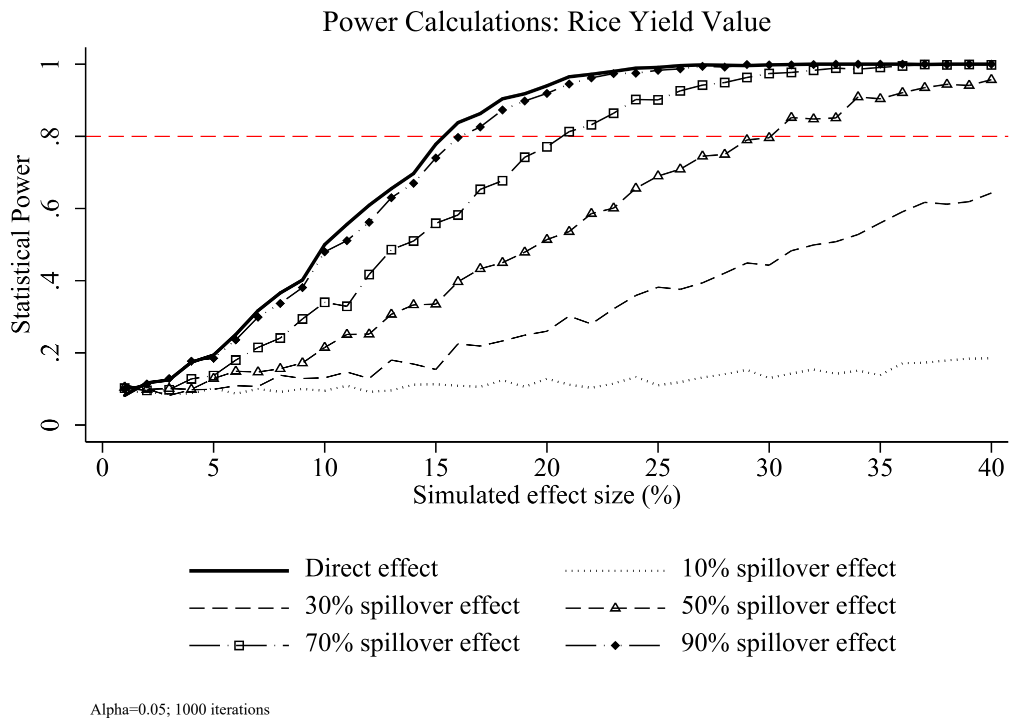
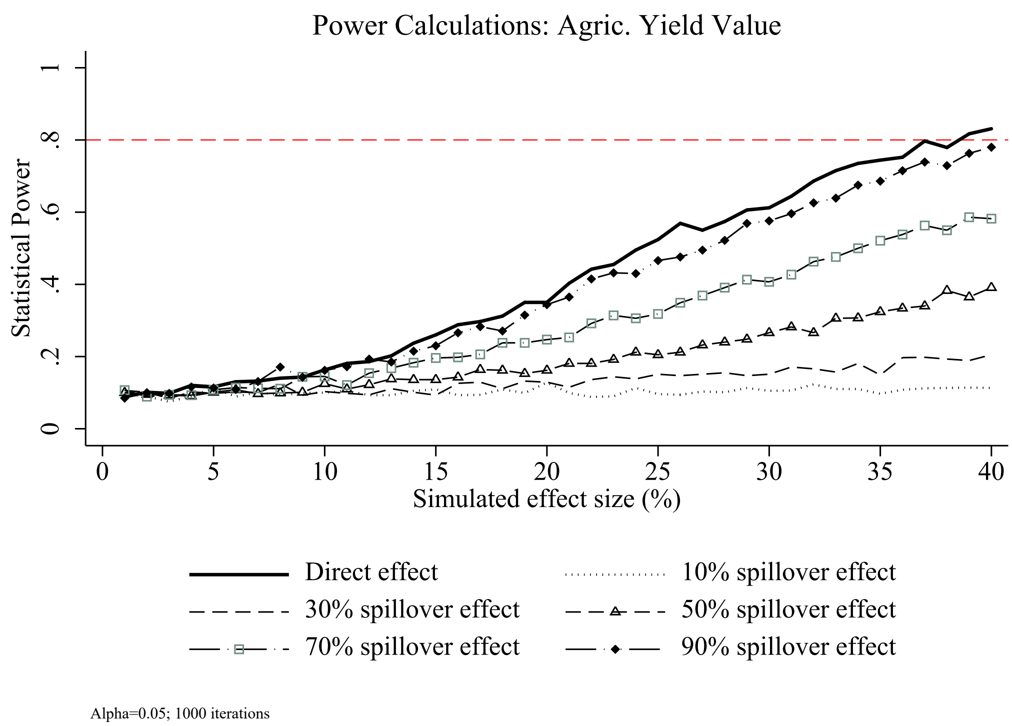


Figure 8. Simulated Power Calculation, Agricultural Revenues, ASPIRE Sample



Using the final sample, we can also consider an *ex post* calculation of statistical power given the realized attrition rate (Table 6). We find larger minimum detectable effects than in the ePADEE pilot, due to the three treatments rather than two. Minimum detectable effects, particularly with some explained variance, are somewhat lower with some explained variation, which should be easily possible with CEW fixed effects.

Table 6. ASPIRE power calculations based on sample of 72 villages, 24 households per village, 2.4% attrition rate

	Baseline Level	Minimum Detectable Effect	Minimum Detectable Effect, 20% Explained
Discrete Variable	10%	0.122	0.109
	20%	0.162	0.146
	30%	0.186	0.168
	40%	0.200	0.180

6 Programme or policy: Design, methods and implementation

The PADEE and ASPIRE programs themselves were originally designed by the International Fund for Agricultural Development in collaboration with MAFF. The development of the two extension innovations were developed in different contexts, which we describe again briefly below. We then explain delivery in each context and describe exposure as it was supposed to occur, and then we provide details on what actually occurred.

6.1 ePADEE Design

The ePADEE program had been designed by a consortium of MAFF, SNV, iDE, and Grameen Intel during the PADEE project and while our contract was being negotiated. Since it covered or was planned to cover much of, but not all, of the technology use described in our proposal, it was an obvious choice to test in a more rigorous manner. ePADEE consisted of three apps at the time, one of which used a soil test to make seed recommendations (and seeding methodology), a second app that used the soil test results to make fertilizer recommendations and use recommendations, and a third app that made recommendations about how to deal with specific pests and/or weeds.[9] The software was originally designed in Bangladesh but has been adopted to several different countries.

ePADEE was first used by farmer groups supported by SNV in PADEE as well as some of the business activities that were started and supported by iDE. The implementation of initial ePADEE activities involved 97 MSTs, but each worked with only a handful (around 7) farmers. Initial activities were heavily supported and monitored by a team from the GDA, which made regular field visits to MSTs. Monitoring in initial activities took place centrally through the Grameen

[9] A fourth app related to market information was also developed but not used in Cambodia; since the pilot project the four apps have been combined into one app and Lors Thmey has worked with Grameen Intel to develop a vegetable app as vegetables are a more lucrative crop for their business-oriented clients.

software, which registered which farmers had received recommendations from the software, at least for seed and fertilizer. Their monitoring indicated that the pests and weeds application was not used as frequently as the other apps.

In the pilot project here, the idea was for MSTs to work with 20 specific farmers from a farmer group with the app; we suggested one soil test for each group of ten farmers, to trade off some precision in requirements for cost of conducting the tests. As discussed in the randomization section, MSTs who were randomly selected for our ePADEE pilot were brought to Phnom Penh for a refresher training; the training reminded MSTs how to use the three apps, took them through practice, and placed emphasis on the app that had not been used as much in the initial activities. The training was conducted by Grameen Intel staff with support from IFPRI and a translator.

During the training, schedules for visits were discussed and worked out. The schedule is linked to the payment schedule for the MSTs in the ICT Plus group as well; with those MSTs, the amount and form of payment of per diems was discussed collaboratively. MSTs in the ICT Plus groups felt that it was fair to reduce per diems for visits by approximately 10%, and the amount that they earned from incentive payments could be paid in a lump sum at the end of the project. The following schedule was therefore worked out collaboratively:

Each MST was to visit each group five times, and a visit could take multiple days, as extension visits will be done in 2 groups of 10 farmers from a list of farmers surveyed in the village. The visits would include the following components:

Visit 1. During seed selection and planting period. During this visits the MST's will use the **seed selection module** of the tablets to advise their farmer group. In addition, MST's will perform a **soil test** and provide an initial **fertilizer recommendation**.

Visit 2: Visit before first fertilizer application to remind farmers about the fertilizer recommendation, and identify any further information and inputs needs.

Visit 3: Visit before second fertilizer application to remind farmers of the second fertilizer recommendation and check for pests using the **pest module** of the software.

Visit 4: Visit before third fertilizer application and check for pests.

Visit 5. Before harvesting to check for pests and identify post-harvest needs.

Payments of per diem, which took place through the mobile money system called Wing, would take place in three blocks:

Initial payments for visits 1 and 2

- Month 1 - Set up an initial payment to support travel for first visits
    - ICT: Base pay
    - ICT Plus: 90% of base pay
- Month 2
    - ICT: Base pay
    - ICT Plus: 90% of base pay

Second payment for visits 3 and 4

- Month 3/4
  - ICT: Base pay
  - ICT Plus: 90% of base pay

Third for last visit and incentives

- Month 4/5
  - ICT: Base pay
  - ICT Plus: Base pay + Incentive based on average score of the farmer groups during the season.

There were two main deviations from an "ideal" protocol, though the first we do not fully consider a deviation. First, the GDA wanted to be involved in monitoring the pilot project. However, they had no allocated budget to pay for it, and PADEE was running over-budget in either case. We attempted to help MAFF and the GDA fundraise to help support our pilot, but the funding fell through. So, the GDA was not able to monitor the activities; however, their monitoring would have just added cost to an activity that was already somewhat costly per beneficiary, as we will discuss in the cost effectiveness section. In other words, the implementation of incentives here becomes more important from a monitoring perspective. The second deviation was that a few villages did not plant rice during the late season, but rather during the initial season in the following year. We had to adjust procedures for those few villages and conducted tests later for farmers (and ensured later payments for farmers).

6.2 VOTO Mobile Messaging in ASPIRE

As discussed above, in ASPIRE we tested sending direct, basic messages to farmers. Messages were developed by IFPRI and MAFF's extension unit, who used the Grameen Intel software as a guide for development for the rice crop, and extension manuals for long beans, cucumbers, and chickens as a guide for the messages for vegetables and chicken rearing, respectively. Initial drafts of messages were written by an IFPRI consultant, and then there was substantial dialogue between IFPRI and MAFF to get the content and the length of each message right. It was decided that messages should be between one and two minutes long, so they did not lose the subject's attention. Initial drafts of messages were in English but were quickly translated into Khmer.

Messages were recorded professionally by VOTO Mobile in a neutral accent. Initial messages were to be delivered between 6 and 7 AM: if no one answered then three more attempts were made during the day by the automated system. Phone numbers were collected as part of the baseline survey, so households without phones or who changed numbers were necessarily excluded from the sample.[10]

To determine which messages to send, the following procedure was followed. Soon after the baseline survey was conducted and phone numbers were compiled, a text message was sent to

[10] We were concerned about this problem early in the planning for this pilot, as anecdotally farmers often change numbers to obtain better deals or lower rates. To deal with the problem, enumerators were instructed to call numbers and then the initial offers or text messages were sent out soon after the baseline survey was complete.

all phone numbers that were collected within the treatment groups; these included phone numbers from the farmer groups (10 times 54, or 540 maximum); phone numbers from farmers outside the farmer groups (14 times 54, or 756 maximum); and phone numbers from the remainder of the list of 50 phone numbers collected for the enhanced plus group (36 times 18, or 648 total).[11] So a total of 1944 phone numbers were potentially collected for the phone messages. Of those, we found that 1141 of them were functional by the second rice call, or 59 percent.[12]

We followed the following procedure to determine the type of messages delivered to farmers. First, we sent text messages asking farmers to sign up for the type of messages that they wanted. Second, we checked the list of topics that farmer groups had requested following, and we then ensured that those messages were sent to farmer group and non-farmer group members from each village. So, for example, if a village had received extension on rice and chickens, all farmers who did not specifically request types of messages through the text message received rice and chicken messages. Messages were then sent on the following schedule to basic and enhanced message groups, respectively:

Table 7. Phone call schedule, by crop/chickens and type of group

	September				*October*				*November*				*December*	
	W1	W2	W3	W4	W1	W2	W3	W4	W1	W2	W3	W4	W1	W2
Chicken														
Enhanced Calls	X		X	X	X	X	X	X	X					
Basic Calls	X		X	X	X									
Rice														
Enhanced Calls	X	X	X	X	X	X	X	X		X				X
Basic Calls	X	X	X											
Cucumber														
Enhanced Calls							X	X	X	X	X	X	X	X
Basic Calls							X	X	X					
Long Beans														
Enhanced Calls							X	X	X	X	X	X	X	X
Basic Calls							X	X	X					

Note: W stands for "week", and phone calls were made in weeks denoted by "X".

Calls were monitored automatically by VOTO mobile; their system listed whether each call was successfully answered; whether each call was completed, and whether calls were simply not answered or not were all recorded. Although there was a plan to reduce lists after week 3, so many calls were successfully completed, we continued to make calls to all working numbers instead of reducing the number of calls slightly.

[11] The 14 non-farmer group members were always a subset of the 50 farmers with phone numbers collected from the village head within the 18 enhanced plus villages.

[12] We made calls to a total of 1108 farmers for rice, 602 farmers for vegetables, and 500 farmers for chicken. The initial rice call went to 1197 numbers but we were able to ascertain that 89 of those numbers were no longer being used.

6.3 Monitoring Results, ePADEE

Here, we focus on the monitoring results for the ICT Plus group, as data from Grameen Intel broadly suggest that the intervention was generally successful in signing up farmers; 620 of the 800 possible farmers signed up according to monitoring data. We also obtained access to PADEE's monitoring information system (MIS), which was well designed, and we were able to match about 65 percent of households in our sample to the MIS. Whereas there were supposed to be some useful variables in the MIS, including an approximate land area for each household, a subjective poverty measure, and a literacy measure, we wanted to use those variables to either corroborate or refute information in our sample. However, the most interesting finding is that the information that is supposed to be in the MIS (rice production and land area, in particular) is not there for most farmers- in fact, we found zeroes. Clearly, the MIS is not useful for monitoring the project's progress if data are not being entered.

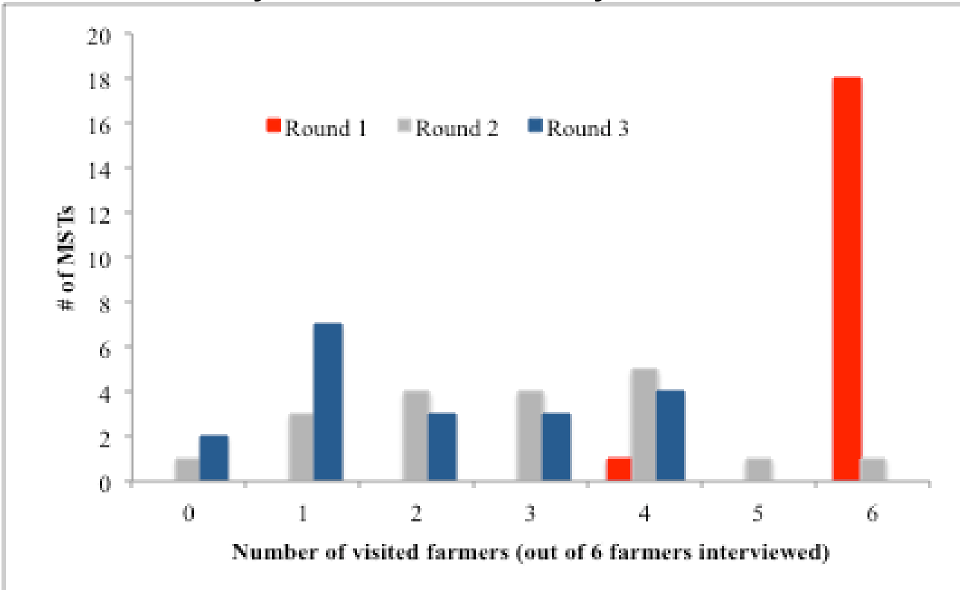
Within the ICT-Plus group, we collected short surveys among farmers in each of the villages where the 19 MSTs in the ICT-Plus group worked.[13] We randomly sampled six farmers in each of these villages and performed bimonthly phone interviews (N=114). Farmers were interviewed about their MST's frequency of visits, the advice they had received, and the new practices / techniques they had learned from the program. Based on the responses of farmers in each village, we can assess each MST's performance and determine their bonus payments.

We collected phone interviews in August, October, and December 2016. In each of these surveys we asked participant farmers whether their assigned MST had visited them and the number of visits they received. Each survey also asked farmers about the particular types of advice they should have received from the MSTs based on the timing of the project. In each survey, first we determine whether MSTs had visited farmers. We interviewed six farmers in each of the villages assigned to the MSTs. Out of these six farmers, at least four (in the first phone survey) or three (in the second and third phone surveys) of them should have received at least one visit for the MST to be eligible to receive bonus payments. Those who did *not* meet this threshold did not receive any bonus payments. Among the MSTs who met the threshold, their payments were determined by farmers' answers to seven questions related to the practices the MST is supposed to explain. As previously explained, these questions varied between surveys based on farmers' cropping cycle and the stage of the project.

MSTs should have visited farmers in the villages where they were assigned to work to be eligible for any bonus payments. Figure 9 shows that during the initial months of the interventions, MSTs had a high rate of visits to farmers in their areas. In 18 (out of 19) of the villages covered by the ICT-Plus intervention, the MSTs visited (at least once) all six farmers selected to participate in our first phone survey. However, there is a considerable reduction in the number of farmers that reported any subsequent visits by the MSTs: only 39% and 21% of the MSTs paid any visits to at least four farmers (out of six) in their villages in the second and third rounds of the phone survey, respectively.

[13] One MST worked with two farmer groups.

Figure 9. Distribution of Visits by Round of Phone Survey

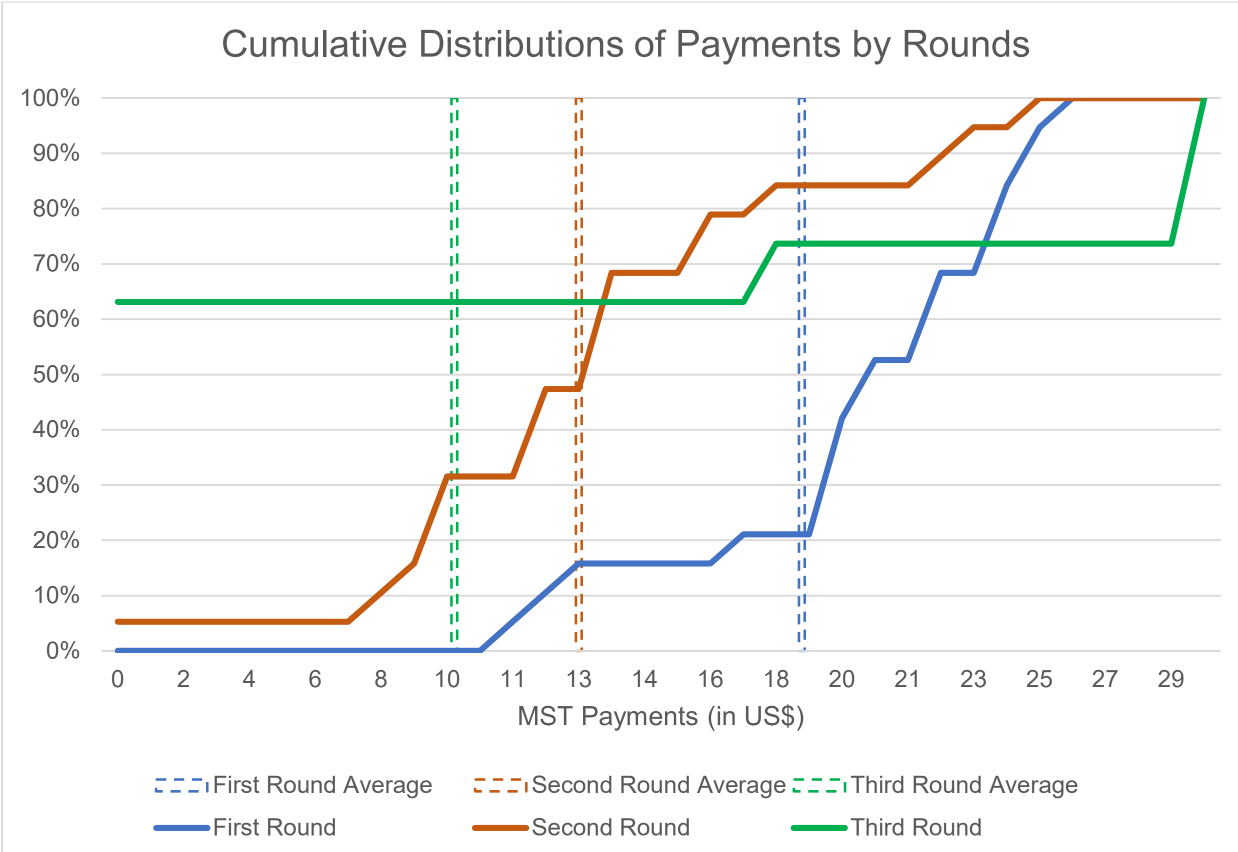


The reduction in the number of visits is likely to be related to the contents of the training. During the first few months, MSTs were mostly focused on collecting and testing soil samples among farmers. The results of those tests determine the plots' levels of nitrogen, phosphorus, potassium, pH, and organic carbon. With this input, the MSTs provided farmers with advice on fertilizer application and crop pests / diseases in later stages of the program.

Arguably, during the first months, MSTs' duties were mostly based on tangible and salient actions (i.e., soil sampling) that were required for later stages of the intervention. This might have encouraged visits to a larger number of farmers. In the following stages, MSTs were supposed to provide farmers with advice regarding fertilizer application and pest control. The reduction in the number of farmers they visited might be related to the somewhat less concrete actions in these stages. The reduction in the number of visits might also be related to the initial novelty of the new technology being introduced and the bonus payments, which might have waned over time.

Only MSTs that visited a minimum number of farmers were eligible to receive any bonus payments. For the first round of phone surveys, only MSTs that visited at least four (out of six farmers in their villages that participated in our survey) farmers were eligible to receive any bonus payments. Due to the reduction in the number of visits, this threshold was reduced for the second and third rounds: MSTs would be eligible for bonus payments if at least three of the farmers from their villages that participated in the survey reported any visits. The overall level of bonuses for all MSTs were lower than they would have been had they been receiving the full per diem, suggesting that the bonus was not large enough to induce the expected behaviour (Figure 10).

Figure 10. Bonus Payments in PADEE, by Round



In sum, we find that the MSTs in the ICT Plus group appeared to start the project with vigour, but then they did not follow through to continue to visit the communities at the same rates. There are two potential implications. First, it could be that the bonuses were not large enough to catalyse action. If so, then MSTs simply did not find the incentive large enough to continue to do this part of their job without any further monitoring. One concern that some MSTs expressed in interviewing them was that we asked them to work with "too many" farmers; they may have simply perceived the work as too difficult. Second, it could be that the monitoring and/or assistance that they normally expect from the GDA is truly essential. If so, then the grant we could not obtain would have been essential to make the ePADEE program "work;" however, as we discuss in the cost analysis sub-section, this type of extension was already quite costly on a per beneficiary basis.

6.4 VOTO Mobile Monitoring Results

To carry out the phone calls, the following schedule was developed (as shown above). The basic calls group all received three weekly messages on rice in September. The enhanced calls groups received the basic calls, and they received reminder calls throughout October and are receiving one call in November and December, respectively. For chicken rearing, the basic calls group received four calls in September and October; the enhanced calls groups received reminders starting in October, two messages in November and one in December. Cucumber and long bean messages began in the second half of October; the basic messages group received three messages in October and the first week of November, and reminders took place through December for the enhanced messages groups.

In general, the calls were quite successful. We find that for chicken messages, between 50 and 70 percent of farmers tend to listen to the message, whereas for rice, typically between 55 to 70 percent of farmers listen to the entire message (Figures 11 and 12). These numbers are even higher when we only consider farmers who requested a specific type of messages; we find that around two-thirds of farmers listen to rice messages, and up to 78 percent of farmers listened to chicken messages. Notably, the proportion of farmers who are listening to messages does not appear to decrease over time, as we would expect if farmers did not like receiving them. Even messages that were provided a few weeks after the main set of messages, which occurred in November and December, had quite high listening rates.

Figure 11. Percent of farmers listening to full rice message, by week

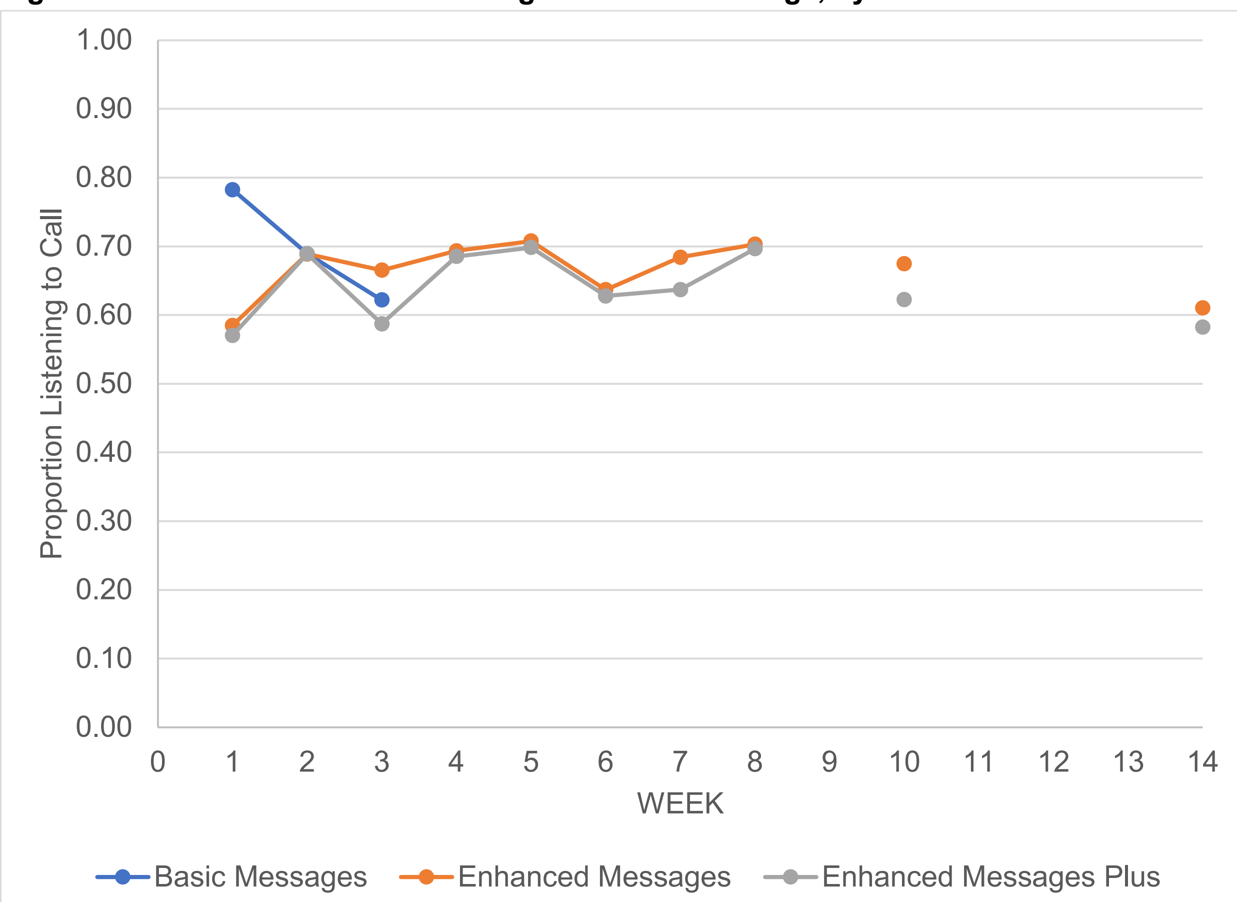
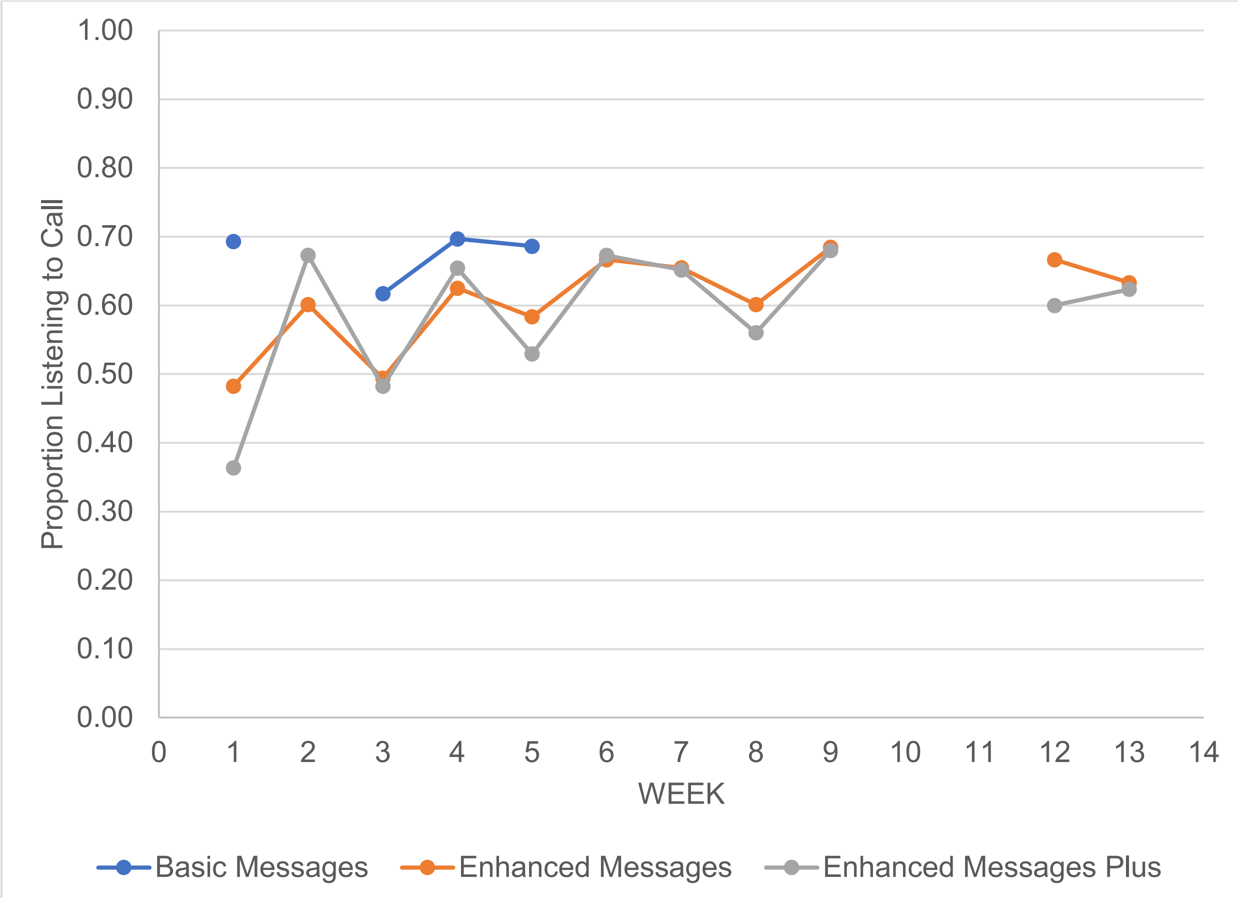


Figure 12. Proportion of farmers listening to full chicken message, by week



Calls related to long beans and cucumbers began in the third week of October, to coincide with the timing of cultivation (Figures 13 and 14). Estimated rates of listening to calls are typically over 60% for both crops and are close to 80% in some cases. Further, they do not decline over time, suggesting again that the basic messages are useful to farmers. In the future, we can fill in information on the proportion of farmers who had grown long beans or cucumbers in the past.

Figure 13. Proportion of farmers listening to full cucumber message, by week, beginning in the 3rd week of October

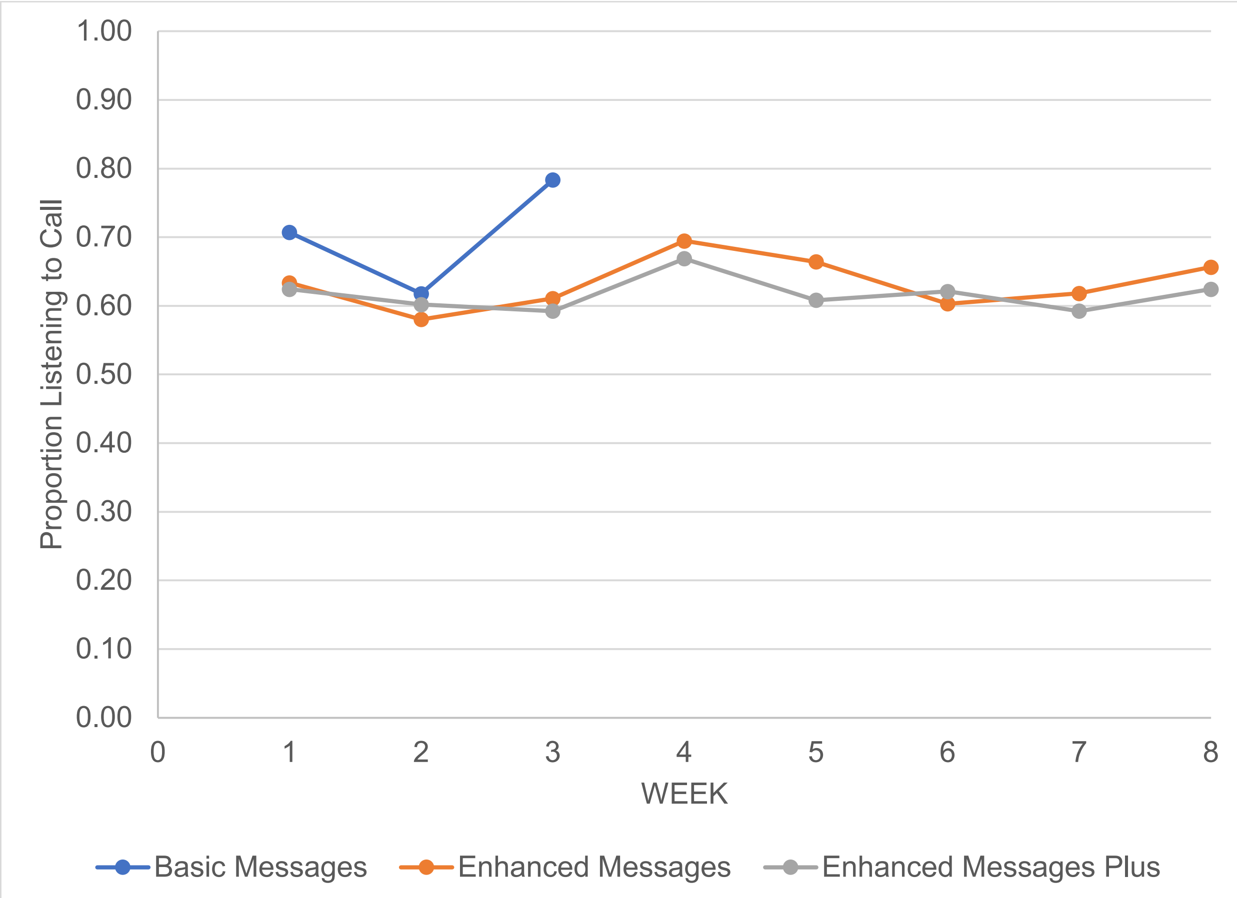
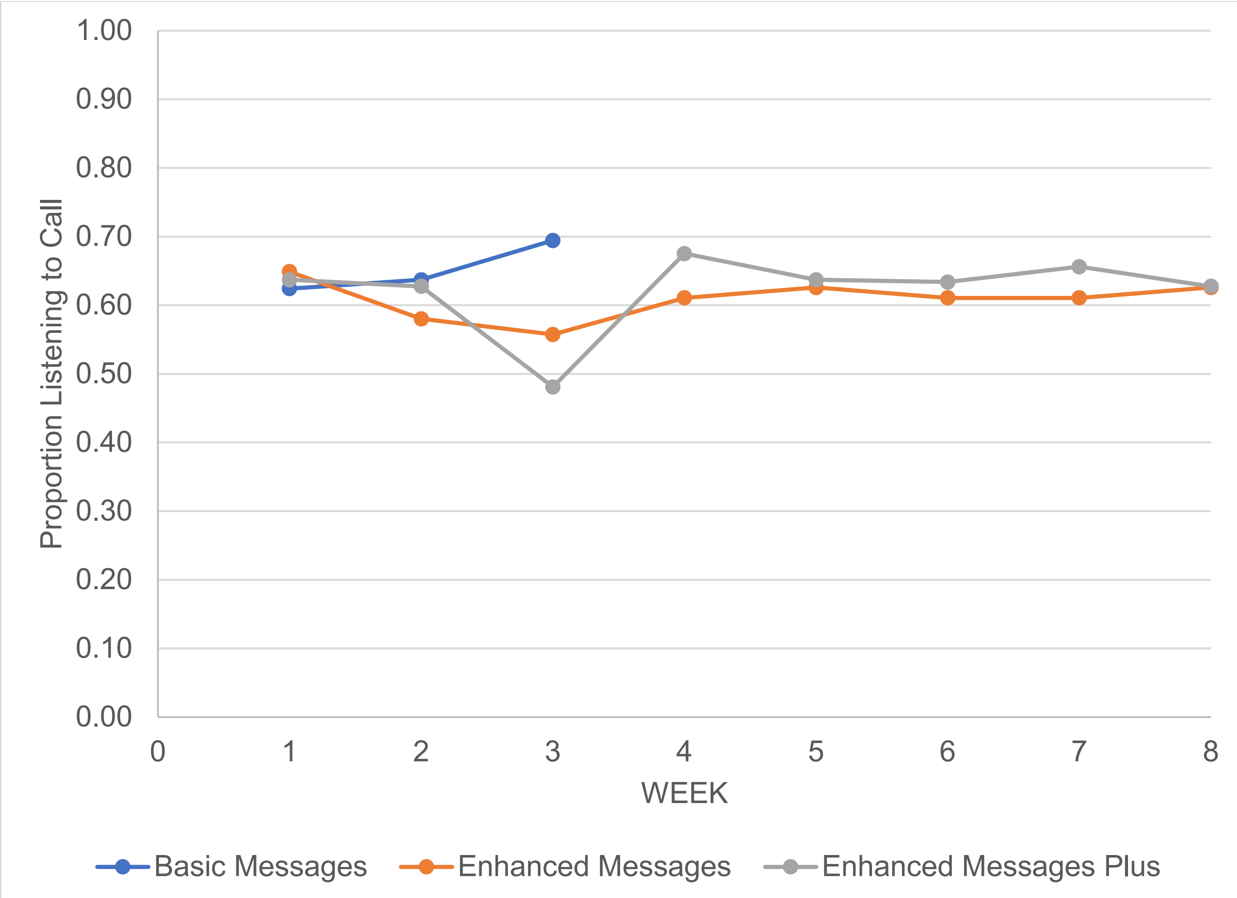


Figure 14. Proportion of farmers listening to full long beans message, by week, beginning in the 3rd week of October



If anything, spot check calls to participants suggested that they wanted either more calls or the ability to repeat calls. The spot checks took place in October, and November, and gave us confidence that the calls had been effective at reaching farmers. The main complaint from farmers was that there was no way to repeat messages.

In sum, the monitoring data suggest that the calls were quite popular and effective in reaching at least a portion of farmers; we note that farmers who did change numbers were not able to receive calls. Since VOTO (now Viamo) mobile is currently working on a call-in service, a main question in our minds is whether direct calling or on-demand phone trees are more effective at providing advice to farmers at this time.

7 Impact Analysis and Main Results

In this section, we provide our main analysis, following the pre-analysis plan that appears in Appendix B. We follow the same format as the report to this point, sequentially discussing the PADEE pilot project and then the ASPIRE pilot project.

7.1 PADEE Analysis Plan

In analyzing results for the ePADEE pilot, we want to compare the ICT (T^1) and ICT Plus (T^2) treatments with a control group. With the presence of baseline data, our main specification is an ANCOVA:

$$Y_{iv1} = \alpha + \beta_1 T_v^1 + \beta_2 T_v^2 + \gamma Y_{iv0} + \boldsymbol{\tau X_{iv0}} + \epsilon_{iv} \qquad (1)$$

where i indexes households, v indexes villages, 1 denotes variables that are collected at endline, and 0 variables collected at baseline. The variables included in the main estimating equation include Y (an outcome), a vector **X** (potentially important explanatory variables, collected at baseline), and an error term assumed to be clustered at the village or farmer group level.

The first set of variables that we use test for whether the two treatments were basically effective or not. We ask farmers whether they know the MST's name or phone number; the frequency of advice received over the past 12 months, whether the MST who visited had a tablet or not, and whether subjectively farmers thought that the tablet helped the MST give better advice.

Next, we examine the basic relationship between the two treatments and farmer knowledge about messages they should have learned related to rice production. Given that we focus on rice production, farmers' knowledge variables that are included in analysis are the ideal number of times to apply fertilizer during the wet season; the number of days to wait for the first fertilizer application after rice seeds are sown, the number of rice diseases the respondent claims they can identify out of 10; and the number of diseases they correctly identify out of 10. Finally, we look at standardized effects across the 10 diseases using the Kling et al (2007) normalization procedure for classes of outcomes.

The following set of regressions relate to the production process. We measure whether or not farmers realize that plots have any types of deficiencies, which should have been identified as part of the ePADEE procedure. We then test whether improved inputs were used in production, and examine potential impacts on rice yields and overall agricultural income.

7.2 ASPIRE Analysis Plan

In analyzing results for the ASPIRE pilot, we want to compare the basic calls (T^1), the enhanced calls (T^2), the enhanced calls plus (T^3), with a "business as usual" control group. Recall that an important difference between the PADEE and ASPIRE pilots is that we were able to stratify at the CEW level in the ASPIRE sample. So the basic ANCOVA equation we estimate is:

$$Y_{ivc1} = \alpha_c + \beta_1 T_{vc}^1 + \beta_2 T_{vc}^2 + \beta_3 T_{vc}^3 + \gamma Y_{ivc0} + \boldsymbol{\tau X_{ivc0}} + \epsilon_{ivc} \qquad (2)$$

where i indexes households, v indexes villages, c indexes CEWs, 1 denotes variables that are collected at endline, and 0 variables collected at baseline. The variables included in the main estimating equation, in addition to the treatment variables, include Y (an outcome), a vector **X** (potentially important explanatory variables, collected at baseline), and an error term assumed to be clustered at the village or farmer group level.

We estimate an alternative version of equation (2) for selected outcomes to test for differences between the farmer group and non-farmer group members. We use the non-farmer group members of the control group as a base, since they should have the least contact with ASPIRE, and then we estimate:

$$Y_{ivc1} = \alpha_c + \beta_1 T_{vc}^1 G_{ivc} + \beta_2 T_{vc}^1 NG_{ivc} + \beta_3 T_{vc}^2 G_{ivc} + \beta_4 T_{vc}^2 NG_{ivc} + \beta_5 T_{vc}^3 G_{ivc} + \beta_6 T_{vc}^3 NG_{ivc} + \beta_7 C_{vc} G_{ivc} + \gamma Y_{ivc0} + \boldsymbol{\tau X_{ivc0}} + \epsilon_{ivc} \quad (3)$$

where G represents farmer group membership, NG represents non-farmer group members, and C represents the control group. All regressions at this point are reported as intent-to-treat effects, though some are presented as conditional on the existence of a reported value; those variables will be pointed out in the discussion.

We use both equations (2) and (3) to estimate impacts on variables measuring knowledge of the program, the presence of social information flows within the village, and experience with ASPIRE and the CEWs, as well as basic knowledge about rice, chicken rearing, and cucumber and long bean production. We then use equation (2) to estimate impacts on different types of input use.

Finally, we use equation (2) to estimate impacts on the production process. We measure whether improved inputs were used in production, expenditures on inputs, and then we examine potential impacts on rice yields and overall agricultural income.

7.3 Balance Tables

7.3.1 PADEE Sample Balance

We first present baseline values of all variables that appear in the analysis by treatment status (Table 8).[14] Prior to the intervention, only 35 percent of control group households knew the MST, but those who did know the MST appeared to get advice often, as the average across the whole sample was 2.5 times per farmer over 12 months. Still these differences are not statistically significant across groups.

However, we do find differences in fertilizer use across groups. The ICT group appears to be more likely to have used NPK in rice cultivation; however, use rates in general are high, so the difference is not terribly meaningful. That said, the ICT and ICT Plus groups both have more experience using pesticides in rice cultivation in the past 12 months than the control group, which may mean they are less likely to increase pesticide use further.

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[14] No imputation has been completed for missing variables. However, we have used prevailing local prices to value agricultural products that were not sold.

Quantities of both NPK and urea reported used by the ICT group are also higher than the other two groups. However, all of the averages appear to be relatively high, considering 100 kg/ha as a benchmark. Given the higher fertilizer application rates, it is not surprising that the ICT group has higher expenditures than the other two groups; however, yields are no different between the three groups.

As a result of the baseline differences between the ICT group and the other two groups, we may want to consider implications of these differences after presenting results. We return to these differences in section 8.

Table 8. Group averages at baseline by treatment status and tests for equality, ePADEE pilot

	(1) Control Mean [SE]	(2) ICT Mean [SE]	(3) ICT plus Mean [SE]	(4) (1)-(2) p-value	(5) (1)-(3) p-value	(6) (2)-(3) p-value	(7) No. of obs.
HH knows MST member	0.351 [0.025]	0.385 [0.025]	0.387 [0.025]	0.331	0.308	0.958	1127
Frequency HH received MST advice in last 12 months	2.479 [0.227]	2.583 [0.234]	2.621 [0.254]	0.749	0.676	0.913	1127
Used NPK in rice cultivation	0.947 [0.012]	0.985 [0.007]	0.955 [0.011]	0.008***	0.630	0.026**	1005
Used urea in rice cultivation	0.859 [0.019]	0.900 [0.017]	0.896 [0.017]	0.109	0.151	0.859	1005
Used compost in rice cultivation	0.783 [0.022]	0.790 [0.022]	0.791 [0.022]	0.818	0.799	0.981	1005
Used pesticides in rice cultivation	0.607 [0.026]	0.693 [0.025]	0.687 [0.025]	0.020**	0.031**	0.858	1005
Quantity of NPK applied per ha of land (Kg/ha)	144.905 [5.663]	173.071 [7.419]	139.594 [5.147]	0.003***	0.488	0.000***	1005
Quantity of urea applied per ha of land (Kg/ha)	101.774 [4.468]	114.953 [5.460]	100.163 [4.668]	0.061*	0.803	0.040**	1005
Quantity of compost applied per ha of land (Kg/ha)	1066.39 [80.495]	973.86 [93.347]	1018.41 [97.977]	0.452	0.705	0.742	1005
Value of NPK applied per ha of land (Kg/ha)	34.62 [1.330]	41.18 [1.782]	34.30 [1.274]	0.003***	0.860	0.002***	1005
Value of urea applied per ha of land (Kg/ha)	18.38 [0.817]	20.81 [0.957]	18.31 [0.819]	0.053*	0.950	0.047**	1005
Value of compost applied per ha of land (Kg/ha)	0.547 [0.227]	2.928 [1.163]	1.425 [0.528]	0.042**	0.125	0.237	1005
Expenditure on agricultural input & labor (0000 Riels/ha)	133.76 [3.805]	145.53 [5.012]	132.56 [4.120]	0.061*	0.830	0.046**	1005
Total rice harvest (kg) per ha of land	3027.48 [73.712]	2919.08 [86.213]	2946.74 [86.871]	0.338	0.478	0.821	1005

Note: ***, **, and * indicate significance at the 1, 5, and 10 percent critical level. Standard errors of means are reported in brackets.

7.3.2 ASPIRE Sample Balance

We next examine balance among the ASPIRE sample. To do so, we generally estimate the following equation using the baseline sample:

$$Y_{ivc1} = \alpha_c + \beta_1 T_{vc}^1 + \beta_2 T_{vc}^2 + \beta_3 T_{vc}^3 + \epsilon_{ivc} \quad (4)$$

So we are looking for rejections of the null hypothesis that $\beta_j = 0$, for j=1,2, and 3.

First, we examine variables that measure awareness of ASPIRE (Table 9). We find that the awareness of ASPIRE is not high, but that might not be surprising as only 42 percent of the sample should be in ASPIRE farmer groups. Only 32 percent had been visited by a CEW and very few knew the phone number of the CEW; there are no differences in means between groups.

Table 9. Balance Table, Indicators for Crop Production, ASPIRE Sample, 2017

	Basic Messages	Enhanced Messages	Enhanced Messages Plus	Control Group Mean
Knows about ASPIRE program	-0.002	0	0.005	0.36
	(0.035)	(0.033)	(0.030)	
Has been visited by Community Extension Worker	-0.009	-0.012	-0.021	0.32
	(0.029)	(0.029)	(0.029)	
Knows CEW phone number	-0.046	0.035	0.042	0.02
	(0.049)	(0.074)	(0.074)	
Has ever called CEW	0.002	-0.002	-0.005	0.02
	(0.010)	(0.008)	(0.008)	

Note: 1728 observations in all regressions. Standard errors in parentheses.

In Table 10, we examine variables measuring which crops were cultivated by households. We find differences significant at the 10 percent level between the basic messages group and the control, for rice production and rice cultivation in the wet season. Coefficients are generally negative for the treatment groups on vegetable and all other crop production, respectively, but they are not significantly different from zero. Somewhat surprisingly, a relatively large share (20 percent) of the sample do not cultivate rice at all.

Table 10. Balance Table, Indicators for Crop Production, ASPIRE Sample, 2017

	Basic Messages	Enhanced Messages	Enhanced Messages Plus	Control Group Mean
HH cultivated rice	0.072	0.056	0.062	0.8
	(0.039)*	(0.048)	(0.043)	
HH cultivated rice in wet season	0.076	0.051	0.072	0.78
	(0.044)*	(0.049)	(0.044)	
HH cultivated rice in dry season	-0.046	0.035	0.042	0.1
	(0.049)	(0.074)	(0.074)	
HH cultivated vegetables	-0.032	-0.049	-0.042	0.49
	(0.046)	(0.052)	(0.059)	
HH produces other crops	-0.056	-0.072	-0.044	0.68
	(0.080)	(0.076)	(0.084)	

Note: 1728 observations in all regressions. Standard errors in parentheses.

Next, we examine conditional means for variables that relate to crop production (Table 11).[15] Whereas coefficients may appear large, differences are not significant in most cases, because of wide distributions on variables. Moreover, most of the coefficients are within a reasonably small percentage of the control group mean. Among these variables, an unbalanced sample does not appear to be much of a concern.

Table 11. Balance Table, Indicators for Crop Production, ASPIRE Sample, 2017

	Basic Messages	Enhanced Messages	Enhanced Messages Plus	Control Group Mean (Std. Dev.)
Total rice harvest (kg), wet season	1031.8	4202.6	2401.6	9529
	(3146.8)	(3938.3)	(3166.9)	(13141)
Total rice harvest (kg)	637.3	4570.8	2682.1	10156
	(3131.4)	(3898.5)	(3103.1)	(13782)
Total rice production value (0000 Riels)	171.4	1290.6	753.9	2894.1
	(888.4)	(1106.4)	(880.3)	(3918.5)
Number of vegetables HH grows	0.047	0.008	0.2	2.45
	(0.16)	(0.17)	(0.025)	(1.5)
Vegetable Production Value (0000 Riels)	34.9	-10.2	105.2	55.2
	(33.6)	(19.3)	(103.1)	(135.2)
Other Crops Production Value (0000 Riels)	117.3	42.3	8.89	112.4
	(91.6)	(67.8)	(39.9)	(338.6)

Notes: All means are conditional on growing the crop, so sample sizes are 1462 observations in rows 1-3, 799 observations in row 4-5, and 1098 observations in row 6. Standard errors in parentheses in columns 1-3 and standard deviation of the mean in column 4. Crop values adjusted for consumer price index.

Finally, we provide information on rice yields and total agricultural income and expenditures on rice (Table 12). Some of the larger differences, in terms of magnitude, found in Table 11 appear

[15] For brevity, we report rice as either wet season or total rice production, omitting dry season production; only 10 percent of households grew dry season rice at baseline.

due to differences in land holdings, as rice yield differences from the control group mean are all within 10 percent, and are not significantly different from zero. Similarly, the value of production per hectare of rice and total income are not statistically different from the control group. As with the previous table, none of the differences are statistically different from zero.

Table 12. Balance Table, Rice Yields and Agricultural Income, ASPIRE Sample, 2017

	Basic Messages	Enhanced Messages	Enhanced Messages Plus	Control Group Mean (Std. Dev.)
Rice yield (Kg/ha)	-112.3	232.5	176.3	2590
	(222.2)	(252.8)	(228.3)	(1089)
Rice yield (Kg/ha), wet season	-109.7	217.7	194.2	2580
	(225.5)	(248.7)	(238.1)	(1100)
Value of production per hectare of rice	-9.3	16.6	13.2	208.4
	(18.7)	(20.7)	(18.9)	(90.6)
Value of production per hectare of rice, wet season	-10.1	16.2	14.2	208.6
	(19.1)	(21.0)	(19.9)	(92.5)
Total HH agricultural income (0000 riels)	444.2	1279.2	884.1	2461.3
	(696.1)	(900.4)	(717.6)	(3693.9)
Expenditure on agricultural inputs (0000 Riels) for rice	33.4	123.6	65.9	376.3
	(122.3)	(134.5)	(111.7)	(499.3)

Notes: All means are conditional on growing the crop, so sample sizes are 1462 observations in rows 1-4, 1680 observations in row 5, and 1728 observations in row 6. Standard errors in parentheses in columns 1-3 and standard deviation of the mean in column 4.

In sum, balance tables based on outcomes of interest imply that the sample balance in the ASPIRE pilot is reasonably good, particularly in comparison to the findings from the ePADEE sample.

7.4 PADEE Impact Analysis

In this sub-section, we initially present quantitative analysis that closely follows the pre-analysis plan to estimate impacts of the ICT and ICT Plus treatments. The quantitative analysis is followed by some of the qualitative insights, and then we briefly summarize the findings.

7.4.1 Quantitative Analysis

First, we examine outcomes related to knowledge of the program (Table 13).[16] We find that the ICT and ICT Plus groups are more likely than the control group to report that either the respondent knows the MST or that the MST they know visited the village in the past twelve months. However, the magnitude of the coefficients in each case is perhaps lower than one would have expected. The former coefficients suggest the households are only 8 percentage points more likely to know the MST's name and phone number than the control group. Whereas they are between 27 and 29 percent more likely to report that the MST visited the village in the past 12 months. Farmers in the treatment groups are also much more likely to report having received advice from the MST in the past 12 months, at between 0.72 and 0.81 times, on average. However, each of these

[16] At present, all regressions only include regional fixed effects and do not include baseline control variables.

suggests 1.5 visits relative to the control group on average, which is far below the 5 visits that were supposed to occur.

Table 13. Outcomes related to MST and tablet based advice, ePADEE Intervention

	(1) HH know MST member (name or phone number)	(2) MST member visited in the past 12 months	(3) Frequency HH received advice from MST, last 12 months	(4) MST who visited had tablet	(5) MST's tablet helped the MST give better advise
Treatment arm: ICT	0.086	0.281	0.736	0.133	0.123
	(0.036)**	(0.057)***	(0.169)***	(0.041)***	(0.037)***
Treatment arm: ICT plus	0.080	0.306	0.820	0.188	0.131
	(0.031)**	(0.062)***	(0.223)***	(0.039)***	(0.034)***
Province dummies	Yes	Yes	Yes	Yes	Yes
Baseline controls	Yes	Yes	Yes	Yes	Yes
Control for outcome at baseline	Yes		Yes		
Observations	1,127	1,127	1,127	1,127	1,038
R-squared	0.054	0.113	0.069	0.069	0.065
Mean in Control Group	0.0851	0.287	0.689	0.165	0.120
p-value: ICT = ICT plus	0.883	0.705	0.729	0.249	0.859

Note: Standard errors are clustered at the village level. ***, **, and * indicate significance at 1, 5 and 10 percent respectively. Controls include household head's age, gender, dummies for education level, asset index, and household non-agricultural income at baseline.

As these outcomes are clearly related, it is worth further conducting multiple hypothesis testing on the set of outcomes in Table 13. To do so, we use the resampling method of Westfall and Young (1993) as coded in Stata by Jones et al. (2018), with and without the set of baseline control variables, bootstrapping standard errors with 1000 replications. We find that all ten outcomes have corrected p-values of 0.023 or lower, lending confidence that the estimates are statistically significant (Table 14).

Table 14. p-values Corrected for Multiple Hypothesis Testing, Outcomes Related to MST and tablet based advice, ePADEE Intervention

	(1) HH knows MST member (name or phone number)	(2) MST member visited in the past 12 months	(3) Frequency HH received advice from MST, last 12 months	(4) MST who visited had tablet	(5) MST's tablet helped the MST give better advice
Without Baseline Controls					
ICT	0.023	<0.001	0.001	0.005	0.008
ICT Plus	0.018	<0.001	0.001	0.001	0.002
With Baseline Controls					
ICT	0.021	<0.001	0.001	0.009	0.009
ICT Plus	0.012	<0.001	0.001	0.001	0.002

MSTs visiting the treatment groups were much more likely to have the tablet with them when they visited, and farmers were more likely to report that MSTs visiting had tablets and that they helped them give better advice. The two treatment groups reported that the MSTs were between 12 and 13 percentage points more likely to give better advice with the tablets; adding to the control group mean, these figures imply about 25 percent of treated farmers responded to this question in the affirmative (columns 4 and 5).

We next explore impacts on whether households received specific types of extension messages that should have been expected (Table 15). The ten variables include disease prevention and treatment, improved rice varieties, seed recommendations, chemical fertilizer applications, composting and compost application (separately), water management, pest prevention, access to credit, and soil nutrient deficiencies. We report these results only among rice producers. For both groups, we find all the estimated coefficients are positive but vary substantially in magnitude. The largest coefficients are on the soil nutrient deficiencies indicator; they suggest that 38 and 40 percentage points more rice farmers in the ICT and ICT plus groups, respectively, received extension on soil nutrient deficiencies than the control group, which only received such messages 8 percent of the time. Meanwhile, the least common message received over the control group related to water management; we find only 2.5 and 1 percentage point more farmers in the ICT and ICT plus groups received such messages, over 8 percent of the control group.

Table 15. Impacts on varieties of extension services received, ePADEE Intervention

	(1)	(2)	(3)	(4)	(5)
	Disease prevention/ treatment	Improved rice varieties	Seed recommend-ations	Chemical fertilizer application	Composting/ organic residue management
Panel A					
Treatment: ICT	0.108	0.054	0.064	0.148	0.082
	(0.048)**	(0.043)	(0.056)	(0.036)***	(0.036)**
Treatment: ICT plus	0.037	0.020	0.021	0.127	0.070
	(0.038)	(0.040)	(0.045)	(0.039)***	(0.044)
Province dummies	Yes	Yes	Yes	Yes	Yes
Baseline controls	Yes	Yes	Yes	Yes	Yes
Control for outcome at baseline	Yes	Yes	Yes	Yes	Yes
Observations	977	977	977	977	977
R-squared	0.026	0.041	0.033	0.047	0.060
Mean in Control	0.151	0.204	0.237	0.195	0.142
p-value, ICT = ICT plus	0.154	0.438	0.426	0.590	0.761
	Water/ irrigation manage-ment	Pest prevention / treatment	Access to credit	Soil nutrient deficiencies	Compost application
Panel B					
Treatment: ICT	0.025	0.095	0.140	0.388	0.018
	(0.025)	(0.041)**	(0.032)***	(0.057)***	(0.025)
Treatment: ICT plus	0.010	0.063	0.114	0.407	0.027
	(0.024)	(0.035)*	(0.031)***	(0.047)***	(0.033)
Province dummies	Yes	Yes	Yes	Yes	Yes
Baseline controls	Yes	Yes	Yes	Yes	Yes
Control for outcome at baseline	Yes	Yes	Yes	Yes	Yes
Observations	977	977	977	1,000	1,000
R-squared	0.042	0.033	0.082	0.208	0.039
Mean in Control	0.0828	0.0917	0.257	0.0740	0.112
p-value, ICT = ICT plus	0.506	0.488	0.500	0.759	0.777

Note: Standard errors are clustered at the village level. ***, **, and * indicate significance at 1, 5 and 10 percent respectively. Controls include household head's age, gender, dummies for education level, asset index, and household non-agricultural income at baseline.

As we again are estimating a number of hypotheses simultaneously, we present p-values corrected for multiple hypothesis testing for Table 15 in Table 16. The p-values suggest strong significance of the chemical fertilizer and soil nutrient messages for both treatment groups. These results are both consistent with the monitoring data, which showed that the first visits definitely occurred and used the tablet based extension more often than not, at least in the ICT plus group. Somewhat surprisingly, the ICT group also received more extension on access to credit (the ICT plus result is significant as well at the 10 percent level). This result is surprising only because it would have been a secondary outcome.

Table 16. p-values Corrected for Multiple Hypothesis Testing, Varieties of extension services received, ePADEE Intervention

	(1)	(2)	(3)	(4)	(5)
	Disease prevention / treatment	Improved rice varieties	Seed recomm-endations	Chemical fertilizer application	Composting /organic residue manage-ment
Panel A					
Without Baseline Controls					
ICT	0.070	0.383	0.383	0.013	0.181
ICT plus	0.599	0.883	0.883	0.013	0.250
With Baseline Controls					
ICT	0.065	0.325	0.325	0.01	0.149
ICT plus	0.631	0.842	0.842	0.024	0.228
	Water/ irrigation manage-ment	Pest prevention / treatment	Access to credit	Soil nutrient deficiencie s	Compost application
Panel B					
Without Baseline Controls					
ICT	0.522	0.07	0.017	<0.001	0.536
ICT plus	0.883	0.25	0.068	<0.001	0.722
With Baseline Controls					
ICT	0.494	0.065	0.015	<0.001	0.508
ICT plus	0.842	0.228	0.056	<0.001	0.742

We aggregate the extension variables from Table 15 and Table 16 into two indices. First, we measure whether a rice farmer has received any extension at all, and then the number of extension messages they received in general (Table 17 columns 1 and 2).[17] We then split the sample by whether they report receiving extension from the government (column 3) or from PADEE (column 4), and finally measure the reported extension visits (column 5). We find positive and nominally statistically significant results for all variables; however, we do not find detectable differences between the two treatment groups. The latter finding suggests that the incentives were

[17] We drop the three services that were received by almost no one from estimation in Table 15.

not effective at increasing extension received by the treatment group. We further note that it is not a power issue, as the point estimates for the ICT group are generally higher than for the ICT plus group.

Table 17. Impacts on extension services received (aggregated), ePADEE Intervention

	(1)	(2)	(3)	(4)	(5)
	Received any extension service	Range of extension services received (Range: 0-13)	Received extension service from a government agent	Received extension service through PADEE program	Number of extension visits
Treatment: ICT	0.254	1.199	0.293	0.311	2.674
	(0.042)***	(0.307)***	(0.047)***	(0.048)***	(0.626)***
Treatment: ICT plus	0.247	0.942	0.330	0.340	2.150
	(0.046)***	(0.291)***	(0.047)***	(0.046)***	(0.594)***
Province dummies	Yes	Yes	Yes	Yes	Yes
Baseline controls	Yes	Yes	Yes	Yes	Yes
Observations	1,000	1,000	1,000	1,000	1,000
R-squared	0.092	0.055	0.101	0.108	0.051
Mean in Control	0.518	1.675	0.370	0.328	1.938
p-value, ICT = ICT plus	0.891	0.448	0.479	0.570	0.510

Note: Standard errors are clustered at the village level. ***, **, and * indicate significance at 1, 5 and 10 percent respectively. Controls include household head's age, gender, dummies for education level, asset index, and household non-agricultural income at baseline.

Point estimates suggest that both treatment groups became about 50 percent more likely to receive any extension, as about half the control group reported receiving at least one extension visit. They received about one more extension service than the control group, and the average farmer received an additional two visits, over both treatment groups. As the latter result is not conditional, if we divide by the approximate share of farmers receiving any visits, we note that control farmers receiving visits received roughly 4, whereas treatment farmers received about 5 and a half on average. Correcting for multiple hypothesis testing, the aggregate variables all appear significantly different from zero at any reasonable level of significance (Table 18).

Table 18. p-values Corrected for Multiple Hypothesis Testing, Extension services received (aggregated), ePADEE Intervention

	(1)	(2)	(3)	(4)	(5)
	Received any extension service	Range of extension services received (Range: 0-13)	Received extension service from a governme nt agent	Received extension service through PADEE program	Number of extension visits
Without Baseline Controls					
ICT	<0.001	0.001	<0.001	<0.001	<0.001
ICT plus	<0.001	0.004	<0.001	<0.001	0.003
With Baseline Controls					
ICT	<0.001	<0.001	<0.001	<0.001	<0.001
ICT plus	<0.001	0.003	<0.001	<0.001	<0.001

Recalling receiving extension messages does not necessarily mean that farmers adopt them. Next, we measure whether farmers adopted any of the 10 recommendations listed in Table 15. Not surprisingly, the point estimates in Table 19 are generally smaller than those in Table 15, and they remain positive in all cases. The former result is sensible as farmers might be reticent to apply techniques that they do not fully understand. From the perspective of the e-PADEE intervention, perhaps the most interesting results are whether farmers applied seed or fertilizer recommendations (columns 3 through 5). We find that point estimates show a 5 to 6 percentage point increase in farmers following seed recommendations over the control group, and 11 to 13 percent on chemical fertilizers.[18]

[18] The result regarding improved rice varieties is somewhat subtle. The treatment effect represents the additional farmers who followed recommendations given to them, rather than the farmers who used improved seed in general.

Table 19. Impacts on varieties of extension services adopted, ePADEE Intervention

	(1)	(2)	(3)	(4)	(5)
	Disease prevention/ treatment	Improved rice varieties	Seed recommend -ations	Chemical fertilizer application	Composting/ organic residue management
Panel A					
Treatment: ICT	0.055	0.049	0.059	0.116	0.060
	(0.021)***	(0.024)**	(0.037)	(0.027)***	(0.026)**
Treatment: ICT plus	0.025	0.043	0.049	0.138	0.046
	(0.024)	(0.022)*	(0.025)*	(0.030)***	(0.026)*
Province dummies	Yes	Yes	Yes	Yes	Yes
Baseline controls	Yes	Yes	Yes	Yes	Yes
Control for outcome at baseline	Yes	Yes	Yes	Yes	Yes
Observations	977	977	977	977	977
R-squared	0.029	0.027	0.045	0.048	0.056
Mean in Control	0.0503	0.0503	0.0917	0.0769	0.0592
p-value: ICT = ICT plus	0.225	0.802	0.771	0.528	0.654
	Water/ irrigation manage-ment	Pest prevention/ treatment	Access to credit	Soil nutrient deficiencies	Compost application
Panel B					
Treatment: ICT	0.023	0.032	0.067	0.245	0.029
	(0.017)	(0.019)	(0.033)**	(0.043)***	(0.016)*
Treatment: ICT plus	0.011	0.034	0.076	0.274	0.034
	(0.020)	(0.019)*	(0.034)**	(0.043)***	(0.018)*
Province dummies	Yes	Yes	Yes	Yes	Yes
Baseline controls	Yes	Yes	Yes	Yes	Yes
Control for outcome at baseline	Yes	Yes	Yes		
Observations	977	977	977	1,000	1,000
R-squared	0.035	0.026	0.054	0.136	0.029
Mean in Control	0.0562	0.0355	0.175	0.0355	0.0414
p-value: ICT = ICT plus	0.501	0.944	0.811	0.576	0.779

Note: Standard errors are clustered at the village level. ***, **, and * indicate significance at 1, 5 and 10 percent respectively. Controls include household head's age, gender, dummies for education level, asset index, and household non-agricultural income at baseline.

Checking the latter two results for multiple hypothesis testing, we cannot conclude either coefficient on seed recommendations is different than zero, but the chemical fertilizer variables are both significant from zero at better than the 1 percent level (Table 20). Interestingly, in Table 19 we find that the coefficient for "soil nutrient deficiencies" is largest in magnitude, though it is not entirely clear what farmers would have done other than apply additional compost or chemical fertilizer. The multiple hypothesis test also suggests significance of those coefficients, though not the ones on the credit variables that also suggested statistical significance in Table 19.

Table 20. p-values Corrected for Multiple Hypothesis Testing, Varieties of extension services adopted, ePADEE Intervention

	(1)	(2)	(3)	(4)	(5)
	Disease prevention/ treatment	Improved rice varieties	Seed recommend-ations	Chemical fertilizer application	Composting/ organic residue management
Panel A					
Without Baseline Controls					
ICT	0.13	0.191	0.191	0.009	0.18
ICT plus	0.335	0.222	0.222	<0.001	0.157
With Baseline Controls					
ICT	0.128	0.167	0.167	0.007	0.167
ICT plus	0.328	0.178	0.178	<0.001	0.16
	Water/ irrigation manage-ment	Pest prevention/ treatment	Access to credit	Soil nutrient deficiencies	Compost application
Panel B					
Without Baseline Controls					
ICT	0.209	0.201	0.191	<0.001	0.209
ICT plus	0.637	0.231	0.141	<0.001	0.231
With Baseline Controls					
ICT	0.176	0.176	0.167	<0.001	0.176
ICT plus	0.593	0.216	0.149	<0.001	0.216

Next, we examine whether ePADEE caused farmers in the treatment groups to report giving or receiving more advice about farming to others (Table 21).[19] These questions ask about

[19] Since none of the null hypotheses are rejected in these regressions, we do not conduct multiple hypothesis testing as it will inflate p-values even further and will not change the result of the hypothesis test. We follow this logic throughout the remainder of the report.

whether farmers received or gave advice to other farmers, either within or outside their home village. We find no evidence that the ICT or ICT Plus treatment arms affected these variables.

Table 21. Impacts of ePADEE on receipt or giving of advice about rice production, by treatment status

	(1)	(2)	(3)	(4)
	Received advice about rice production/ marketing	Gave advice about rice production/ marketing	Received advice about rice production/ marketing	Gave advice about rice production/ marketing
	Within village		Outside village	
Treatment arm: ICT	0.015	0.019	0.017	0.010
	(0.034)	(0.038)	(0.015)	(0.012)
Treatment arm: ICT plus	-0.020	-0.011	-0.000	0.000
	(0.028)	(0.034)	(0.015)	(0.012)
Province dummies	Yes	Yes	Yes	Yes
Baseline controls	Yes	Yes	Yes	Yes
Observations	1,127	1,127	1,127	1,127
R-squared	0.082	0.037	0.031	0.041
Mean in Control Group	0.168	0.194	0.0346	0.0293
p-value: ICT = ICT plus	0.318	0.437	0.324	0.424

Note: Standard errors are clustered at the village level. ***, **, and * indicate significance at 1, 5 and 10 percent respectively. Controls include household head's age, gender, dummies for education level, asset index, and household non-agricultural income at baseline.

We next consider whether or not the ePADEE treatments affected knowledge related to fertilizer applications or the diseases that households could identify in rice, from pictures that enumerators showed respondents (Table 22). We do not find any significant coefficients on the four variables tested, which are the ideal number of fertilizer applications on rice plots, the number of days after planting to wait before applying the first fertilizer, and the number of diseases the household claims to identify or correctly identifies out of ten. These results show that farmers do not appear to have learned much about the way to use fertilizer or the type of diseases that might afflict their crops. Whereas the former is a little surprising, the latter might not be; if farmers did not express a need for help with rice diseases, they might not have learned that information from the MST.

Table 22. Impacts of ePADEE on variables related to knowledge of rice practices and diseases, by treament status

	(1)	(2)	(3)	(4)
	Ideal # of fertilizer application in rice plots per wet season	Days to wait before applying fertilizer the 1st time after seeds are sown	# of diseases HH claims to identify out of 10	# of diseases that HH correctly identifies out of 10
Treatment arm: ICT	0.049	-0.850	0.143	0.031
	(0.057)	(1.695)	(0.249)	(0.082)
Treatment arm: ICT plus	0.024	-1.300	-0.065	-0.004
	(0.051)	(1.499)	(0.203)	(0.072)
Province dummies	Yes	Yes	Yes	Yes
Baseline controls	Yes	Yes	Yes	Yes
Observations	1,000	1,000	1,000	1,000
R-squared	0.048	0.053	0.053	0.038
Mean in Control Group	2.716	21.85	2.432	0.500
P-value: ICT = ICT plus	0.624	0.776	0.348	0.645

Note: Sample includes all households that engage in rice cultivation. Standard errors are clustered at the village level. ***, **, and * indicate significance at 1, 5 and 10 percent respectively. Diseases include: bacterial blight, leaf streak, sheath brown rot, blast (leaf and collar), blast (neck and node), brown spot, rice rugged stunt, sheath blight, stem rot, and tungro. Controls include household head's age, gender, dummies for education level, asset index, and household non-agricultural income at baseline.

To build on results in columns 3 and 4 of Table 22, we further disaggregate the practices and diseases into indices that measure the ability to identify diseases, how to prevent and treat them, and whether farmers were affected by specific diseases (Table 23). Not surprisingly, we do not find significant impacts; in fact, we find two negative coefficients that are significant at the 10 percent level for the ICT plus specification. Note that we should not have expected significance in the final column unless farmers became able to retroactively assess diseased crops.

Table 23. Impacts of ePADEE on variables related to knowledge of rice practices and diseases, by treatment status

	(1)	(2)	(3)	(4)	(5)
	Claims to identify the disease	Correctly identifies the disease	Knows how to prevent the disease	Knows how to treat the disease	Was affected by the disease last year
Treatment: ICT	0.0366	0.0380	-0.0129	-0.0092	0.0190
	(0.0582)	(0.0402)	(0.0626)	(0.0674)	(0.0537)
Treatment: ICT plus	-0.0135	0.0038	-0.0802	-0.0886	-0.0573
	(0.0477)	(0.0348)	(0.0477)	(0.0545)	(0.0446)
Province dummies	Yes	Yes	Yes	Yes	Yes
Baseline controls	Yes	Yes	Yes	Yes	Yes
Number of Obs	1000	1000	1000	1000	1000

Note: Standard errors are clustered at the village level. ***, **, and * indicate significance at 1, 5 and 10 percent respectively. Sample includes all households that engage in rice cultivation. Controls include household head's age, gender, dummies for education level, asset index, and household non-agricultural income at baseline.

Following Kling et al. (2007), we report the standarized effect across 10 outcomes, corresponding to 10 diseases, using the seemingly-unrelated regression framework to account for covariance across estimates. Diseases include: bacterial blight, leaf streak, sheath brown rot, blast (leaf and collar), blast (neck and node), brown spot, rice rugged stunt, sheath blight, stem rot, and tungro.

Cols. 3, 4, and 5 are not conditional upon correct identification of the disease.

Next we study, in a disaggregated manner, whether farmers are able to identify different types of soil deficiencies (Table 24). These deficiencies would have been identified during the soil testing phase, after which specific seeds and fertilizers would have been recommended. We find that treatment group households are no more likely to both know whether any plot has a soil deficiency, can identify a plot with a soil deficiency, nor can they identify specific deficiencies any better than the control group. To note, some of the deficiencies are not very common, likely due to the relatively high rates of NPK use identified earlier.[20]

[20] Results are similar for the proportion of area identified as deficient; see Appendix Table C.1.

Table 24. Impacts of ePADEE on variables related to knowledge of soil deficiencies, by treatment status

	Treatment: ICT	Treatment: ICT plus	Control Group Mean
HH knows whether any of the plot has soil deficiency or not	0.013 (0.035)	0.011 (0.030)	0.747
HH identifies at least one plot with soil deficiency	-0.009 (0.043)	-0.002 (0.044)	0.593
HH identifies at least one plot with urea deficiency	0.033 (0.029)	0.010 (0.034)	0.162
HH identifies at least one plot with phosphorous deficiency	0.015 (0.022)	-0.002 (0.022)	0.122
HH identifies at least one plot with nitrogen deficiency	0.010 (0.011)	0.020 (0.012)	0.024
HH identifies at least one plot with iron deficiency	0.008 (0.008)	-0.005 (0.007)	0.016
HH identifies at least one plot with compost deficiency	-0.017 (0.034)	-0.048 (0.037)	0.279

Note: Standard errors are clustered at the village level. ***, **, and * indicate significance at 1, 5 and 10 percent respectively. Each row represents a separate regression that includes province level fixed effects. 1127 observations in all regressions. Controls include household head's age, gender, dummies for education level, asset index, and household non-agricultural income at baseline.

We next examine whether or not treatment status affected the use of various inputs (Table 25).[21] We find only one marginally significant coefficient, on the use of urea in rice cultivation. Although we do not perform a multiple hypothesis test here, given that it is one coefficient out of eight reported here one might think it is the expected rejection of the null. Note that the discrete variables have relatively high mean values in general, as households tend to use these inputs in general. Perhaps a more interesting question is if they changed their intensity of use.

[21] We do not include a variable for the use of improved seed because all households at endline who grew rice used improved seed.

Table 25. Impacts of ePADEE on discrete variables related to input use, by treatment status

	(1) Used improved seed in rice cultivation	(2) Used NPK in rice cultivation	(3) Used urea in rice cultivation	(4) Used compost in rice cultivation	(5) Used pesticide in rice cultivation
Treatment arm: ICT	0.046	0.004	0.032	-0.028	0.074
	(0.062)	(0.033)	(0.032)	(0.047)	(0.057)
Treatment arm: ICT plus	0.016	0.025	0.063	0.016	0.072
	(0.059)	(0.031)	(0.029)**	(0.042)	(0.053)
Province dummies	Yes	Yes	Yes	Yes	Yes
Control for outcome at baseline	Yes	Yes	Yes	Yes	Yes
Baseline controls	Yes	Yes	Yes	Yes	Yes
Observations	976	976	976	976	976
R-squared	0.241	0.094	0.175	0.271	0.317
Mean in Control Group	0.497	0.852	0.861	0.751	0.624
p-value: ICT = ICT plus	0.553	0.505	0.208	0.292	0.966

Note: Standard errors are clustered at the village level. ***, **, and * indicate significance at 1, 5 and 10 percent respectively. Controls include household head's age, gender, dummies for education level, asset index, and household non-agricultural income at baseline.

To address that question, we next examine the quantity and value of various types of fertilizer, on a per hectare basis (Table 26). Note that these regressions are all conditional on already growing rice. We find no significant coefficients on either quantity or value of NPK, urea, or compost applied per hectare. Given the relatively high rates of MPK and urea application, *a priori* it is not clear whether recommendations should have been to increase or decrease fertilizer use, or to time it differently to attempt to maximize yields. Still, there is no obvious effect on use as the coefficients are all quite imprecisely estimated.

Table 26. Impacts of ePADEE on fertilizer use per hectare variables related to input use, by treatment status

	(1)	(2)	(3)	(4)	(5)	(6)
	Quantity of NPK applied per ha of land (Kg/ha)	Quantity of urea applied per ha of land (Kg/ha)	Quantity of compost applied per ha of land (Kg/ha)	Value of NPK applied per ha of land (Kg/ha)	Value of urea applied per ha of land (Kg/ha)	Value of compost applied per ha of land (Kg/ha)
Treatment arm: ICT	8.531	5.815	16.010	1.425	0.432	0.907
	(10.211)	(11.540)	(176.010)	(2.319)	(2.052)	(0.608)
Treatment arm: ICT plus	-6.451	-2.470	-101.269	-1.806	-1.081	0.309
	(8.727)	(8.790)	(165.215)	(2.055)	(1.487)	(0.304)
Province dummies	Yes	Yes	Yes	Yes	Yes	Yes
Baseline controls	Yes	Yes	Yes	Yes	Yes	Yes
Control for outcome at baseline	Yes	Yes	Yes	Yes	Yes	Yes
Observations	976	976	976	976	976	976
R-squared	0.276	0.224	0.150	0.274	0.179	0.107
Mean in Control Group	119.6	114.4	1246	27.75	20.11	0.548
p-value: ICT = ICT plus	0.0853	0.466	0.375	0.0938	0.428	0.320

Note: Standard errors are clustered at the village level. ***, **, and * indicate significance at 1, 5 and 10 percent respectively. Controls include household head's age, gender, dummies for education level, asset index, and household non-agricultural income at baseline.

Finally, we examine the impacts on agricultural expenditures and rice yields (Table 27). Not surprisingly, we find that the ePADEE intervention neither appears to affect expenditures nor yields per hectare. Point estimates for both groups are small relative to the control group mean and are not close to significant. Given the previous results, it is not surprising that the ePADEE intervention appears not to have affected input expenditures or rice yields

Table 27. Impacts of ePADEE on agricultural expenditures and rice yields

	(1)	(2)
	Expenditure on agricultural input & labor (0000 Riels/ha)	Total rice harvest (kg) per ha of land
Treatment arm: ICT	10.040	110.070
	(8.024)	(153.587)
Treatment arm: ICT plus	-3.767	-69.531
	(5.721)	(144.567)
Province dummies	Yes	Yes
Baseline controls	Yes	Yes
Control for outcome at baseline	Yes	Yes
Observations	976	976
R-squared	0.172	0.355
Mean in Control Group	124.7	2973
p-value: ICT = ICT plus	0.0600	0.264

Note: Standard errors are clustered at the village level. ***, **, and * indicate significance at 1, 5 and 10 percent respectively. Controls include household head's age, gender, dummies for education level, asset index, and household non-agricultural income at baseline.

In sum, it appears that the ePADEE intervention was effective in increasing the contact between MSTs and farmers. A notable share of farmers believe that the tablets helped MSTs give better advice. This point came through in our qualitative work as well; working with the MSTs, farmers feel like they have learned a great deal. However, in our qualitative work several farmers admitted that they still could not follow all the procedural steps given to them by the MSTs. There were two main reasons. First, there was sometimes a lack of either water or availability of enough of a key input, such as Kali, one of the sometimes recommended fertilizers.[22] Second, farmers are not used to following such recommendations, so they might need more time to further experiment. Nonetheless, in the aggregate the intervention did not appear to change knowledge among farmers relative to the control group, and so lack of aggregate changes in input use not surprisingly do not appear to have much effect on input use by farmers or final outcomes.

7.4.2 Qualitative Analysis

Experience with Extension

Our focus groups included a set of questions meant to stimulate discussion about farmers' experience with extension. Prior to the ePADEE intervention, farmers reported that contact with agricultural extension workers has been irregular and minimal (even within PADEE). Farmers stated that the district official comes to their commune once a year for agricultural development

[22] Kali was not included in the quantitative work except as "other".

planning. If there is financial support, the official would then provide some extension services as well. However, farmers expressed that the content of any such extension they have received in the past from was very general and not terribly relevant. For example, they mentioned general information being disseminated, such as how to apply inputs such as fertilizer and pesticides. However, they did not know exactly the types of inputs to apply, not anything about how to time the application of fertilizer to optimize its effectiveness. In general, though farmers apply inputs, they do not necessarily know how to best apply them.

Whereas farmers appear to know of more recently disseminated techniques to grow rice, they perceive that the effort to apply them is too high relative to the benefits. As a result, they have not begun to use such techniques. Rather, they are happy to continue following traditional practices, which minimizes the effort in cultivating rice. Weeding (especially removing grasses) and newer water supervision techniques that are time consuming tend not to be used.

With respect to e-PADEE, farmers appreciated their tailored recommendations, which allowed them to know exactly what inputs to use and the amount of inputs required for each unique case. Farmers that tried to precisely follow the recommendations stated their rice grew better than in previous years and perceived higher yields They also stated they learned a great deal. However, for two reasons many farmers admitted that they could not follow all the procedural steps given to them by the MSTs. First, there was sometimes a lack of either water or availability of enough of a key input, such as Kali, one of the recommended fertilizers. Second, farmers are not used to following such recommendations, so they might need more time to further experiment.

Perhaps not surprisingly, farmers are uncertain about continuing to use these techniques once PADEE and e-PADEE are gone. While farmers perceive that the soil test was important, and they do not know where to get the soil testing service, and the groups made it relatively clear that farmers are not committed to pay for such services. In terms of where to get advice, farmers did realize they can call their MST, but not a single farmer in any focus group had ever placed a call to an MST. As a result, at endline the enumerators for the ICT and ICT Plus villages were asked to read a short script at the end of the interview stating that the fertilizer recommendations they had received were still valid, and that they should feel free to call the MSTs for additional help.

In general, farmers in the focus group discussions tended to split farmers in the village into two groups – the first group are farmers who work hard at farming, and so they are interested in learning and applying new technologies or techniques to try to make their lives better. A second group of farmers are not as interested in farming and are perceived to just try to farm just to grow enough food and minimize effort. These farmers, it was argued, simply do not care much in improving their livelihoods, though they may have other sources of income. Targeting these projects might be instrumental to their success; in our case, we relied fully on the targeted that was conducted earlier by MAFF.

Finally, we were interested in whether farmers would trust people other than MSTs, who tend to be district agricultural officers. In determining that the initial e-PADEE program would work with MSTs, SNV and MAFF had determined that Community Extension Workers (CEWs) would not be trusted enough; however, MSTs do not exist in ASPIRE, so any extension like e-PADEE would need to occur through CEWs. Farmers in the focus group discussions stated that they were not concerned about who was teaching extension techniques, so long as the extension worker has relevant skills and experience. So long as CEWs or those they facilitate in conducting extension can convince farmers they have relevant skills, the absence of MSTs should not present a problem.

Crop Decisions and Income

In general, farmers want to grow both rice and cash crops. Cash crops are, according to farmers, a potential source for supplemental income, as they perceive the main source of income as rice production. So farmers perceive vegetables in particular as an attractive cash crop, for interesting reasons. The groups tended to suggest vegetables are attractive as the growing period is shorter than for rice, and therefore they receive income more quickly. Moreover, they perceive that growing vegetables typically uses less labor than rice.

That said, in some villages farmers have experimented with growing vegetables, but they have stopped for a few reasons. First, some claim to lack enough water. Second, vegetables have different diseases than rice, and farmers do not know how to deal with them when they come about. Third, similarly farmers lack knowledge about how to deal with pests that attack vegetables. As a consequence, vegetable production is not as lucrative as it could be.

On the output side, farmers feel like it is easier to try to develop crops beyond rice if there is a formal network linking them to agricultural markets. Farmers in places that are not so far from cities or provincial seats (around 10 km or below) have no problem selling livestock and vegetables. However, the price is not guaranteed. On the other hand, farmers in villages farther from cities or provincial seats have a major issue in trying to sell livestock or vegetables. Farmers in such villages state they do not want to grow more vegetables, as there is no market. So, they grow only a small amount for own consumption. Some farmers can sell their products within their village, but the market is very small.

When conversations turned to discussing the best strategies for generating a household income, and whether or not that includes farming, most of the farmers stated that they thought that off-farm work was not for them. Many of them had worked as unskilled labor migrants elsewhere for a time, but they did not find that the wages were providing attractive incomes. However, this condition might be due to the timing of that migration, which often occurs among younger people; migrant opportunity appears to have increased within Cambodia. Also, migrant perspectives were not represented in these discussions, as they are not regularly present in the village. Rearing livestock is also an opportunity for supplemental income generation. So long as chickens are for sale, farmers state that middlemen will come to collect them at the farm gate. The problem is that most farmers do not have "technology" to raise chickens, and they sometimes do not raise livestock in the closed household system.

Farmers who were present stated that so long as they have agricultural land and there is sufficient water, they prefer to stay put in their village. A concern is whether there will continue to be enough water. Moreover, they are concerned about the price of inputs, which might not be profitable to use given the marginal increase in production and prevailing prices for outputs.

Phone Call Reactions

Finally, we asked farmers to react to the phone call interviews that occurred in one of the treatment groups. Some farmers found the first call was a bit confused. It could be that interviewers were not used to asking questions by phone; most interviews are conducted with pen and paper in the households. However, farmers who received calls reported that after the first call, they got used to answering questions in this manner, and the calls were relatively "normal." However, they did state there were problems recalling some of the information that was asked. This problem might occur, of course, in a standard interview within the household as well.

Software

A further question of interest based on the MST interviews is whether the MSTs find e-PADEE useful in providing recommendations. Recall that the e-PADEE software is predicated on initially conducting a soil test. The MSTs interviewed acknowledged that the tool was quite effective at providing tailored recommendations to farmers, that it is fast and easy to use, as inputs just need to go into the apps and the recommendations come out. A challenge was that a very small proportion of farmers had already planted when recommendations came about, so they were unable to follow seed recommendations.

A larger challenge for a handful of MSTs (no more than four) was that their tablets did not work very well; e.g. they would crash if on-line with the software open. They had to use the e-PADEE apps offline, and then load the data to the server when connectivity improved. Another suggestion made by a few MSTs is that an additional meeting in the middle of the project would have been helpful to increase discussion about how people were using the software and could have helped with some of the ongoing problems. In ASPIRE, during 2017 it was planned that all CEWs would receive tablets with several different options for extension software; we recommended to the government that they set up *Whatsapp* groups for the CEWs to correspond with one another about potential problems, as communication across extension workers can be quite beneficial and the technologies already exist to facilitate that communication.

7.5 PADEE Heterogeneity

We complement the impact analysis of the complementary innovations implemented under PADEE with a heterogeneity analysis related to MST characteristics. We present the heterogeneity by estimating the difference in the impact estimates for farmers that share a given characteristic.

First, we present heterogeneity across social connectedness, which we define as the MST knowing more than 10% of the farmers in the village before starting to work as an MST. Table 28 shows the estimated impacts among farmers with high social connectedness. Among the ICT group, there are clear differences between impacts among those with low social connectedness and the higher social connectedness, as demonstrated by the significant coefficents on the interaction term between ICT group and MSTs with high connectedness. The incentive intervention may have manipulated this effect; we see fewer significant coefficients in the interaction term for the ICT plus group, while the basic coeffiicents remain significant as they were in the previous subsection. The exception is in column (4), as high social connectedness MSTs were more likely, apparently, to bring their tablet as they visit.

Table 28. Impacts of ePADEE on interaction with MST, treatment status interacted with MST's social connectedness

	(1)	(2)	(3)	(4)	(5)
	HH knows MST member (name or phone number)	MST member visited in the past 12 months	Frequency HH received advice from MST in last 12 months	MST who visited had tablet	MST's tablet helped the MST give better advise
ICT	0.053	0.097	0.357	0.029	0.081
	(0.049)	(0.082)	(0.249)	(0.057)	(0.057)
ICT plus	0.084	0.245	0.727	0.128	0.107
	(0.044)*	(0.086)***	(0.301)**	(0.048)**	(0.046)**
High social connectedness	-0.004	-0.126	-0.267	-0.096	-0.041
	(0.037)	(0.080)	(0.212)	(0.049)*	(0.042)
ICT X High social connectedness	0.070	0.385	0.866	0.222	0.107
	(0.075)	(0.111)***	(0.326)**	(0.076)***	(0.073)
ICT plus X High social connectedness	-0.015	0.146	0.295	0.173	0.093
	(0.069)	(0.149)	(0.534)	(0.084)**	(0.078)
Province dummies	Yes	Yes	Yes	Yes	Yes
Control for outcome at baseline	Yes		Yes		
Observations	1,087	1,087	1,087	1,087	998
R-squared	0.056	0.134	0.084	0.078	0.071
Mean in control group	0.0851	0.287	0.689	0.165	0.120

Note: Standard errors are clustered at the village level. ***, **, and * indicate significance at 1, 5 and 10 percent respectively. Controls include head's age, gender, education level dummies, household asset index, and off-farm household income at baseline. Social connectedness is a dummy which equals 1 if the MST knows more than 10% of the villagers before starting work as MST.

Table 29 shows the impact estimates for extensions services received. These estimates differ from the previous table, as they show social connectedness mattering in both treatments. Estimates indicate that farmers under high social connectedness MSTs are less likely to have received extension; although the main effect among both treatment groups remains positive. In additions, high social connectedness seems increase the impact estimates for receiving extensions through the government or the PADEE program. The estimates suggest that a sense of familiarity between the MSTs and the village farmers decreases the likelihood of delivering services and that this was counterbalanced by the ICT and incentive scheme that was tested.

Table 29. Impacts of ePADEE on extension services received, treatment status interacted with MST's social connectedness

	(1)	(2)	(3)	(4)	(5)
	Received any extension service	Range of extension services received (Range: 0-13)	Received extension service from a government agent	Received extension service through PADEE program	Number of extension visits
ICT	0.153	0.852	0.125	0.130	1.822
	(0.055)***	(0.521)	(0.048)**	(0.051)**	(1.017)*
ICT plus	0.178	0.717	0.247	0.255	2.034
	(0.056)***	(0.390)*	(0.057)***	(0.055)***	(0.825)**
High social connectedness	-0.150	-0.562	-0.144	-0.138	-0.479
	(0.059)**	(0.377)	(0.057)**	(0.061)**	(0.619)
ICT X High social	0.203	0.551	0.333	0.355	1.402
connectedness	(0.082)**	(0.629)	(0.077)***	(0.080)***	(1.293)
ICT plus X High social	0.135	0.052	0.203	0.208	-0.553
connectedness	(0.102)	(0.616)	(0.108)*	(0.112)*	(1.176)
Province dummies	Yes	Yes	Yes	Yes	Yes
Observations	975	975	975	975	975
R-squared	0.102	0.056	0.123	0.130	0.052
Mean in control group	0.518	1.675	0.370	0.328	1.938

Note: Standard errors are clustered at the village level. ***, **, and * indicate significance at 1, 5 and 10 percent respectively. Controls include head's age, gender, education level dummies, household asset index, and off-farm household income at baseline. Social connectedness is a dummy which equals 1 if the MST knows more than 10% of the villagers before starting work as MST.

We next disaggregate the impact estimates by incentives received. MSTs who received incentives above the median put more effort into implementing than those who received incentives below the median, so we effectively disaggregate the ICT plus group only. We find the MSTs in the ICT plus group that received a larger incentive were the ones that farmers report were more likely to visit them, had the tablet with them and the farmers believes that the technology helped the MST give better advise (Table 30). While the differences in impacts between those above and below the median incentives at the end of the program are not large (statistically different), the estimates suggest the boost in effort among MSTs in the incentive group that is described in the theory of change.

Table 30. Impacts of ePADEE on interaction with MST, treatment status interacted with incentive payment to MST

	(1)	(2)	(3)	(4)	(5)
	HH know MST member (name or phone number)	MST member visited in the past 12 months	Frequency HH received advice from MST in last 12 months	MST who visited had tablet	MST's tablet helped the MST give better advise
ICT	0.085	0.281	0.740	0.133	0.123
	(0.036)**	(0.057)***	(0.170)***	(0.041)***	(0.037)***
ICT plus X MST incentive	0.062	0.288	0.794	0.177	0.112
below median	(0.038)	(0.088)***	(0.317)**	(0.057)***	(0.050)**
ICT plus X MST incentive	0.110	0.319	0.726	0.186	0.138
above median	(0.052)**	(0.076)***	(0.281)**	(0.042)***	(0.040)***
Province dummies	Yes	Yes	Yes	Yes	Yes
Control for outcome at baseline	Yes		Yes		
Observations	1,107	1,107	1,107	1,107	1,022
R-squared	0.056	0.112	0.063	0.067	0.066
Mean in control group	0.0851	0.287	0.689	0.165	0.120

Note: Standard errors are clustered at the village level. ***, **, and * indicate significance at 1, 5 and 10 percent respectively. Controls include head's age, gender, education level dummies, household asset index, and off-farm household income at baseline.

Similarly, we estimate the impact disaggregating by this effort measure in Table 31, for the extension services received by farmers. Consistent with the what we observed before, MSTs with above the median incentives generally have impact estimates of larger magnitude, though in this context the difference is not statistically significant. As a consequence of these two sets of tables, we observe that the incentives did have some effect; they broke any effects of social connectedness on visits undertaken by MSTs, and those who put in more effort appear to have also been more visible to farmers. However, that visibility was not enough to generate stronger impacts.

Table 31. Impacts of ePADEE on extension services received, treatment status interacted with incentive payment to MST

	(1)	(2)	(3)	(4)	(5)
	Received any extension service	Range of extension services received (Range: 0-13)	Received extension service from a government agent	Received extension service through PADEE program	Number of extension visits
ICT	0.254	1.200	0.294	0.311	2.676
	(0.042)***	(0.306)***	(0.047)***	(0.048)***	(0.625)***
ICT plus X MST incentive	0.266	0.939	0.329	0.321	2.115
below median	(0.068)***	(0.362)**	(0.073)***	(0.070)***	(0.940)**
ICT plus X MST incentive	0.237	1.075	0.330	0.365	2.274
above median	(0.044)***	(0.401)***	(0.050)***	(0.051)***	(0.657)***
Province dummies	Yes	Yes	Yes	Yes	Yes
Observations	980	980	980	980	980
R-squared	0.093	0.057	0.100	0.107	0.052
Mean in control group	0.518	1.675	0.370	0.328	1.938

Note: Standard errors are clustered at the village level. ***, **, and * indicate significance at 1, 5 and 10 percent respectively. Controls include head's age, gender, education level dummies, household asset index, and off-farm household income at baseline.

7.6 ASPIRE Main Impact Analysis

In this sub-section, we follow the pre-analysis plan closely to estimate impacts of the basic message, enhanced message, and enhanced message plus treatments.

7.6.1 Impacts on ASPIRE Knowledge and Receipt of Calls

First, we examine outcomes related to knowledge of the program (Table 32).[23] We expect that households have no increased interaction with the ASPIRE program, as the phone calls did not specifically promote any of ASPIRE. We find that none of the treatment arms have a significant relationship with knowledge of ASPIRE. This result is not entirely surprising, since we do choose both ASPIRE group members and non-members in the same proportion in each treatment. Note that household membership of ASPIRE groups is lower than expected; it should be 42%, but the average is 35%. Only 16% of households have been visited by a CEW, though more have attended a meeting or training during the previous 12 months.

[23] At present, all regressions only include regional fixed effects and do not include baseline control variables.

Table 32. Impacts of Direct Calls on knowledge of ASPIRE Program, by treatment status

	(1)	(2)	(3)	(4)	(5)
	Knows about ASPIRE program	HH is a member of an ASPIRE group	Has been visited by CEW	Attended meeting/training organized by CEW in past 12 months	Knows CEW's name
Treatment arm: Basic	-0.000	0.006	0.025	0.000	-0.016
	(0.020)	(0.018)	(0.023)	(0.025)	(0.015)
Treatment arm: Enhanced	0.005	0.017	0.014	0.009	-0.005
	(0.021)	(0.019)	(0.022)	(0.027)	(0.015)
Treatment arm: Enhanced plus	-0.003	0.008	0.026	-0.001	-0.014
	(0.023)	(0.017)	(0.025)	(0.026)	(0.016)
CEW fixed effects	Yes	Yes	Yes	Yes	Yes
Control for outcome at baseline	Yes		Yes		
Observations	1,687	1,687	1,687	1,687	1,687
R-squared	0.438	0.022	0.192	0.033	0.018
Mean in Control Group	0.435	0.352	0.163	0.239	0.0615

Note: Standard errors are clustered at the village level. ***, **, and * indicate significance at 1, 5 and 10 percent respectively. Baseline controls include household head's age, gender, whether the head is literate, and household's non-agricultural wage income.

We next examine household experience with receiving phone calls related to rice (Table 33), since almost all households that received phone calls received calls about rice. First, we ask if households ever received a call. We find that the basic treatment arm households are 7.7 percentage points more likely to state they received calls than the control group, and the enhanced arm households are 12 to 15 percentage points more likely to receive calls. According to the schedule, basic message farmers should have received them in September; although their recall is not perfect, they are much more likely to report receiving messages in September than other months. Enhanced message farmers are more likely in all months to report receiving messages than the control group. Finally, it is notable that households feel like the rice messages helped them increase production on the margin; 5.8 percentage points more farmers in the basic treatment arm and 10-12 percentage points more farmers in the enhanced treatment arms answered that they felt the phone calls helped their rice production increase.

Table 33. Impacts of direct calls to ASPIRE groups on receipt of rice messages, by treatment status

	(1)	(2)	(3)	(4)	(5)	(6)
	Ever received voice message	Message received in Septem-ber	Message received in October	Message received in November	Message received in December	Voice message helped increase production
Treatment arm: Basic	0.075 (0.017)***	0.049 (0.012)***	0.027 (0.011)**	0.014 (0.009)	0.021 (0.010)**	0.056 (0.015)***
Treatment arm: Enhanced	0.131 (0.026)***	0.066 (0.013)***	0.053 (0.016)***	0.039 (0.013)***	0.036 (0.011)***	0.107 (0.021)***
Treatment arm: Enhanced plus	0.149 (0.020)***	0.075 (0.013)***	0.067 (0.014)***	0.058 (0.011)***	0.058 (0.010)***	0.116 (0.017)***
CEW fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,687	1,687	1,687	1,687	1,687	1,687
R-squared	0.057	0.034	0.030	0.030	0.028	0.051
Control Group Mean	0.0213	0.00709	0.0142	0.0118	0.00946	0.0118

Note: Standard errors are clustered at the village level. ***, **, and * indicate significance at 1, 5 and 10 percent respectively. Baseline controls include household head's age, gender, whether the head is literate, and household's non-agricultural wage income.

We find similar coefficients for impacts of the treatments on receipt of messages about chicken production (Table 34). As with the rice messages, farmers in the enhanced message groups are twice as likely to report ever receiving messages than the basic message groups, but both are far more likely to report receipt than the control group. Messages to the basic group went out in both September and October, and that is reflected in the coefficient estimates, though they are higher for each month among the enhanced message groups. Notably some farmers in all three groups report than the voice messages helped their chicken production; the difference is 4.8 percentage points for the basic message group and between 7.8 and 9.2 percentage points for the enhanced message groups.

Table 34. Impacts of direct calls to ASPIRE groups on receipt of chicken messages, by treatment status

	(1)	(2)	(3)	(4)	(5)
	Ever received voice message	Message received in September	Message received in October	Message received in November	Voice message helped increase production
Treatment arm: Basic	0.061	0.027	0.036	0.013	0.047
	(0.017)***	(0.010)***	(0.010)***	(0.008)	(0.015)***
Treatment arm: Enhanced	0.124	0.048	0.043	0.031	0.079
	(0.022)***	(0.010)***	(0.011)***	(0.008)***	(0.017)***
Treatment arm: Enhanced plus	0.128	0.036	0.043	0.050	0.091
	(0.018)***	(0.011)***	(0.010)***	(0.009)***	(0.015)***
CEW fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	1,687	1,687	1,687	1,687	1,687
R-squared	0.045	0.023	0.017	0.025	0.036
Mean in Control Group	0.0236	0.00709	0.0118	0.00946	0.0189

Note: Standard errors are clustered at the village level. ***, **, and * indicate significance at 1, 5 and 10 percent respectively. Baseline controls include household head's age, gender, whether the head is literate, and household's non-agricultural wage income.

Finally, we examine the impacts of the treatments on the receipt of calls about cucumber and long bean production (Table 35). Somewhat surprisingly, the coefficients are lower than those for chickens. Farmers have a harder time recalling in which months they received calls, and farmers are less likely to report that they ever received voice messages about cucumbers and long beans. Although literally true (recall, only 602 farmers in the treatment groups were targeted with vegetable messages, and not all such farmers are in our sample), this finding is surprising relative to the chicken messages, since those were received by even fewer farmers. Finally, the farmers suggest that the voice messages were not terribly helpful in increasing production, though the coefficients in the enhanced treatment arms for cucumbers suggest some of those farmers found the messages subjectively useful.

Table 35. Impacts of direct calls to ASPIRE groups on receipt of cucumber and long bean messages, by treatment status

	(1)	(2)	(3)	(4)	(5)
	Ever received voice message	Message received in October	Message received in November	Message received in December	Voice message helped increase production
Panel A: Cucumber					
Treatment arm: Basic	0.038	0.018	0.004	0.006	0.002
	(0.014)***	(0.010)*	(0.009)	(0.007)	(0.005)
Treatment arm:	0.042	0.022	0.010	0.009	0.014
Enhanced	(0.016)**	(0.011)*	(0.011)	(0.008)	(0.006)**
Treatment arm:	0.059	0.013	0.016	0.016	0.023
Enhanced plus	(0.016)***	(0.009)	(0.011)	(0.007)**	(0.007)***
CEW fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	1,687	1,687	1,687	1,687	1,687
R-squared	0.035	0.022	0.020	0.011	0.016
Mean in Control Group	0.0189	0.0118	0.0118	0.00946	0.00236
	Ever received voice message	Message received in September	Message received in October	Message received in November	Voice message helped increase production
Panel B: Long Beans					
Treatment arm:	0.048	0.012	0.016	0.007	0.010
Basic	(0.009)***	(0.008)	(0.008)*	(0.005)	(0.006)*
Treatment arm:	0.037	0.011	0.010	0.001	0.013
Enhanced	(0.011)***	(0.007)	(0.009)	(0.006)	(0.007)*
Treatment arm:	0.059	0.022	0.002	0.025	0.019
Enhanced plus	(0.011)***	(0.009)**	(0.007)	(0.008)***	(0.008)**
CEW fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	1,687	1,687	1,687	1,687	1,687
R-squared	0.021	0.014	0.012	0.025	0.020
Mean in Control Group	0.00946	0.00473	0.00946	0.00709	0.00236

Note: Standard errors are clustered at the village level. ***, **, and * indicate significance at 1, 5 and 10 percent respectively. Baseline controls include household head's age, gender, whether the head is literate, and household's non-agricultural wage income.

In sum, we find that farmers in the enhanced message groups were more likely to report that they had received calls than farmers in the basic message group, and they were more likely to report that the messages helped them increase production, at least in a subjective manner. However, it is worth discussing the magnitude of estimates. Certainly, not all farmers should have been receiving calls, based on the set up of the experiment. However, we might have expected larger coefficients on the “ever received” variables than we find. Some of the difference between what we might have expected from administrative data and what we found comes from the fact that we may not have interviewed the person who received messages; however, these treatment effects are quite small. Second, it is worth mentioning there is some clear measurement error; the control group means should have all been zero, since we never gave any phone numbers from the control group to VOTO mobile. The subjective measures of production are nice; however, they are not necessarily positive for finding impacts in measured intent-to-treat effects unless those farmers reporting subjectively positive impacts had really large increases in production.

7.6.2 Impacts within Social Networks

A major goal of the phone calls was to provide additional information into the community about farming practices for rice, cucumbers, long beans, and chicken rearing, so they would spread into communities. Hence, we next measure whether the direct calls affected any farmers gave or received advice from others. Recall, some of the farmers in our data set did not receive messages, but they may have benefitted from farmers who did.

We start by examining the impacts of treatments on whether farmers shared messages related to rice (Table 36). We find that there appears to be an impact on sharing messages within villages, though the magnitude is small. For the basic message group, the increase in sharing advice over the control group is 1.9 percentage points; it is between 3.8 and 5.7 percentage points for the enhanced groups. The changes among the enhanced groups leads to more sharing within the village; both of the enhanced groups suggest sharing advice with an average of 0.3 to 0.4 more people. Sort of interestingly, the enhanced group has a larger magnitude coefficient than the enhanced plus group, which had a larger number of farmers overall receive calls.

Table 36. Impacts of direct calls to ASPIRE groups on sharing advice about rice messages, by treatment status

	(1)	(2)	(3)
	Rice message: Shared advice from the message with other farmers/persons	Rice message: # of people inside village with whom farmer shared advice	Rice message: # of people outside village with whom farmer shared advice
Treatment arm: Basic	0.019	0.048	-0.021
	(0.009)**	(0.064)	(0.023)
Treatment arm: Enhanced	0.058	0.401	0.022
	(0.014)***	(0.131)***	(0.030)
Treatment arm: Enhanced plus	0.037	0.334	0.076
	(0.010)***	(0.094)***	(0.038)*
CEW fixed effects	Yes	Yes	Yes
Observations	1,687	1,687	1,687
R-squared	0.028	0.019	0.018
Mean in Control Group	0.00709	0.0355	0.0236

Note: Standard errors are clustered at the village level. ***, **, and * indicate significance at 1, 5 and 10 percent respectively. Baseline controls include household head's age, gender, whether the head is literate, and household's non-agricultural wage income.

The enhanced message treatments were more effective in creating networks around the rice messages that were received (Table 37). We next measure whether farmers knew someone who received a message, the number of people known, whether techniques were learned from those who received messages and whether those techniques were again shared, and then the number of households with whom production improvements were discussed. In the first three cases (columns 1-3), the enhanced message treatments had positive impacts; these effects are not surprising, given that those treatments occurred more frequently and in the case of the enhanced message plus group, more farmers received the message. If farmers learned techniques second-hand from the messages, only in the enhanced treatment plus do they appear to then share those techniques forward; the coefficient is quite small, however.

Table 37. Impacts of direct calls to ASPIRE groups receiving advice and sharing it on about rice messages, by treatment status

	(1)	(2)	(3)	(4)	(5)
	Knows someone who received message	# of people known who received message	Learned techniques from others who received message	Shared techniques learned from others	# of HH with whom the farmer discussed improvement in production
Treatment arm:	0.013	0.035	0.004	0.005	-0.287
Basic	(0.009)	(0.024)	(0.006)	(0.004)	(0.981)
Treatment arm:	0.049	0.118	0.024	0.005	3.845
Enhanced	(0.015)***	(0.038)***	(0.010)**	(0.004)	(1.288)***
Treatment arm:	0.053	0.133	0.025	0.014	1.683
Enhanced plus	(0.011)***	(0.030)***	(0.009)***	(0.006)**	(0.967)*
CEW fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	1,687	1,687	1,687	1,687	1,687
R-squared	0.029	0.020	0.020	0.014	0.059
Mean in Control Group	0.00709	0.0165	0.00473	0	6.792

Note: Standard errors are clustered at the village level. ***, **, and * indicate significance at 1, 5 and 10 percent respectively. Baseline controls include household head's age, gender, whether the head is literate, and household's non-agricultural wage income.

We next repeat the previous two tables for chicken messages (Table 38 and Table 39). Just as with rice, the chicken messages appear to have stimulated some discussion within villages. Again, as with the rice messages, there is a small, significant coefficient in the basic message group on sharing advice; coefficients are larger for the enhanced message group. The coefficients suggest that an additional 0.2 people on average received advice within the enhanced treatment groups. Notably, there are also coefficients significant at the 10 percent level for advice given outside the village. For receipt of advice, we find small but significant coefficients on the number of people known who received messages and whether techniques were learned from others only in the enhanced calls plus group, suggesting that the additional saturation of chicken calls in those villages might have had an effect above the smaller number of calls received within other villages.

Table 38. Impacts of direct calls to ASPIRE groups on sharing advice about chicken messages, by treatment status

	(1) Shared advice from the message with other farmers/persons	(2) # of people inside village with whom farmer shared advice	(3) # of people outside village with whom farmer shared advice
Treatment arm: Basic	0.017	0.217	0.087
	(0.007)**	(0.134)	(0.060)
Treatment arm: Enhanced	0.031	0.270	0.055
	(0.012)**	(0.141)*	(0.036)
Treatment arm: Enhanced plus	0.031	0.212	0.086
	(0.009)***	(0.094)**	(0.047)*
CEW fixed effects	Yes	Yes	Yes
Observations	1,687	1,687	1,687
R-squared	0.019	0.014	0.012
Mean in Control Group	0.00709	0.0378	0

Note: Standard errors are clustered at the village level. ***, **, and * indicate significance at 1, 5 and 10 percent respectively. Baseline controls include household head's age, gender, whether the head is literate, and household's non-agricultural wage income.

Table 39. Impacts of direct calls to ASPIRE groups receiving advice and sharing it on about chicken messages, by treatment status

	(1) Knows someone who received message	(2) # of people known who received message	(3) Learned techniques from others who received message	(4) Shared techniques learned from others	(5) # of HH with whom the farmer discussed improvement in product
Treatment arm: Basic	0.020	0.034	0.004	0.000	-0.425
	(0.007)***	(0.015)**	(0.004)	(0.002)	(0.842)
Treatment arm: Enhanced	0.022	0.041	0.009	-0.002	0.890
	(0.009)**	(0.024)*	(0.007)	(0.002)	(0.937)
Treatment arm: Enhanced plus	0.029	0.056	0.014	0.000	-0.223
	(0.006)***	(0.017)***	(0.005)***	(0.003)	(0.873)
CEW fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	1,687	1,687	1,687	1,687	1,687
R-squared	0.021	0.019	0.014	0.018	0.060
Mean in Control Group	0.00236	0.0142	0.00236	0.00236	3.780

Note: Standard errors are clustered at the village level. ***, **, and * indicate significance at 1, 5 and 10 percent respectively. Baseline controls include household head's age, gender, whether the head is literate, and household's non-agricultural wage income.

Finally, we examine whether long bean or cucumber messages affected networks; given the small magnitude of coefficients on farmers recalling they received messages, this component seems unlikely. In terms of sharing from the messages, indeed we find only two weakly significant coefficients among the enhanced plus group (Table 40), in terms of whether messages were shared with others and the number of people with whom advice was shared. We include estimates on receipt of advice on long beans or cucumbers in appendix tables (Tables C.2 and C.3). The coefficients are small and largely not different from zero.

Table 40. Impacts of direct calls to ASPIRE groups on sharing advice about long bean and cucumber messages, by treatment status

	(1)	(2)	(3)	(4)	(5)	(6)
		Long Beans			**Cucumbers**	
	Shared advice from the message with other farmers/ persons	# of people inside village with whom farmer shared advice	# of people outside village with whom farmer shared advice	Shared advice from the message with other farmers/p ersons	# of people inside village with whom farmer shared advice	# of people outside village with whom farmer shared advice
Treatment arm:	0.007	0.051	0.005	0.000	0.002	0.005
Basic	(0.005)	(0.041)	(0.018)	(0.005)	(0.021)	(0.018)
Treatment arm:	0.010	0.034	0.009	0.008	0.032	0.009
Enhanced	(0.006)*	(0.028)	(0.018)	(0.006)	(0.021)	(0.018)
Treatment arm:	0.007	0.053	0.048	0.014	0.081	0.048
Enhanced plus	(0.006)	(0.043)	(0.037)	(0.007)**	(0.036)**	(0.037)
CEW fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,687	1,687	1,687	1,687	1,687	1,687
R-squared	0.011	0.011	0.012	0.013	0.015	0.012
Control Group Mean	0.00473	0.0142	0	0.00709	0.0165	0

Note: Standard errors are clustered at the village level. ***, **, and * indicate significance at 1, 5 and 10 percent respectively. Baseline controls include household head's age, gender, whether the head is literate, and household's non-agricultural wage income.

The implications of our findings are that the rice and chicken messages certainly catalyzed some conversation around the phone messages and limited sharing with farmers who did not receive the calls. The intensity of calls (e.g. the enhanced versus the basic messages) appears to have more of an impact on the amount of sharing than the saturation of calls within a community. In sum, though, we do find that the calls have impacts on the use of social networks for farming advice.

7.6.3 Impacts on Knowledge of Practices

We next use questions that were asked in the endline questionnaire to understand whether farmers learned about specific practices related to rice production, chicken rearing, and vegetable production. The questions were derived specifically from the messages that were delivered; since we know that farmers in the enhanced groups were likely to have listened to more of the messages, we might expect larger coefficients among the enhanced treatment groups.

First, we examine 16 variables related to rice production techniques. The first set of variables relates to land and seed preparation (Table 41). We do not find any significant coefficients on variables related to land or seed preparation, suggesting that farmers have not changed techniques or beliefs about how they would prepare land for planting in the future.

Table 41. Impacts of direct calls to ASPIRE groups on variables related to land preparation for rice, by treatment status

	(1)	(2)	(3)	(4)	(5)
	Plows the field twice	Waits a week between two rounds of plowing	Waits a week for between last plowing and broadcast ing	Prepares rice 2-3 days in advance	Soaks the seeds in water before drying
Treatment arm: Basic	-0.015	-0.034	-0.003	0.029	0.027
	(0.013)	(0.019)*	(0.008)	(0.032)	(0.040)
Treatment arm: Enhanced	0.006	-0.028	-0.007	0.048	0.044
	(0.014)	(0.018)	(0.009)	(0.030)	(0.037)
Treatment arm: Enhanced plus	-0.012	-0.013	-0.005	0.054	-0.033
	(0.011)	(0.019)	(0.008)	(0.030)*	(0.034)
CEW fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	1,687	1,687	1,687	1,687	1,687
R-squared	0.026	0.045	0.015	0.124	0.197
Mean in Control Group	0.0355	0.0804	0.0213	0.298	0.260

Note: Standard errors are clustered at the village level. ***, **, and * indicate significance at 1, 5 and 10 percent respectively. Baseline controls include household head's age, gender, whether the head is literate, and household's non-agricultural wage income.

We next examine variables related to both weed removal and an initial set of variables related to fertilizer use for rice (Table 42). In this context, we find two significant coefficients, both among the enhanced group; the enhanced group appears to become more likely to apply fertilizer to their rice fields. Note that the control group mean is much lower among these farmers than those from the PADEE sample, so there is more scope for improvement in fertilizer use among the farmers in this sample. The point estimate suggests that in the enhanced group the share of farmers applying fertilizer on rice increased by 12 percentage points. It is somewhat curious that the enhanced plus treatment did not have the same effect as the enhanced group (4 percentage points, and it is not statistically different from zero), given the larger saturation of calls in those villages.

Table 42. Impacts of direct calls to ASPIRE groups on variables related to weeding and basic fertilizer use for rice, by treatment status

	(1)	(2)	(3)	(4)	(5)
	Soaks the seeds in hot water before drying	Removes weeds at least once during the season	Removes weeds regularly during the season (4X or more)	Applies fertilizer for rice cultivation	Applies fertilizer when the land is wet
Treatment arm: Basic	0.004	0.077	0.068	0.047	0.044
	(0.005)	(0.035)**	(0.031)**	(0.038)	(0.038)
Treatment arm: Enhanced	-0.002	0.066	0.062	0.127	0.106
	(0.004)	(0.035)*	(0.029)**	(0.033)***	(0.035)***
Treatment arm: Enhanced plus	-0.001	0.001	-0.006	0.044	0.049
	(0.004)	(0.040)	(0.028)	(0.037)	(0.038)
CEW fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	1,687	1,687	1,687	1,687	1,687
R-squared	0.023	0.132	0.086	0.102	0.095
Mean in Control Group	0.00709	0.402	0.182	0.721	0.707

Note: Standard errors are clustered at the village level. ***, **, and * indicate significance at 1, 5 and 10 percent respectively. Baseline controls include household head's age, gender, whether the head is literate, and household's non-agricultural wage income.

Last in this category, we examine the impacts on two more variables related to fertilizer use, specifically urea, and the use of pesticides (Table 43). As with the previous variables, we find some significant coefficients among the enhanced message treatment group, but no others. The enhanced message group is 15 percentage points more likely, for example, to apply pesticides for rice cultivation, and they appear more likely to use protective gear when applying pesticides. Households in the enhanced group become 13 percentage points more likely to apply urea, and 15 percentage points more likely to apply pesticides, over control means of 68 and 63 percent, respectively. Coefficients for the other two groups are not statistically different from zero. If anything, the enhanced messages appear to have improved knowledge of techniques, but surprisingly the same is not true among the enhanced plus group.

Table 43. Impacts of direct calls to ASPIRE groups on variables related to urea and pesticide use for rice, by treatment status

	(1)	(2)	(3)	(4)	(5)	(6)
	Applies urea for rice cultivation	Applies urea when the field is dry	Applies pesticide for rice cultivation	Applies pesticide when the field is dry	Applies pesticide around midday	Uses protective gear while applying pesticide
Treatment arm:	0.051	-0.007	0.054	0.003	0.019	0.016
Basic	(0.040)	(0.004)*	(0.035)	(0.010)	(0.011)*	(0.036)
Treatment arm:	0.133	-0.005	0.155	0.005	0.020	0.113
Enhanced	(0.037)***	(0.004)	(0.037)***	(0.010)	(0.012)*	(0.037)***
Treatment arm:	0.053	-0.007	0.049	0.003	-0.005	0.027
Enhanced plus	(0.037)	(0.004)*	(0.032)	(0.010)	(0.009)	(0.030)
CEW fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,687	1,687	1,687	1,687	1,687	1,687
R-squared	0.126	0.019	0.243	0.040	0.054	0.166
Control Group Mean	0.681	0.00709	0.631	0.0331	0.0260	0.508

Note: Standard errors are clustered at the village level. ***, **, and * indicate significance at 1, 5 and 10 percent respectively. Baseline controls include household head's age, gender, whether the head is literate, and household's non-agricultural wage income.

Next, we examine 17 practices related to chicken rearing that were covered in the chicken messages. Recall, only 500 households received messages, so we again might only expect to find significant coefficients among the treated groups. To ensure sample balance, for variables that are conditional we fill in zeroes where appropriate. For example, if households did not face a disease outbreak, we code the “consulted vet” variable as zero.

We begin by examining 5 variables related to disease prevention among chickens (Table 44). Note that we asked about the previous 12 months, which includes a few months before the messages were sent. We examine whether or not chickens were vaccinated, whether the farmer faced a disease outbreak in the past twelve months, and then steps that were taken to deal with outbreaks in columns 3 through 5. We find no significant differences between treatment groups and the control group. The sixth column examines a variable measuring whether farmers reported spreading compost to grow or attract insects for chickens; we find a statistically significant and negative coefficient, but it is quite small (1.6 percentage points).

Table 44. Impacts of direct calls to ASPIRE groups on variables related to chicken disease prevention, by treatment status

	(1)	(2)	(3)	(4)	(5)	(6)
	Vaccinated chicken regularly in past 12 months	Faced disease outbreak in past 12 months	Identifies the disease or disorder faced	Consulted vet or bought medicine from vet for treatment	Took no steps to treat disease	Spread compost in area to grow insects for chicken
Treatment arm: Basic	-0.015	-0.024	-0.004	-0.038	0.022	-0.009
	(0.020)	(0.034)	(0.035)	(0.025)	(0.031)	(0.008)
Treatment arm: Enhanced	-0.011	-0.045	-0.024	-0.039	-0.006	-0.006
	(0.018)	(0.029)	(0.032)	(0.027)	(0.027)	(0.008)
Treatment arm: Enhanced plus	-0.010	-0.005	0.044	-0.005	-0.013	-0.016
	(0.017)	(0.031)	(0.032)	(0.028)	(0.030)	(0.007)**
CEW fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,687	1,687	1,687	1,687	1,687	1,687
R-squared	0.029	0.020	0.022	0.029	0.028	0.030
Control Group Mean	0.0686	0.676	0.553	0.175	0.348	0.0284

Note: Standard errors are clustered at the village level. ***, **, and * indicate significance at 1, 5 and 10 percent respectively. Baseline controls include household head's age, gender, whether the head is literate, and household's non-agricultural wage income.

Next, we consider variables related to the way that chickens are kept, information about breeding cocks, and the number of eggs being generated by laying hens (Table 45). We find negative coefficients for the enhanced treatment group only on the variable measuring whether or not rice husks or the like are used on the chicken house floor to reduce the spread of potential disease and to ease cleaning; very few households use this technique regardless of the negative coefficients.

Table 45. Impacts of direct calls to ASPIRE groups on variables related to chicken rearing procedures, by treatment status

	(1)	(2)	(3)	(4)	(5)	(6)
	Has chicken house	Uses rice-hust/saw dust/hay on the chicken house floor	Has cocks weight 2 kg or larger	Knows a breeding cock should weigh 2 kg or more	# of eggs per generation the newest hen lays	# of eggs per generation the oldest hen lays
Treatment arm:	-0.034	-0.016	-0.014	-0.025	0.150	0.049
Basic	(0.034)	(0.010)	(0.034)	(0.033)	(0.325)	(0.376)
Treatment arm:	-0.009	-0.033	-0.012	-0.005	0.063	0.039
Enhanced	(0.028)	(0.009)***	(0.028)	(0.027)	(0.271)	(0.344)
Treatment arm:	-0.013	-0.028	-0.015	0.017	0.373	0.207
Enhanced plus	(0.028)	(0.010)***	(0.031)	(0.027)	(0.244)	(0.287)
CEW fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,687	1,687	1,687	1,687	1,687	1,687
R-squared	0.051	0.027	0.041	0.047	0.049	0.049
Control Group Mean	0.749	0.0567	0.655	0.778	8.123	9.286

Note: Standard errors are clustered at the village level. ***, **, and * indicate significance at 1, 5 and 10 percent respectively. Baseline controls include household head's age, gender, whether the head is literate, and household's non-agricultural wage income.

Finally, we examine a set of variables that measures chick selection and feeding practices (Table 46). Once again, we find no significant differences between groups, with one exception which is only marginally significant. So whereas there are some interesting coefficients related to rice practices, we find little related to chicken rearing.

Table 46. Impacts of direct calls to ASPIRE groups on variables related to further chicken rearing procedures, by treatment status

	(1)	(2)	(3)	(4)	(5)
	# of eggs per generation a good hen for breeding lays	Selects chick for raising	Makes own chicken feed	Buys commercial feed	Feeds only commercial feed to chicks below 14 days of age
Treatment arm: Basic	-0.076	-0.031	-0.025	-0.011	-0.027
	(0.420)	(0.035)	(0.020)	(0.036)	(0.033)
Treatment arm: Enhanced	-0.029	0.006	-0.036	-0.035	-0.044
	(0.375)	(0.030)	(0.019)*	(0.031)	(0.032)
Treatment arm: Enhanced plus	0.257	0.000	-0.047	-0.047	-0.068
	(0.329)	(0.025)	(0.022)**	(0.033)	(0.033)**
CEW fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	1,687	1,687	1,687	1,687	1,687
R-squared	0.069	0.041	0.059	0.065	0.065
Control Group Mean	10.61	0.643	0.199	0.362	0.288

Note: Standard errors are clustered at the village level. ***, **, and * indicate significance at 1, 5 and 10 percent respectively. Baseline controls include household head's age, gender, whether the head is literate, and household's non-agricultural wage income.

We then examine impacts on 10 variables related to vegetable production. First, we examine variables related to seedling production, watering vegetables, and fertilizer (Table 47). We find that the basic and enhanced plus treatment arms appear to have led to more seedling production, though the number of farmers underlying the treatment effects are fairly small. Other coefficeints are not significantly different from zero.

Table 47. Impacts of direct calls to ASPIRE groups on variables related to growing vegetables, by treatment status

	(1)	(2)	(3)	(4)	(5)
	Produced seedlings for vegetables in past 12 months	Produced seedlings for cucumber/long beans in past 12 months	Waters vegetables in the morning	Applies fertilizers for vegetables	Identifies any disease that vegetables suffered from last season
Treatment arm:	0.027	0.029	-0.000	-0.030	-0.011
Basic	(0.016)*	(0.010)***	(0.026)	(0.025)	(0.029)
Treatment arm:	0.003	0.003	-0.018	-0.029	-0.037
Enhanced	(0.018)	(0.008)	(0.026)	(0.028)	(0.028)
Treatment arm:	0.017	0.019	0.005	-0.006	-0.007
Enhanced plus	(0.015)	(0.009)**	(0.031)	(0.031)	(0.033)
CEW fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	1,687	1,687	1,687	1,687	1,687
R-squared	0.053	0.025	0.030	0.085	0.028
Control Group Mean	0.0544	0.0118	0.220	0.213	0.241

Note: Standard errors are clustered at the village level. ***, **, and * indicate significance at 1, 5 and 10 percent respectively. Baseline controls include household head's age, gender, whether the head is literate, and household's non-agricultural wage income.

With the next set of variables, which are related to disease treatment or prevention, we again find only one significant coefficient, for not taking action at all to deal with a disease; again, it is not significant for the enhanced group, but is for the basic and the enhanced plus groups (Table 48). Note here that the negative coefficient implies households became more active in dealing with diseases. However, again the coefficient is quite small in relative terms, so it may not reflect a large number of households changing behavior.

Table 48. Impacts of direct calls to ASPIRE groups on disease prevention when growing vegetables, by treatment status

	(1) Used chemicals for disease treatment/prevention	(2) Used other preventative /curative steps for diseases	(3) Took no action for disease prevention or cure	(4) Knows of thrip	(5) Knows one or more preventative /curative step for thrip
Treatment arm:	0.005	-0.004	-0.049	-0.031	-0.006
Basic	(0.022)	(0.017)	(0.019)**	(0.016)*	(0.010)
Treatment arm:	-0.013	-0.022	-0.019	-0.028	0.002
Enhanced	(0.024)	(0.018)	(0.016)	(0.017)	(0.011)
Treatment arm:	-0.001	-0.001	-0.044	0.006	0.013
Enhanced plus	(0.028)	(0.019)	(0.020)**	(0.025)	(0.014)
CEW fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	1,687	1,687	1,687	1,687	1,687
R-squared	0.061	0.021	0.036	0.040	0.053
Control Group Mean	0.125	0.102	0.156	0.0875	0.0378

Note: Standard errors are clustered at the village level. ***, **, and * indicate significance at 1, 5 and 10 percent respectively. Baseline controls include household head's age, gender, whether the head is literate, and household's non-agricultural wage income.

In sum, we find that the farmer knowledge base appears to have changed due to the phone calls, but the most meaningful changes in knowledge or practices occurred with respect to rice production. There are some suggestive changes for vegetable production, but they are likely too small in magnitude to place much stock in. We next turn to changes in input use.

7.6.4 Impacts on Input Use

We focus on input use in rice production, as it is challenging to break out input use for chicken rearing and vegetable production. First, we find that there were positive impacts of the enhanced treatment on improved seed, NPK, and pesticide use (Table 49). These findings are consistent with findings from the previous set of regressions for rice production, which also highlighted the enhanced group relative to the other two groups. The treatment effect is somewhat small for improved seed, but reasonable (more than 7 percentage points) for NPK use, representing slightly more than a 10 percent increase over the control group.

Table 49. Impacts of direct calls to ASPIRE groups on input use in rice production, by treatment status

	(1)	(2)	(3)	(4)	(5)
	Used improved rice seed in rice cultivation	Used NPK in rice cultivation	Used urea in rice cultivation	Used compost in rice cultivation	Used pesticide in rice cultivation
Treatment arm: Basic	-0.010	0.000	0.009	-0.010	0.007
	(0.016)	(0.019)	(0.029)	(0.004)**	(0.023)
Treatment arm: Enhanced	0.032	0.071	0.044	-0.007	0.071
	(0.014)**	(0.016)***	(0.025)*	(0.004)	(0.026)***
Treatment arm: Enhanced plus	0.001	0.025	0.018	-0.000	0.003
	(0.014)	(0.019)	(0.024)	(0.005)	(0.019)
CEW fixed effects	Yes	Yes	Yes	Yes	Yes
Control for outcome at baseline	Yes	Yes	Yes	Yes	Yes
Observations	1,687	1,687	1,687	1,687	1,687
R-squared	0.601	0.471	0.492	0.018	0.558
Mean in Control Group	0.780	0.655	0.631	0.00946	0.612

Note: Standard errors are clustered at the village level. ***, **, and * indicate significance at 1, 5 and 10 percent respectively. Baseline controls include household head's age, gender, whether the head is literate, and household's non-agricultural wage income.

There are also some significant findings, among the same group, on the intensive margin (Table 50). Specifically, we find that the enhanced group appears to put more NPK on their rice per hectare. The mean here among the control group is much lower than in the PADEE sample, so the additional fertilizer should be helpful in this range; the coefficient is suggestive of a 20 percent increase in NPK use.[24] There are no significant effects for the other two groups on chemical fertilizer. Both the basic and enhanced groups appear to have negative effects on compost usage but note that compost usage per hectare is quite low in the sample in general.

[24] Note we do not find the same with expenditures, which are in Appendix Table C.4.

Table 50. Impacts of direct calls to ASPIRE groups on input use in rice production, by treatment status

	(1)	(2)	(3)	(4)	(5)	(6)
	Quantity of NPK (Kg) applied on rice	Quantity of urea (Kg) applied on rice	Quantity of compost (Kg) applied on rice	Quantity of NPK applied per ha of land (Kg/ha), plot mean wet season	Quantity of urea applied per ha of land (Kg/ha), plot mean wet season	Quantity of compost applied per ha of land (Kg/ha), plot mean wet season
Treatment arm:	-23.357	1.449	-4.859	3.843	4.520	-1.356
Basic	(24.615)	(18.399)	(3.465)	(3.283)	(3.540)	(0.717)*
Treatment arm:	60.367	59.850	-4.648	15.139	6.640	-1.345
Enhanced	(34.497)*	(22.702)**	(3.337)	(3.860)***	(3.767)*	(0.712)*
Treatment arm:	3.337	49.511	4.744	3.069	4.272	0.793
Enhanced plus	(28.723)	(23.640)**	(5.798)	(3.476)	(3.244)	(1.033)
CEW fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Control for outcome at baseline	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,687	1,687	1,687	1,687	1,687	1,687
R-squared	0.611	0.556	0.013	0.424	0.366	0.018
Control Group Mean	328.6	219	4.515	74.81	51.97	1.298

Note: Standard errors are clustered at the village level. ***, **, and * indicate significance at 1, 5 and 10 percent respectively. Baseline controls include household head's age, gender, whether the head is literate, and household's non-agricultural wage income.

7.6.5 Impacts on Production

We next examine impacts on total rice production (Table 51). First, we examine whether households became more likely to grow rice, and whether their total harvest changed. We find a small portion of the enhanced message group start cultivating rice (3.2 percentage points), but there are no production impacts for any of the groups. These results are consistent when we use the logarithm of yields as the dependent variable, shown in the appendix (Table C.6), or median regression, also in the appendix (Table C.7).

Table 51. Impacts of direct calls to ASPIRE groups on rice cultivation and production, by treatment status

	(1)	(2)	(3)	(4)
	HH cultivated rice	HH cultivated rice in wet season	Total rice harvest per hectare in wet season (kg/ha)	Total rice harvest per hectare (kg/ha)
Treatment: Basic	-0.010	-0.012	-76.390	-71.923
	(0.016)	(0.017)	(61.462)	(61.871)
Treatment: Enhanced	0.032	0.034	30.180	9.809
	(0.014)**	(0.014)**	(65.998)	(63.559)
Treatment: Enhanced plus	0.001	-0.001	58.346	44.185
	(0.014)	(0.015)	(79.334)	(78.919)
CEW fixed effects	Yes	Yes	Yes	Yes
Control for outcome at baseline	Yes	Yes	Yes	Yes
Observations	1,687	1,687	1,687	1,687
R-squared	0.601	0.554	0.542	0.551
Mean in Control Group	0.780	0.778	2147	2144

Note: Standard errors are clustered at the village level. ***, **, and * indicate significance at 1, 5 and 10 percent respectively. Baseline controls include household head's age, gender, whether the head is literate, and household's non-agricultural wage income.

We next examine impacts on the total sales during the wet season, and the total value of rice production (Table 52). We find positive coefficients on wet season sales, but no changes in production value, with the exception of negative coefficients in the basic message group that are marginally significant. Note that the sales are a small proportion of overall production for most households, and wet season production, which is the timing of this activity, accounts for most rice production in terms of value.[25]

[25] We include regressions of rice productivity on the treatment variables in Appendix Table C.5; again, we do not find significant results.

Table 52. Impacts of direct calls to ASPIRE groups on rice sales and total rice production, by treatment status

	(1)	(2)	(3)
	Rice sales (0000 Riels) in wet season	Wet season rice production value (0000 Riels) per hectare	Total rice production value (0000 Riels) per hectare
Treatment: Basic	14.165	-11.596	-10.758
	(12.999)	(6.466)*	(6.338)*
Treatment: Enhanced	17.945	-0.640	-3.680
	(8.514)**	(6.698)	(6.497)
Treatment: Enhanced plus	23.854	6.628	4.306
	(10.741)**	(7.672)	(7.694)
CEW fixed effects	Yes	Yes	Yes
Control for outcome at baseline	Yes	Yes	Yes
Observations	1,687	1,687	1,687
R-squared	0.171	0.544	0.540
Mean in Control Group	76.60	206.1	204.5

Note: Standard errors are clustered at the village level. ***, **, and * indicate significance at 1, 5 and 10 percent respectively. Baseline controls include household head's age, gender, whether the head is literate, and household's non-agricultural wage income.

Next, we look at the value of poultry production (Table 53). We find households in the treatment groups are no more likely to have additional poultry varieties, to sell poultry, or to have additional production value or income from selling poultry, with the exception of the basic treatment group; however, this coefficient is marginally significant. These findings are relatively consistent with the previous results, which show that whereas some knowledge has increased production patterns may have not.

Table 53. Impacts of direct calls to ASPIRE groups on poultry production, by treatment status

	(1)	(2)	(3)	(4)
	Number of poultry varieties	Whether HH sell any poultry	Production Value from poultry (0000 Riels)	Income from selling poultry (0000 Riels)
Treatment: Basic	-0.001	-0.039	0.991	3.940
	(0.078)	(0.037)	(1.768)	(2.283)*
Treatment: Enhanced	-0.051	-0.044	0.329	-2.328
	(0.069)	(0.033)	(1.554)	(2.102)
Treatment: Enhanced plus	0.048	-0.044	1.152	0.006
	(0.059)	(0.031)	(1.296)	(1.940)
CEW fixed effects	Yes	Yes	Yes	Yes
Control for outcome at baseline	Yes	Yes	Yes	Yes
Observations	1,687	1,687	1,687	1,687
R-squared	0.171	0.217	0.201	0.152
Mean in Control Group	2.390	0.522	30.18	18.04

Note: Standard errors are clustered at the village level. ***, **, and * indicate significance at 1, 5 and 10 percent respectively. Baseline controls include household head's age, gender, whether the head is literate, and household's non-agricultural wage income.

Next, we look at the value of vegetable production (Table 54). As with the other two tables, we find no significant impacts on vegetable production. Note that we might not be surprised by this result; we are aggregating far more than just cucumbers and long beans, and the intervention did not affect all households in the treatment groups.

Table 54. Impacts of direct calls to ASPIRE groups on vegetable production, by treatment status

	(1)	(2)	(3)	(4)
	HH cultivated vegetable	Number of vegetables HH grow	Number of vegetables HH sell	Vegetable Production Value (0000 Riels)
Treatment: Basic	-0.024	-0.054	-0.016	4.158
	(0.032)	(0.106)	(0.069)	(11.265)
Treatment: Enhanced	-0.034	-0.078	-0.079	5.859
	(0.029)	(0.099)	(0.071)	(11.548)
Treatment: Enhanced plus	-0.016	-0.033	-0.003	23.733
	(0.034)	(0.112)	(0.067)	(22.417)
CEW fixed effects	Yes	Yes	Yes	Yes
Control for outcome at baseline	Yes	Yes	Yes	Yes
Observations	1,687	1,687	1,687	1,687
R-squared	0.166	0.204	0.189	0.222
Mean in Control Group	0.364	1.054	0.518	30.64

Note: Standard errors are clustered at the village level. ***, **, and * indicate significance at 1, 5 and 10 percent respectively. Baseline controls include household head's age, gender, whether the head is literate, and household's non-agricultural wage income.

Finally, we examine the impacts on overall agricultural income on a per hectare basis (Table 55). We find no significant coefficients for the basic and enhanced treatment groups; however, the enhanced plus treatment group demonstrates significant coefficients at the 10 percent level. The magnitude on both would suggest an increase in net agricultural income of 15 percent above the control group, or around $83/hectare. However, none of the results leading up to this result are suggestive of impacts, nor do we find significant coefficients in either appendix table which use logarithms or median regression. So we are somewhat skeptical of this result suggesting average impacts over that group. We conclude that although there are some interesting effects on knowledge, knowledge sharing and fertilizer use, there do not appear to be any really robust impacts on productivity or net agricultural income.

Table 55. Impacts of direct calls to ASPIRE groups on total agricultural income, by treatment status

	(1)	(2)
	Agricultural net income per hectare (0000 riels/ha)	Agricultural gross income per hectare (0000 riels/ha)
Treatment: Basic	-15.074	-20.981
	(13.870)	(19.413)
Treatment: Enhanced	-16.932	5.910
	(13.180)	(17.011)
Treatment: Enhanced plus	34.169	31.782
	(14.111)**	(17.901)*
CEW fixed effects	Yes	Yes
Control for outcome at baseline	Yes	Yes
Observations	1,687	1,687
R-squared	0.519	0.307
Mean in Control Group	224	348.5

Note: Standard errors are clustered at the village level. ***, **, and * indicate significance at 1, 5 and 10 percent respectively. Baseline controls include household head's age, gender, whether the head is literate, and household's non-agricultural wage income.

7.7 ASPIRE Heterogeneity Analysis

To begin to explore heterogeneity, here we try to understand whether the experience of farmer group and non-farmer group members differed. We initially present the difference in the impact estimates for ASPIRE farmer group and the non-farmer group members. We examine whether households reported receipt of messages at different rates, to help us infer among whom gains were attained. For rice production messages, considering the interaction effects, we observe to significant differences between coefficients for farmer group members and non-farmer group members (Table 56). This result is quite sensible, as both should have received messages at approximately the same rate.

Table 56. Impacts of direct calls to ASPIRE groups on receipt of rice messages, by treatment status interacted with ASPIRE group membership status

	(1)	(2)	(3)	(4)	(5)	(6)
	Ever received voice message	Message received in September	Message received in October	Message received in November	Message received in December	Voice message helped increase production
Basic message	0.073	0.062	0.040	0.012	0.016	0.057
	(0.022)***	(0.016)***	(0.013)***	(0.008)	(0.011)	(0.018)***
Enhanced message	0.116	0.066	0.053	0.041	0.037	0.096
	(0.029)***	(0.019)***	(0.016)***	(0.015)***	(0.014)**	(0.024)***
Enhanced plus message	0.123	0.072	0.071	0.035	0.044	0.099
	(0.023)***	(0.013)***	(0.016)***	(0.013)**	(0.016)***	(0.018)***
Belongs to ASPIRE	0.016	0.005	0.021	0.016	0.002	0.014
	(0.014)	(0.012)	(0.013)	(0.009)*	(0.007)	(0.010)
Basic X Belongs	0.004	-0.028	-0.030	0.004	0.011	-0.002
to ASPIRE	(0.035)	(0.024)	(0.025)	(0.021)	(0.018)	(0.024)
Enhanced X Belongs	0.034	-0.002	-0.001	-0.005	-0.003	0.026
to ASPIRE	(0.046)	(0.034)	(0.024)	(0.017)	(0.020)	(0.040)
Enhanced+ X Belongs	0.062	0.007	-0.011	0.055	0.033	0.041
to ASPIRE	(0.047)	(0.028)	(0.027)	(0.034)	(0.032)	(0.036)
CEW fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,687	1,687	1,687	1,687	1,687	1,687
R-squared	0.063	0.035	0.031	0.039	0.031	0.055
Mean in Control Group	0.0213	0.00709	0.0142	0.0118	0.00946	0.0118

Note: Standard errors are clustered at the village level. ***, **, and * indicate significance at 1, 5 and 10 percent respectively. Baseline controls include household head's age, gender, literacy status, and non agricultural wage income level.

We find a slightly differernt story for chicken messages (Table 57). Whereas impact coefficients are positive for all treatment groups, the farmers in the enhanced plus treatment group have higher magnitudes among ASPIRE farmer group members; impact estimates double for the probability of ever receiving a message. The base coefficient of 8.7 percentage points increases by 9.8 percentage points among those in the ASPIRE farmer group. It could be that these messages were more salient among ASPIRE farmer group members because they are more likely to actually keep chickens than non-farmer group members. Farmers in the ASPIRE group are much more likely to report the messages helped increase their production; the basic coefficient is 4.6 percentage points and increases by 10.6 percentage points for the ASPIRE group. This result implies that farmer group members within that specific treatment group found these particular messages more useful than non-farmer group members, again likely because farmer group members were more likely to raise chickens.

Table 57. Impacts of direct calls to ASPIRE groups on receipt of chicken messages, by treatment status interacted with ASPIRE group membership status

	(1)	(2)	(3)	(4)	(5)
	Ever received voice message	Message received in September	Message received in October	Message received in November	Voice message helped increase production
Basic message	0.057	0.032	0.044	0.023	0.044
	(0.022)**	(0.010)***	(0.013)***	(0.010)**	(0.018)**
Enhanced message	0.123	0.041	0.040	0.036	0.073
	(0.030)***	(0.014)***	(0.013)***	(0.012)***	(0.021)***
Enhanced plus message	0.087	0.039	0.051	0.051	0.046
	(0.022)***	(0.011)***	(0.014)***	(0.011)***	(0.016)***
Belongs to ASPIRE	0.014	0.006	0.016	0.010	0.014
	(0.019)	(0.012)	(0.012)	(0.008)	(0.013)
Basic X Belongs to ASPIRE	0.009	-0.011	-0.018	-0.025	0.006
	(0.031)	(0.021)	(0.027)	(0.012)*	(0.027)
Enhanced X Belongs to ASPIRE	0.002	0.016	0.005	-0.012	0.012
	(0.041)	(0.029)	(0.028)	(0.020)	(0.030)
Enhanced+ X Belongs to ASPIRE	0.098	-0.007	-0.019	-0.000	0.106
	(0.039)**	(0.022)	(0.026)	(0.021)	(0.036)***
CEW fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	1,687	1,687	1,687	1,687	1,687
R-squared	0.054	0.024	0.018	0.025	0.050
Mean in Control Group	0.0236	0.00709	0.0118	0.00946	0.0189

Note: Standard errors are clustered at the village level. ***, **, and * indicate significance at 1, 5 and 10 percent respectively. Baseline controls include household head's age, gender, literacy status, and non agricultural wage income level.

We examine treatment interacted with group status for vegetable messages (Table 58). We find fewer significant effects and no significant differences within treatment groups for cucumber (panel A) and some primarily marginal statistically significant differences among farmer groups for long beans (panel B). We find significant coefficients among farmer group members in the basic treatment arm, implying that farmer group members within this treatment group found the messages more useful than non-farmer group members; other coefficients are positive but not statistically significant. Except for rice messages, it seems that farmer group members found the messages more useful. A speculative rationale might be that some of these farmers are already trying to improve their farming, and that some of these groups actually chose to work on chicken or vegetable farming, so they are better placed to find additional information useful.

Table 58. Impacts of direct calls to ASPIRE groups on receipt of cucumber and long bean messages, by treatment status interacted with ASPIRE group membership status

	(1)	(2)	(3)	(4)	(5)
	Ever received voice message	Message received in October	Message received in November	Message received in December	Voice message helped increase production
Panel A: Cucumber					
Basic message	0.037	0.024	0.012	0.003	-0.004
	(0.017)**	(0.012)**	(0.010)	(0.009)	(0.005)
Enhanced message	0.025	0.012	0.008	0.008	0.008
	(0.019)	(0.012)	(0.010)	(0.009)	(0.007)
Enhanced plus message	0.049	0.015	0.011	0.011	0.020
	(0.021)**	(0.010)	(0.012)	(0.011)	(0.010)*
Belongs to ASPIRE	0.004	0.007	0.007	0.002	-0.004
	(0.014)	(0.012)	(0.006)	(0.007)	(0.004)
Basic X Belongs	0.001	-0.013	-0.019	0.007	0.015
to ASPIRE	(0.025)	(0.025)	(0.014)	(0.015)	(0.009)*
Enhanced X Belongs	0.038	0.023	0.004	0.003	0.015
to ASPIRE	(0.027)	(0.022)	(0.013)	(0.016)	(0.012)
Enhanced+ X Belongs	0.025	-0.004	0.010	0.010	0.008
to ASPIRE	(0.032)	(0.019)	(0.018)	(0.019)	(0.018)
CEW fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	1,687	1,687	1,687	1,687	1,687
R-squared	0.038	0.024	0.022	0.012	0.017
Mean in Control Group	0.0189	0.0118	0.0118	0.00946	0.00236
Panel B: Long Beans					
Basic message	0.034	0.017	0.016	0.012	-0.004
	(0.013)**	(0.007)**	(0.011)	(0.009)	(0.006)
Enhanced message	0.022	0.005	0.000	-0.004	0.001
	(0.015)	(0.005)	(0.010)	(0.008)	(0.007)
Enhanced plus message	0.045	0.025	0.003	0.011	0.013
	(0.013)***	(0.010)**	(0.010)	(0.011)	(0.010)
Belongs to ASPIRE	0.001	0.010	0.002	-0.004	-0.005
	(0.015)	(0.011)	(0.014)	(0.011)	(0.004)
Basic X Belongs	0.033	-0.011	0.000	-0.013	0.031
to ASPIRE	(0.027)	(0.018)	(0.022)	(0.015)	(0.011)***
Enhanced X Belongs	0.035	0.013	0.023	0.011	0.027
to ASPIRE	(0.029)	(0.018)	(0.021)	(0.012)	(0.015)*
Enhanced+ X Belongs	0.033	-0.007	-0.004	0.034	0.014
to ASPIRE	(0.023)	(0.020)	(0.018)	(0.019)*	(0.016)
CEW fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	1,687	1,687	1,687	1,687	1,687
R-squared	0.026	0.017	0.014	0.030	0.026
Mean in Control Group	0.00946	0.00473	0.00946	0.00709	0.00236

Note: Standard errors are clustered at the village level. ***, **, and * indicate significance at 1, 5 and 10 percent respectively. Baseline controls include household head's age, gender, literacy status, and non agricultural wage income level.

We next disaggregate the impact estimates for the outcomes related to social networks (panel A) and knowledge sharing within these networks (panel B) for the rice messages (Table 59). The estimates do not suggest large differences among farmer ASPIRE farmer group members. The enhanced group is more likely to share messages with other farmers in the village while the enhanced plus treatment group is more likely to know people in the village who received the messages; consistent with the research design increasing the availability of information in those villages by increasing the number of people who were sent messages.

Table 59. Impacts of direct calls to ASPIRE groups on receiving and sharing advice about rice messages, by treatment status interacted with ASPIRE group membership status

	(1)	(2)	(3)	(4)	(5)
	Shared advice from the message with other farmers/ persons	# of people inside village with whom farmer shared advice	# of people outside village with whom farmer shared advice		
Panel A: Social Networks					
Basic message	0.021	0.053	-0.000		
	(0.014)	(0.076)	(0.028)		
Enhanced message	0.054	0.259	0.025		
	(0.018)***	(0.123)**	(0.034)		
Enhanced plus message	0.028	0.120	0.028		
	(0.013)**	(0.082)	(0.033)		
Belongs to ASPIRE	0.006	-0.023	0.055		
	(0.009)	(0.053)	(0.057)		
Basic X Belongs to ASPIRE	-0.004	-0.012	-0.048		
	(0.021)	(0.077)	(0.056)		
Enhanced X Belongs to ASPIRE	0.009	0.331	-0.008		
	(0.027)	(0.386)	(0.085)		
Enhanced+ X Belongs to ASPIRE	0.022	0.507	0.116		
	(0.025)	(0.280)*	(0.146)		
CEW fixed effects	Yes	Yes	Yes		
Observations	1,687	1,687	1,687		
R-squared	0.030	0.023	0.023		
Mean in Control Group	0.00709	0.0355	0.0236		
Panel B: Knowledge Sharing					
	Knows someone who received message	# of people known who received message	Learned techniques from others who received message	Shared techniques learned from others	# of HH with whom the farmer discussed improvement in production
Basic message	0.003	0.011	-0.001	0.004	-0.126

	(0.009)	(0.020)	(0.008)	(0.005)	(1.264)
Enhanced message	0.023	0.045	0.012	0.004	3.447
	(0.013)*	(0.029)	(0.013)	(0.005)	(1.916)*
Enhanced plus message	0.035 (0.016)**	0.082 (0.039)**	0.007 (0.011)	0.008 (0.007)	3.017 (1.594)*
Belongs to ASPIRE	-0.003	-0.002	-0.009	-0.000	2.992
	(0.008)	(0.021)	(0.005)	(0.000)	(1.678)*
Basic X Belongs to ASPIRE	0.025 (0.016)	0.056 (0.045)	0.011 (0.012)	0.002 (0.007)	-0.369 (1.975)
Enhanced X Belongs to ASPIRE	0.061 (0.027)**	0.171 (0.090)*	0.028 (0.015)*	0.002 (0.007)	0.902 (2.864)
Enhanced+ X Belongs to ASPIRE	0.044 (0.028)	0.121 (0.096)	0.043 (0.020)**	0.015 (0.016)	-3.134 (2.773)
CEW fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	1,687	1,687	1,687	1,687	1,687
R-squared	0.039	0.029	0.025	0.016	0.065
Mean in Control Group	0.00709	0.0165	0.00473	0	6.792

Note: Standard errors are clustered at the village level. ***, **, and * indicate significance at 1, 5 and 10 percent respectively. Baseline controls include household head's age, gender, literacy status, and non agricultural wage income level.

We present similar tables for the same effects on chicken messaging in Table 60 , for cucumbers in Table 61, and long beans in Table 62. For these other products we do not find consistent evidence that farmers that belong to the ASPIRE farmer groups are more likely to share the information received thorough the voice messages. In the estimates suggest higher probability of knowing farmers who received the messages among the farmers in the ASPIRE groups for chicken; these effects are smaller for the vegetables regression and not statistically significant at conventional levels.[26]

Table 60. Impacts of direct calls to ASPIRE groups on receiving and sharing advice about chicken messages, by treatment status interacted with ASPIRE group membership status

	(1)	(2)	(3)	(4)	(5)
	Shared advice from the message with other farmers/ persons	# of people inside village with whom farmer shared advice	# of people outside village with whom farmer shared advice		
Panel A: Social Networks					
Basic message	0.013	0.262	0.129		
	(0.011)	(0.237)	(0.112)		
Enhanced message	0.029	0.115	0.028		
	(0.017)	(0.129)	(0.034)		

[26] In excluded heterogeneity regressions, we explored how the impact for these measures would change with depending on how far the villages were from population centers with 25, 000 and above. Those results did not suggest that relative isolation affected the probability to receive or share a message among farmers in the community. In the interest of brevity, we omit those results and provide this description.

Enhanced plus message	0.008	0.001	0.010
	(0.010)	(0.093)	(0.031)
Belongs to ASPIRE	-0.003	-0.071	-0.007
	(0.011)	(0.068)	(0.007)
Basic X Belongs to ASPIRE	0.010	-0.107	-0.098
	(0.020)	(0.300)	(0.133)
Enhanced X Belongs to ASPIRE	0.005	0.364	0.064
	(0.021)	(0.378)	(0.062)
Enhanced+ X Belongs to ASPIRE	0.055	0.501	0.181
	(0.023)**	(0.201)**	(0.117)
CEW fixed effects	Yes	Yes	Yes
Observations	1,687	1,687	1,687
R-squared	0.025	0.017	0.015
Mean in Control Group	0.00709	0.0378	0

Panel B: Knowledge Sharing

	Knows someone who received message	# of people known who received message	Learned techniques from others who received message	Shared techniques learned from others	# of HH with whom the farmer discussed improvement in product
Basic message	0.007	0.009	-0.005	-0.004	-0.265
	(0.006)	(0.020)	(0.004)	(0.003)	(0.907)
Enhanced message	0.015	0.019	0.007	-0.004	1.375
	(0.010)	(0.031)	(0.008)	(0.003)	(0.923)
Enhanced plus message	0.019	0.043	0.003	-0.004	0.192
	(0.011)*	(0.032)	(0.006)	(0.003)	(1.206)
Belongs to ASPIRE	-0.004	-0.024	-0.004	-0.004	1.800
	(0.004)	(0.024)	(0.004)	(0.004)	(1.157)
Basic X Belongs to ASPIRE	0.031	0.059	0.021	0.009	-0.366
	(0.017)*	(0.038)	(0.010)**	(0.007)	(1.284)
Enhanced X Belongs to ASPIRE	0.018	0.053	0.003	0.004	-1.155
	(0.016)	(0.051)	(0.008)	(0.004)	(1.479)
Enhanced+ X Belongs to ASPIRE	0.025	0.029	0.024	0.010	-0.960
	(0.020)	(0.056)	(0.014)*	(0.007)	(1.688)
CEW fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	1,687	1,687	1,687	1,687	1,687
R-squared	0.025	0.020	0.019	0.021	0.063
Mean in Control Group	0.00236	0.0142	0.00236	0.00236	3.780

Note: Standard errors are clustered at the village level. ***, **, and * indicate significance at 1, 5 and 10 percent respectively. Baseline controls include household head's age, gender, literacy status, and non agricultural wage income level.

Table 61. Impacts of direct calls to ASPIRE groups on receiving and sharing advice about cucumber messages, by treatment status interacted with ASPIRE group membership status

	(1)	(2)	(3)	(4)
	Shared advice from the message with other farmers/ persons	# of people inside village with whom farmer shared advice	# of people outside village with whom farmer shared advice	
Panel A: Social Networks				
Basic message	-0.004	-0.012	-0.000	
	(0.005)	(0.024)	(0.017)	
Enhanced message	0.004	0.011	0.001	
	(0.007)	(0.027)	(0.016)	
Enhanced plus message	0.016	0.040	-0.000	
	(0.008)*	(0.035)	(0.017)	
Belongs to ASPIRE	0.007	0.008	-0.000	
	(0.009)	(0.023)	(0.001)	
Basic X Belongs to ASPIRE	0.010	0.034	0.011	
	(0.013)	(0.034)	(0.011)	
Enhanced X Belongs to ASPIRE	0.008	0.048	0.017	
	(0.015)	(0.049)	(0.016)	
Enhanced+ X Belongs to ASPIRE	-0.005	0.096	0.115	
	(0.014)	(0.125)	(0.114)	
CEW fixed effects	Yes	Yes	Yes	
Observations	1,687	1,687	1,687	
R-squared	0.016	0.018	0.015	
Mean in Control Group	0.00709	0.0165	0	
Panel B: Knowledge Sharing				
	Knows someone who received message	Number of people known who received message	Learned techniques from others who received message	Shared techniques learned from others
Basic message	-0.000	-0.012	-0.004	-0.004
	(0.005)	(0.023)	(0.004)	(0.003)
Enhanced message	0.004	-0.017	-0.000	-0.004
	(0.006)	(0.022)	(0.005)	(0.003)
Enhanced plus message	-0.000	-0.017	0.000	-0.004
	(0.005)	(0.022)	(0.005)	(0.003)
Belongs to ASPIRE	-0.004	-0.024	-0.004	-0.004
	(0.004)	(0.024)	(0.004)	(0.004)
Basic X Belongs to ASPIRE	0.022	0.040	0.015	0.009
	(0.014)	(0.033)	(0.009)*	(0.007)

Enhanced X Belongs to ASPIRE	0.012	0.038	0.005	0.004
	(0.012)	(0.028)	(0.008)	(0.004)
Enhanced+ X Belongs to ASPIRE	0.028	0.056	0.022	0.015
	(0.015)*	(0.035)	(0.012)*	(0.009)*
CEW fixed effects	Yes	Yes	Yes	Yes
Observations	1,687	1,687	1,687	1,687
R-squared	0.020	0.018	0.016	0.020
Mean in Control Group	0.00236	0.0142	0.00236	0.00236

Note: Standard errors are clustered at the village level. ***, **, and * indicate significance at 1, 5 and 10 percent respectively. Baseline controls include household head's age, gender, literacy status, and non agricultural wage income level.

Table 62. Impacts of direct calls to ASPIRE groups on receiving and sharing advice about long bean messages, by treatment status interacted with ASPIRE group membership status

	(1)	(2)	(3)	(4)
	Shared advice from the message with other farmers/ persons	# of people inside village with whom farmer shared advice	# of people outside village with whom farmer shared advice	
Panel A: Social Networks				
Basic message	-0.004	-0.022	-0.000	
	(0.005)	(0.032)	(0.017)	
Enhanced message	0.005	0.002	0.001	
	(0.007)	(0.031)	(0.016)	
Enhanced plus message	0.008	0.009	-0.000	
	(0.007)	(0.039)	(0.017)	
Belongs to ASPIRE	0.001	-0.018	-0.000	
	(0.007)	(0.023)	(0.001)	
Basic X Belongs to ASPIRE	0.026	0.172	0.011	
	(0.013)**	(0.114)	(0.011)	
Enhanced X Belongs to ASPIRE	0.013	0.074	0.017	
	(0.014)	(0.049)	(0.016)	
Enhanced+ X Belongs to ASPIRE	-0.002	0.104	0.115	
	(0.011)	(0.121)	(0.114)	
CEW fixed effects	Yes	Yes	Yes	
Observations	1,687	1,687	1,687	
R-squared	0.016	0.015	0.015	
Mean in Control Group	0.00473	0.0142	0	
Panel B: Knowledge Sharing				

	Knows someone who received message	Number of people known who received message	Learned techniques from others who received message	Shared techniques learned from others
Basic message	0.004	0.008	0.001	-0.004
	(0.007)	(0.017)	(0.005)	(0.003)
Enhanced message	-0.004	-0.013	-0.004	-0.004
	(0.004)	(0.011)	(0.004)	(0.003)
Enhanced plus message	0.004	0.011	0.005	0.000
	(0.006)	(0.017)	(0.006)	(0.005)
Belongs to ASPIRE	-0.004	-0.013	-0.004	-0.004
	(0.004)	(0.012)	(0.004)	(0.004)
Basic X Belongs to ASPIRE	0.012	0.019	0.011	0.009
	(0.014)	(0.030)	(0.010)	(0.007)
Enhanced X Belongs to ASPIRE	0.026	0.046	0.009	0.004
	(0.011)**	(0.020)**	(0.007)	(0.004)
Enhanced+ X Belongs to ASPIRE	0.013	0.010	0.012	0.005
	(0.012)	(0.025)	(0.012)	(0.008)
CEW fixed effects	Yes	Yes	Yes	Yes
Observations	1,687	1,687	1,687	1,687
R-squared	0.016	0.015	0.016	0.018
Mean in Control Group	0.00236	0.00709	0.00236	0.00236

Note: Standard errors are clustered at the village level. ***, **, and * indicate significance at 1, 5 and 10 percent respectively. Baseline controls include household head's age, gender, literacy status, and non agricultural wage income level.

7.8 Cost Analysis

While neither intervention was successful at raising rice yields or agricultural incomes in the short term, it is worthwhile considering the average and marginal costs of each intervention, as an addition to an extension intervention such as the expanded ASPIRE. It could also be that a variant of one of the two intervention models, as will be discussed in the next section, would act as a compliment to current extension activities within ASPIRE. Hence, we discuss the costs of implementing the two models on both an average basis and a marginal basis.

The ePADEE intervention was supposed to serve 800 farmers. There are several important cost categories borne by a project like ePADEE. First, there are fixed costs; we assume that the MSTs (or CEWs in an ASPIRE-type model) already have access to a motorbike to get to villages, and in this context we were able to use tablets already owned by MAFF. However, there is a maintenance cost to ensuring that tablet software is kept up-to-date.[27] The main costs from the implementation perspective are then soil testing for as many tests as take place, and all visits that take place by extension workers to villages, which include per diems for travel (accounting for fuel costs, etc.). For the purposes of this estimation, we will ignore any opportunity costs for farmers, in part because prevailing wage rates one can impute from the data either on or off the farm are relatively low and would add a small percentage to the overall costs.

First, consider a model with no bonuses; each MST should visit their village 5 times at 2 to 3 days per visit; in our meeting with MSTs, they argued that soil testing took longer, so 14 days were needed. We assume here we can cut to 12 visit-days per village. The prevailing per diem is $15/day; so each village requires $180 in what can be considered extension costs. We add $1 per farmer for soil testing, and we used Wing, a mobile money service, to transfer money to MSTs in three installments. Running a similar project alongside government (or with a private sector firm) would likely do the same, so we add $4.50 per village in costs ($1.50 per transfer). Finally, we add costs of maintaining software and data costs of uploading; in Cambodia, such costs should not be more than $5 per MST. In total, then, the cost per village, excluding any incentives or training costs (even refresher training costs), is around $209.50; this amount works out to about $10.48 per targeted farmer. We can consider $10.48 a low estimate of the average cost per farmer.

However, recall from our qualitative results that there were farmers within farmer groups who were not very interested in farming anymore; they were more interested in finding off-farm opportunities. If we very conservatively estimate that 25 percent of farmer group members are simply not interested in farming, any advice is effectively lost on them, and average costs rise to $13.97 per farmer. If that percentage is higher, then average costs rise even further, since it is not necessarily observable to the project which farmers would be interested in such an intervention.

Finally, let's consider marginal costs. In this case, as noted in our discussion MSTs already felt that they were working with too many farmers. So, an intervention in which more farmers would participate might actually require more MSTs. In other words, the marginal cost of an additional farmer is exceptionally high under the assumption that MSTs are working under full capacity, since another MST is required to serve another farmer. However, the next 19 farmers have a very low marginal cost (the soil test) since that MST would be working with another farmer group. So, we can consider that the marginal cost is approximately the same as the average cost above, assuming that the marginal concept can be extended to the marginal MST.

Next, we turn to the costs of sending messages through Viamo mobile. One-time costs included developing the messages and recording the messages as MP3s. There was an additional platform license ($750) and monthly platform management costs were paid

[27] Furthermore, extension workers either require good access to 3G connections (or better) or the ability to travel to a place with a good connection on a regular basis. In fact, connectivity to strong, reliable connections is an issue in the PADEE provinces, so whereas the programs were updated, for some MSTs it took several tries just to upload the data into the Grameen Intel server.

($1250/month).[28] Then Viamo charged a fee of $0.08 per minute for messages, billed by the second, and all Viamo costs included an administrative fee of 12%. Although these costs are relatively high for Cambodia (airtime costs are much lower, even across mobile companies), we will go forward assuming these costs, as the government is continuing to work with Viamo. This project ended up sending messages to approximately 1200 farmers, and we will only consider extending the enhanced model with the option to repeat messages, since it was frequently requested by farmers in follow-up phone calls. Opportunity costs to farmers are negligible since calls last less than two minutes and receipt of phone calls is free for all providers in Cambodia.

Since we used the platform for 5 months, we assume the total cost is $6720 for platform management. Taking the phone calls made and scaling them to all be enhanced, we would have spent $2389.93 on calls. Even if we add a repeat option and assume that 20 percent of farmers use it, the cost of calls would be $2867.91. Therefore, the average cost per farmer is $7.98, assuming the repeat option is added. So direct phone calls are substantially cheaper than the e-PADEE intervention, even though we have made assumptions inflating the average costs of the direct calls and minimizing the costs of the ePADEE intervention.

However, the marginal costs of adding farmers to the model are much lower. We've assumed that we make calls about 2 crops to each farmer above, and the platform management costs would be spread across more farmers if additional farmers were included. So marginal costs are $2.39 per farmer, using the number of minutes used by our project as a guide. Average costs per farmer simply decline as more farmers are added, since the monthly management costs can be spread over a larger number of farmers.

To give some context to these numbers, the ASPIRE budget for extension at the provincial level was $3.37 million in 2017. By the end of 2017, the project planned to be working with 198 farmer groups (or smallholder learning groups). If we assume an average of 25 members, which is relatively high, there would be a total of 4,950 beneficiaries; so total spending is presently projected at a very high $682 per beneficiary. This amount seems high and is affected by continuing capital expenditures to ramp up the project. However, even if the project was reaching something closer to the projected 60,000 beneficiaries in the initial five provinces, the spending per beneficiary remains at $56 on average. Therefore, the marginal cost of phone calls, under assumptions that likely inflated those costs, represent less than 5% of overall average annual expenditures by ASPIRE on direct extension.

In sum, the direct messaging intervention is a cheaper alternative on a per farmer basis, and its average costs decline as more farmers participate because the marginal costs are much lower than average costs. Further, set up costs are lower, as tablets and their maintenance are substantially more expensive than updating messages related to basic advice. Though neither intervention appears to have affected productivity in the short term, since farmers appear to like the direct call intervention it appears to have more potential for scaling throughout the project.

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[28] For this project, we negotiated a discount but we want to assume a scale up, so we assume a discount is not possible here.

8 Discussion

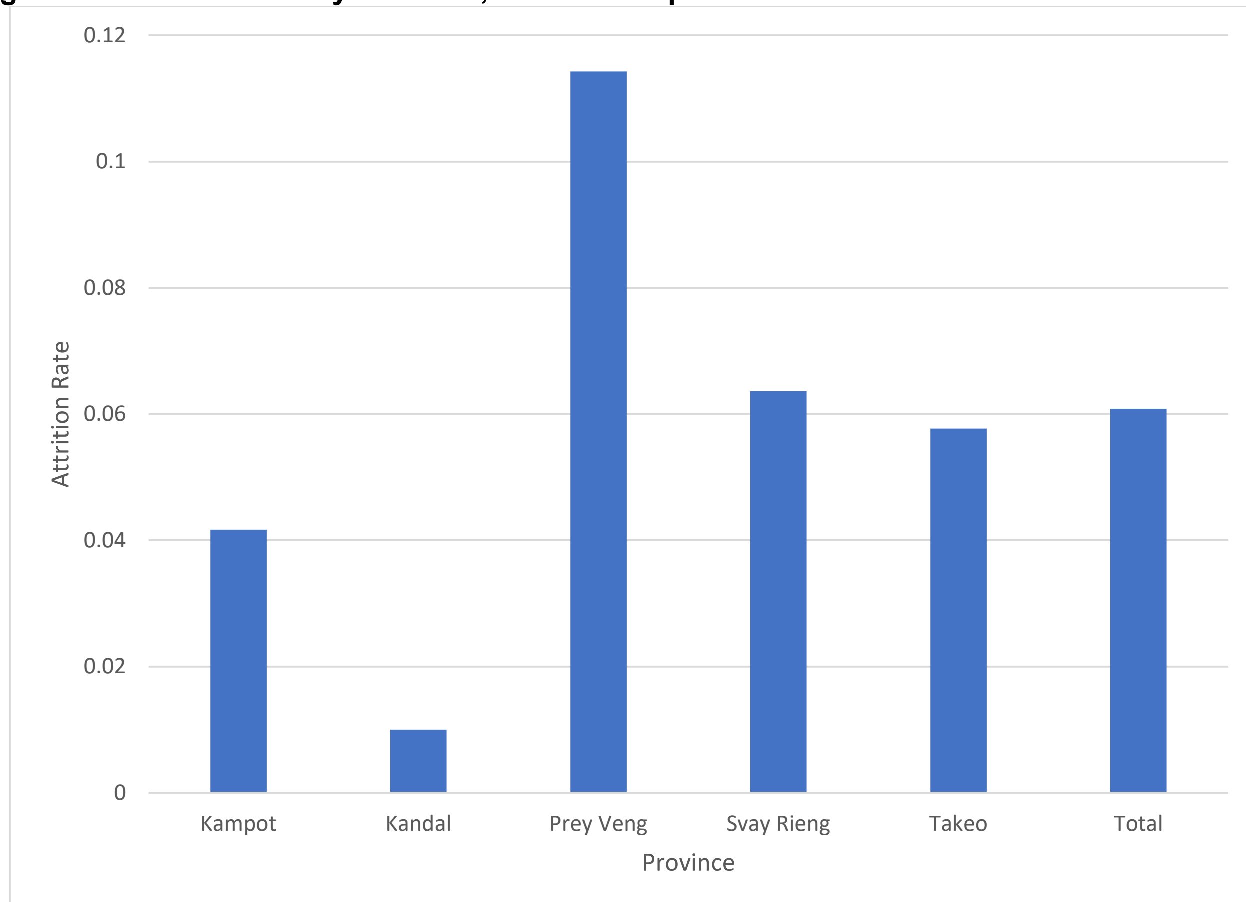
We focus our discussion first on understanding threats to the validity of our results, and then we include a further brief discussion of the results. First, we examine attrition in more detail in both the PADEE and ASPIRE contexts. Second, we discuss whether other agricultural programs in Cambodia might have existed on a scale that could have affected our results. Third, we discuss limitations of the PADEE and ASPIRE pilots, respectively. Finally, we summarize the results and contextualize them. Given that the control groups could not have known much about the intervention in either case, we are not concerned about John Henry effects; given that we do not find productivity impacts, and the treatment was cluster randomized, Hawthorne effects should not have any impact.

8.1 Attrition in PADEE

The data collection team was able to revisit 1127 of the original 1200 households in the baseline data set, implying that the overall attrition rate was 6.1 percent. We are particularly interested in whether the attrition is related to either membership in the ICT or ICT Plus groups; if so, one might be concerned that differential attrition might affect our results. Therefore, we first describe the attrition in more detail, and then regress an indicator for households that attrited on the two indicator variables for treatment status, a set of provincial dummy variables, and other household baseline characteristics.

First, we note that the attrition in the PADEE data set is not geographically balanced (Figure 15). Attrition was as low as 1 percent in Kandal, and as high as 11 percent, in Prey Veng province. In analyzing in collaboration with CDRI, there is quite a bit of seasonal migration from Prey Veng into Phnom Penh, which borders it. Other provinces have attrition rates similar to the overall average.

Figure 15. Attrition Rate by Province, PADEE Sample



Source: Authors' Calculations, PADEE data set.

Next, we examine attrition in a multivariate framework (

Table 63). Using a probit model, we regress a dummy variable for households that attrited on the two treatment group indicators (column 1), the treatment indicators and province indicators (column 2), and then the variables in column 2 and a set of baseline control variables (column 3). In all three cases, we find no correlation between attrition and either of the treatment indicators. As the attrition is not correlated with either of the treatments, regardless of the variables for which we control, we can conclude that attrition did not affect the PADEE analysis.

Table 63. Exploring Correlation between Attrition and Treatment Groups for PADEE intervention, Cambodia

	(1)	(2)	(3)
		Household attrited	
ICT	-0.066	-0.107	-0.136
	(0.177)	(0.150)	(0.154)
ICT plus	0.079	0.042	0.028
	(0.179)	(0.154)	(0.169)
Province dummies		Yes	Yes
Baseline controls			Yes
Observations	1,069	1,069	1,069
Pseudo R-squared	0.125	0.033	0.057
Mean in control group	145.9	101.2	1044

Note: Table reports coefficient from probit regression. Standard errors are clustered at the village level. ***, **, and * indicate significance at 1, 5 and 10 percent respectively. Baseline controls include household head's age, gender, dummies for education level, asset index, and household non-agricultural income.

8.2 Attrition in ASPIRE

The research team was somewhat concerned that the PADEE attrition rate was high. Therefore, IFPRI worked with CDRI prior to the collection of the ASPIRE baseline to improve tracking procedures throughout. The resulting attrition rate in the ASPIRE sample between 2017 and 2018 is much lower than in PADEE, at 2.4 percent, demonstrating that improved tracking procedures helped limit attrition.

Not surprisingly, the attrition rate does not appear to differ by treatment status, given the low overall rate. The treatment group specific attrition rates vary between 1.6 percent (control group) and 3.2 percent (basic messages). Clearly, this variation is not large enough to affect any impact estimates, so we do not conduct any further analysis.

8.3 Potential Influence of Other Projects

Another concern is whether other projects in the intervention areas might have affected the results. There have been few ongoing agricultural projects during the study period, but major ones can be identified fairly easily. USAID's Helping Address Rural Vulnerabilities and Ecosystem Stability (HARVEST) project ran from 2011 to 2016; it worked in a different set of provinces than the PADEE project. The follow up project, HARVEST II, only began in April 2017, and although it is working in Pursat and Battambang, it is unlikely that project activities were mature enough to affect our study. Similarly, the primary Asian Development Bank on agriculture, the Tonle Sap Poverty Reduction and Smallholder Development Project, is managed by the government and therefore by definition works in different places than are covered through PADEE or ASPIRE. Unfortunately, our surveys did not collect information about whether households were participating in any other projects that might have indirect effects on households in either pilot project.

Nonetheless, we can use the data to measure whether treatment groups had received differential attention either from NGOs (which would likely be smaller projects if so) or from the private sector, from the perspective of agricultural extension. We measured the proportion of farmers in each treatment group that report hearing at least two extension messages from either an NGO source (rather than the government) or a private provider in baseline data from both projects. For PADEE, we focus on rice messages. We find that almost no farmers who grow rice report having received extension from sources other than the government; 12 of the 1007 farmers who reported growing rice at the baseline received such extension. Clearly, outside forces did not directly affect the PADEE pilot.

For the ASPIRE project, we find slightly rates of contact with extensions from NGOs or the private sector than among the PADEE sample. About 5 percent of the ASPIRE sample have experience with messages coming from outside government related to rice production, and about 2 percent have some experience with vegetable extension messages. Whereas these rates are fairly low and are not likely to materially affect the results in aggregate, it could be that the rates differ between treatment groups and therefore could plausibly affect the results in section 7.

We therefore next compute the treatment group specific rates at baseline (Table 64). We find that we cannot reject the hypothesis that the rates are all the same, nor that a hypothesis that any pair of rates are the same. Thus, we conclude that the low rates of contact with extension messages outside ASPIRE are not likely to affect the project either.

Table 64. Rate of Contact About Extension Messages from NGOs or Private Institutions at Baseline, ASPIRE Sample

	(1) Control	(2) Basic Messages	(3) Enhanced Messages	(4) Enhanced Plus Messages
Received rice extension in two or more topics from private institutions	0.047 [0.010] *423*	0.055 [0.011] *418*	0.052 [0.011] *423*	0.050 [0.011] *423*
Received vegetable extension in two or more topics from private institutions	0.026 [0.008] *423*	0.024 [0.007] *418*	0.021 [0.007] *423*	0.017 [0.006] *423*

Note: Standard errors in brackets. Size of each group are in italics. For no pair can we reject the null that the two averages are the same at any reasonable level of significance.

As noted in the cost effectiveness section, the ASPIRE intervention appears quite scalable with low marginal costs; some care should be placed in considering whether the vegetable messages were good enough, since they were clearly less effective than the rice and chicken messages. Whereas there are no short term productivity impacts, farmer perceptions of impacts suggest they might be measurable in a much larger sample.

8.4 Limitations: PADEE

There are some specific limitations to each component of the study that are worth again highlighting. First, the type of extension that was offered was limited by the e-PADEE software. As discussed earlier, an initial limitation is that the software gave somewhat automated recommendations once the soil characteristics were measured. It was not designed to be interactive for good reason—there are other tools, such as WhatsApp, Viber, and Line that all can be used in association with e-PADEE. However, it might have been more useful to MSTs if they had been further trained to ask their peers for help answering interesting questions.

Second, the software was, as of the implementation in this project, limited to a rice module. Rice production in Cambodia could increase substantially in the aggregate agronomically, but it is not entirely clear that doing so would be economically optimal, meaning that the costs of additional inputs (or effort) might exceed the potential additional revenues from increasing yields. An alternative way to make more money at the farm level would be through growing more vegetables; however, vegetable value chains are more complex as they spoil quickly, and more importantly the vegetable module had not yet been developed when the pilot was implemented.

Third, the project might have been different had we been able to obtain extra funds for monitoring MST performance throughout the growing period. The MSTs essentially were asked to implement the e-PADEE pilot on their own, as the GDA did not provide their normal support due to budget limitations. However, that would have greatly affected the project costs as well, making any impacts less cost effective. Alternatively, the incentives for the MST group could have been made larger, but it is not clear whether that would have induced more visits or not.

Fourth, recall that in subsection 7.3 we found that the ICT group was more likely to have used fertilizer, in larger amounts, than the other two groups at baseline. Had we found statistically significant impacts on production, this difference might have been meaningful for the analysis. However, we found no such impacts; as such, it is hard to argue that accounting differently for this difference would have meaningfully affected the results.

Fifth, recall that all 60 MSTs originally selected for the project were initially trained to use the ePADEE software, a year before the trial began, and could have been implementing on their own; randomization took place at the MST/cluster pair level. There could have been MSTs that implemented some of the recommendations with farmers in the control group; the evidence would be that we do note that some portion of households within the control group state that MSTs brought tablets with them. However, the software had not been updated, so it is not clear how effective such extension would have been. Moreover, given that we found more intensity of farmers worked in with extension in the treatment groups, it should not have had a major effect on top-line results.

Since MAFF has made tablets available to all CEWs working in ASPIRE, which includes the Grameen Intel tool as well as others, further use of tablet-based extension is already being used. We are not aware of much tablet-based extension that has been very effective, with the exception of video (van Campenhout et al., 2017). We therefore suggest that MAFF consider finding ways to try to pre-load videos with tailored messages onto tablets held by CEWs prior to sending the tablets into the field (or refreshing them at any province level meetings), so that they can best make use of this technology. We return to this issue in the subsection on key lessons.

8.5 Limitations: ASPIRE

As with the PADEE pilot project, there are some limitations within the ASPIRE context. Similar to the PADEE context, one obvious limitation is that we could not establish a “pure” control group. The results are therefore additional to what can be considered ASPIRE as usual. A concern might be that some CEWs worked quite extensively with their ASPIRE groups; if so, then one might be concerned that the phone calls provided no new information. We attempt to limit this concern through the stratification at the CEW level, but it could limit the treatment effects.

Second, the phone calls were limited as they did not have repeat options. We were concerned that farmers would tire quickly of the calls when setting them up, and in fact did a review at 4 weeks to see how many farmers were continuing to listen to calls; we assumed there would be a relatively large group of farmers who never listened to calls. Instead, the information was much appreciated. We did give information about the CEW’s phone number at the end of each call but a repeat option and potentially a call-in for the same information would have likely enhanced impact at little cost.

Third, the sample design limited how much we were able to find. We called a substantial number of farmers who never appear in our data set, particularly in the enhanced plus group. As a result, we are “missing” a deeper understanding of impacts among farmers who received the calls. Indeed, the proportion of farmers in the treatment groups who reported receiving them in the survey appears quite low, so our intent-to-treat effects are dampened by the proportion of farmers who actually received calls. Some portion of farmers in the sample did not have valid phone numbers; in retrospect, an easy way to have increased statistical power in the sample would have been to survey only farmers who had valid phone numbers (although that could have created an alternative type of selection bias in the result, selecting on valid phone numbers).

Fourth, recall that our sample included both farmer group and non-farmer group members. Whereas including both helped us begin to explore how information moves around communities in the social network section, it reduces the eternal validity of the pilot from the perspective of ASPIRE. We were also not able to include as many provinces in the ASPIRE pilot, since there were few CEWs in the other three provinces and the sample exclusions necessitated removing them from the sample. So before extending the pilot to other areas, it might be worthwhile to further explore other secondary sources of data to assess how comparable the farmers in our sample are to others.

An alternative way to design this pilot would have been to work with a much larger sample and to conduct shorter phone surveys rather than long form household visits as were conducted, though it is not clear that a larger sample was actually available to us for the pilot. The benefit would have been to be able to identify smaller impacts. However, that type of design was not clearly available to us in this context, given that we worked in nearly all the appropriate CEW groups that were available to us. We could have intensified some of the data collection within groups (e.g. among farmer group members who did not participate in the survey), but it would not have been possible in 2017 to massively increase the sample size.

8.6 Key Lessons

Now that we have discussed threats to the validity of our results, we summarize and contextualize them in this section. As with other sections, we start with PADEE and follow with ASPIRE, and then we place them in the context of the literature.

8.6.1 Results Summary: PADEE

Within the PADEE intervention, we broadly find evidence that the use of the software was effective at teaching farmers additional rice extension messages, some of which were put into place by farmers. Farmers in the two treatment groups were about 50 percent more likely to report receiving agricultural extension than the control group, and the percentage of farmers reporting using seed and fertilizer recommendations increased by about 5 and 12 percentage points, respectively. However, we do not find any impacts on the extensive margin for fertilizer; households in the treatment groups do not appear to have increased the intensive of fertilizer use on average. We do not find impacts on either rice production or productivity.

The heterogeneity regressions we ran suggest some interesting differences between the ICT and ICT plus treatment groups. Social connections appear to have been important in determining impacts in the ICT group, but this relationship was broken in the incentives treatment. These results suggest that changing incentive structures within the Cambodian extension system could broaden the types of farmers who receive extension.

8.6.2 Results Summary: ASPIRE

In the ASPIRE pilot, the monitoring results show that households in both the basic and enhanced groups listened to entire calls about 60 percent of the time, throughout the period in which calls were implemented. Within the sample, farmers in the two enhanced treatment groups were more likely to report having received the calls than the basic treatment group, and about twice as many farmers perceived the calls had production impacts in the enhanced treatment groups (8-9 percentage points over the control group for rice) than the basic treatment group (about 5 percentage points). Though more farmers received calls on vegetables than on chickens, by this measure farmers found the chicken messages more useful, perhaps because more of them were already keeping chickens. Importantly, for most of these measures heterogeneity is not an issue; since calls were made to farmers both in and outside of farmer groups set up for ASPIRE, it is useful to know that farmer group members and other farmers responded to the calls at approximately the same rates.

We further find that the information that we sent through calls spread somewhat through the village. At least for rice and chicken messages, farmers who received calls were more likely to share messages with others in the village who had not received calls. As with the receipt of messages in general and the perceived impacts on productivity, farmers in the enhanced message groups were about twice as likely to report having shared messages than those in the basic messages group, and farmers were more likely to have shared or heard about the rice and chicken messages.

We find some mixed results related to either techniques applied or input use in response to the treatment, and we do not find strong positive results on changes in productivity. Among the enhanced message group only, we find impacts on the use of improved seed, NPK, urea, and pesticides in rice production. We do not find similar results for the enhanced plus group. However, these effects do not appear to translate over into increased production or income among farmers, nor do we find changes for chicken or vegetable production.

8.6.3 Context and Going Forward

The PADEE and ASPIRE pilots that were implemented in Cambodia fit well into what is a developing body of evidence about how ICT can be used to enhance agricultural extension. In general, technology is increasingly being used in propagate information in a fast and cost-effective manner (Aker 2011; Nakasone et al., 2014; Nakasone and Torero, 2016). However, the best methods for using ICT and the extent it can help are still uncertain.

Our results on the e-PADEE pilot demonstrate that we may have been using the wrong technology. An alternative that demonstrates promise is ICT-mediated extension with video. Recent randomized trials in Ethiopia, India and Uganda have demonstrated effectiveness in achieving outcomes such as increased knowledge and awareness of agricultural practices, higher rates of technology adoption, better crop yields, higher farm incomes, or improvements in household welfare, (Van Campenhout et al., 2018; Vasilaky, 2015; Abate 2018). The results we show in the PADEE heterogeneity analysis suggests that the type of farmer that can be helped by tablet-based extension may change; an interesting extension of this work might be to test using video-based extension along with varying extension worker incentives and monitoring.

Our ASPIRE results are most similar to related messages that have been sent by SMS in other countries (e.g. Feder et al., 2004). As in those models, we find modest changes in knowledge and practices. Cambodia is a bit of a unique case, in that SMS is not an effective way of trying to convey modest messages; however, as smartphones continue to spread, it may become effective in the relatively near future. Therefore, push voice messages may not work in other contexts as well, though we think they are well placed to work in other parts of Cambodia just as well.

In this context, because the government was the implementing partner, we ensured that messages were salient. Other studies have shown that affinity between the messenger and the expected audience plays an important role in ensuring impacts (e.g. Vasilaky et al., 2015; Abate et al., 2018). The challenge in using both types of extension in the future—whether tablet-based, or voice or SMS based—is to tailor more generic messages to both differing and changing agroecologies, while harnessing existing social structures and customs to appeal different types of farmers.

9 Findings for Policy and Practice

Our main findings only suggest changes along part of the theory of change, rather than moving all the way to productivity and farmer income. However, we need to place our results into the policy discussion. Based on ASPIRE budgets, a substantial amount of money is still being spent per prospective beneficiary. Second, there is a feeling that the government should get “out” of extension in the medium term. In other words, policy discussions suggest that the private sector become more involved with providing extension and finding ways for farmers to pay for it. Along a similar line of reasoning, it is believed that farmers would pay more for customized extension. We briefly describe main messages for policy makers and then for implementation, which largely suggests further research.

9.1 Implications for Policy

Our findings have at least two important implications for policy. First, there is clearly a revealed desire among farmers for more access to basic extension information, based on the monitoring results for ASPIRE. As a result, the government should consider ways to continue to provide basic information to farmers using cheap methods. We tested direct voice messages in the ASPIRE pilot in part because SMS messages were not possible in this context; an alternative is to provide a call-in service for farmers to get information through a menu driven system.[29] We think our results suggest that both are potentially promising ways to deliver basic messages to farmers (the former we have demonstrated; the latter would still need further evidence). We understand that the government is continuing to work with Viamo mobile on a call-in service that is already available. We note that continuing to make phone calls or providing support to a call-in system would represent a very small share of the annual ASPIRE budget. That said, more monitoring and evaluation would be worthwhile to understand whether these information systems can lead to increased production or income among farmers.

Second, in a related recommendation, we believe it is important to think about the definition of the private sector in this context. Cell phone and other technology companies may have good incentives to offer access to call-in systems or even to sign up farmers for direct call extension, as these services represent ways to maintain their customer base, which often switch companies when better offers become available. These companies are potentially a better bet than working directly with input suppliers, for example, who could bundle extension with other services. A problem with that type of extension is that it is often narrow relative to what government agents or NGOs provide (e.g. de Brauw et al., 2018).

On a third note, if CEWs are to be provided with tablets, we believe that whereas systems like e-PADEE can be effective, there are other systems that appear to provide more promise at the moment. Specifically, we think recent evidence from trials using video-based extension on tablets might hold more promise than e-PADEE or something similar for delivering impacts. We suggest at least exploring the development of videos that could be placed on CEW tablets when they are in places for meetings with better network coverage.

[29] Many rural farmers in Cambodia have cell phones that cannot text with the Khmer script; hence they tend to use phones for calls rather than text messages.

9.2 Implications for Implementation

As noted several times in the report, we did not find impacts on final outcomes, such as agricultural production or income. However, the relatively cheap model of ICT (direct calls) appeared more promising on a cost effectiveness, basis, and it could be that with a longer trial, impacts on such outcomes could have materialized. One potential implication is that it would be worth continuing to send messages such as these, or at least the rice and chicken messages, to ideally test for impacts from repeated exposure. One could improve the system to allow for either callbacks or repeated messages in doing so.

An interesting test might also be to test direct calls against encouragement to use a call-in service. Whereas the direct call requires no effort, a call-in service requires the farmer to call the number and go through whatever process exists to find the message that they would want to hear. Such a test would be both interesting behaviorally but would also potentially help explore whether call-in services can be as effective, as it is easier to ask the private sector to provide them. One way to achieve a cheaper way to conduct such an evaluation would be to use a larger sample and phone surveys. a model of such a survey might be to send a data collection team to the field just to gather phone numbers, and then to conduct a short phone survey; our experience in the e-PADEE evaluation shows that phone surveys in Cambodia are well received. With such a model, a fairly large sample size could be achieved at fairly low cost. An alternative would be to try to provide pamphlets with the same information, but doing so would require a quality assurance process that could be complex in Cambodia; for example, the initial e-PADEE trial was held up due to long discussions about which words to use for different aspects of rice production.

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Online appendixes

3ie is publishing the following appendixes online only and as submitted by the authors. They have not been copy-edited for formatted.

Online appendix A – Survey Instruments

ASPIRE Baseline Questionnaire 2017:
https://www.3ieimpact.org/sites/default/files/2019-06/TW4.1013-appendix-A-ASPIRE-Baseline-Questionnaire-2017.pdf

ASPIRE Follow up Questionnaire 2018:
https://www.3ieimpact.org/sites/default/files/2019-06/TW4.1013-appendix-A-ASPIRE-Follow-up-Questionnaire-2018.pdf

PADEE Baseline Questionnaire 2016:
https://www.3ieimpact.org/sites/default/files/2019-06/TW4.1013-appendix-A-PADEE-Baseline-Questionnaire-2016.pdf

PADEE Follow up Questionnaire 2017:
https://www.3ieimpact.org/sites/default/files/2019-06/TW4.1013-appendix-A-PADEE-Follow-up-Questionnaire-2017.pdf

PADEE MST Questionnaire 2016:
https://www.3ieimpact.org/sites/default/files/2019-06/TW4.1013-appendix-A-PADEE-MST-Questionnaire-2016.pdf

Online appendix B – Pre-Analysis Plan

https://www.3ieimpact.org/sites/default/files/2019-06/TW4.1013-PADEE-ASPIRE-Cambodia-Appendix-B-Pre-Analysis-Plan.pdf

Online appendix C – Additional Results

https://www.3ieimpact.org/sites/default/files/2019-06/TW4.1013-PADEE-ASPIRE-Cambodia-Appendix-C-Additional-Results.pdf