Harnessing ICT to Increase Agricultural Production: Evidence From Kenya[*]

Lorenzo Casaburi
University of Zurich
lorenzo.casaburi@econ.uzh.ch

Michael Kremer
Harvard University
mkremer@fas.harvard.edu

Sendhil Mullainathan
Harvard University
mullain@fas.harvard.edu

Ravindra Ramrattan**
Innovations for Poverty Action

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Abstract

Sending SMS messages with agricultural advice to smallholder farmers increased yields by 11.5% relative to a control group with no messages. These effects are concentrated among farmers who had no agronomy training and had little interaction with sugar cane company staff at baseline. A follow-up trial of the same intervention has, however, no significant impact on yields.

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1. Introduction

Mobile phone technology has achieved high penetration very rapidly in much of the developing world (Aker and Mbiti, 2010), Nakasone, Torero, and Minten, 2014). While there is some encouraging evidence on its impact on market integration (Jensen 2007; Aker, 2011), education (Aker, Ksoll, and Lybbert, 2012), and access to finance (Karlan et al., 2016; Jack and Suri, 2013), there is little evidence on output effects.

Agricultural yields in Sub-Saharan Africa have been mostly stagnant and there has been limited adoption of new technologies (Jack, 2011; AGRA, 2017). There is widespread consensus that efforts to deliver agricultural information via traditional extension have been disappointing (Anderson and Feder, 2007), in part due to the difficulty in monitoring agriculture extension workers, the expense of the activity (BenYishay and Mobarak, 2018), and the high farmer-extension ratio. Mobile phones could potentially offer the opportunity to deliver personalized agricultural information to farmers at low cost and in a way that is tailored to their context and timed to coincide with the relevant part of the agricultural season. Earlier work on General Purpose Technologies suggests that the impact of ICT may depend on additional complementary technologies and organizational changes (Helpman, 1998; Jovanovich and Rosseau, 2005).

We collaborate with one of the largest agri-business companies in East Africa. The partner company runs a sugarcane large contract farming scheme. Farmers' plots are mostly below one hectare. In the contract farming arrangement, the company provides inputs on credit that are recouped at harvest through payment deductions.

The paper evaluates an intervention that leveraged on the growing penetration of mobile phones in the region to improve agricultural productivity, Farmers receive a set of text messages that inform them about agricultural tasks to be performed right around the time they need to complete such tasks on the plot.

For the evaluation, we rely primarily on rich plot-level administrative data collected by the company to measure their impact. The main outcomes of the analysis are plot yields. In addition, the evaluation uses several other variables recorded in the company database to define strata bin, check balance, and improve precision of the estimates. The interventions are evaluated through randomized controlled trials. Randomization occurred at the level of the field, defined as a set of plots (typically, three to ten) that the company treats homogeneously in terms of planting cycle, input delivery, and harvesting in order to achieve economies of scale in these activities.

We conduct two rounds of the SMS intervention, one in 2011-2013, and one in 2012-2014. In the first round, access to the SMS project raises yields by around 3.3 tons per hectare, or 8% of the control group average. With a sign-up rate for the text message program of 65% in the treatment group, this implies a treatment-on-treated effect of about 11.5%. These effects are concentrated among farmers who at baseline had no agronomy training and had little interaction with company field staff. In the second round, the point estimate of the SMS treatment effect is close to zero, though confidence intervals include large impacts on yields.[1]

In the final part of the paper, we discuss the potential advantages that large contract farming companies have as a source of information provision for small farmers. We estimate that the increase in yields in the first round of the SMS intervention generated an increase of about $43 in company profits and of about $54 in farmer earnings, while the per-farmer cost of the program is about $0.3 per farmer. We then present results from another trial that used farmer response rates to a mobile-based survey to shed light on some of the barriers to information flow in agricultural value chains.

The findings from the paper are in line with those by Cole and Fernando (2016), who show that, in response to a mobile phone based agricultural extension program in India, *Avaaj Otalo*, farmers increased the adoption of more effective and less hazardous

[1] In the second wave, we did not conduct farmer surveys and thus cannot measure heterogeneity in the treatment effect by baseline agronomy training and interaction with company staff.

pesticides and in turn increased their crop yields. Another line of work shows mixed evidence for the role of mobile phones in improving price information the (Jensen, 2007; Aker, 2011; Aker and Fafchamps, 2013; Mitra, Mookherjee, Torero, and Visaria, 2018). Relative to this previous literature, we shift the focus toward agricultural yields and coordination along the supply chain. We also use administrative data, as opposed to self-reported outcomes. The risks of social desirability bias and Hawtorne effects in survey responses (Zwane et al., 2011) seem particularly relevant for information provision interventions, as these generally make recommendations on what the target respondents should be doing. From this standpoint, access to an objective measure of productivity is a major advantage of our study.

The remainder of the paper is organized as it follows. Section 2 provides background on the experimental setting. Section 3 describes the farmer SMS intervention. Section 4 discusses the relative advantages of large organizations, such as contract farming schemes, in leveraging the use of ICT to increase agricultural output. Section 5 concludes.

2. Background

The research described in this paper was conducted in partnership with one of the largest agri-business companies in East Africa. The company runs a large sugarcane contract farming scheme, involving mostly smallholders with plot sizes less than one hectare.[2] Following the establishment of five outgrower schemes between 1968 and 1981, sugarcane has become the most common cash crop in the region of study.

Sugar cane crushing and boiling are capital intensive processes and are subject to significant economies of scale, with a large fixed cost component. Marginal costs (other than sugar cane input and transport costs) for the factory are low, because its capital stock and crushing capacity are fixed, and raw material inflow is almost always less than the plant capacity. The factory runs 24 hours a day and factory labor needs vary little with

2 Additional details on the study setting are provided in Casaburi, Kremer, Mullainathan (2014).

throughout. The plant is actually a net energy seller, because it burns by-product from crushed cane.

Transport costs for sugar cane are very high. The nature of the processing also limits the development of spot markets and the degree of potential competition from other buyers located farther away. These transport costs, combined with economies of scale in processing, thus give the factory substantial market power as a cane buyer. The sugarcane price is de facto regulated through the Kenya Sugar Board. The gap between the input price of sugar cane and the price of processed sugar means that the farmer and the factory are both de facto residual claimants on gains in yield per acre.

Each harvest cycle lasts from 18 to 22 months. The company and the farmer sign a contract that typically spans for one replant cycle, made up of one planting and several ratoon harvests.[3] Planting and harvesting occur in a staggered fashion throughout most of the year, in order to provide a constant supply of cane to the processing mill. Sugar production processing requires high coordination across harvesting, transporting, and processing. Processing needs to occur shortly after harvesting as sugar content starts declining after the cane is cut.

Farmers are paid based on the tonnage of cane provided at harvest time. Input charges plus interest are deducted from the payment. The cane prices are based on the current sugar price, via a formula that includes the conversion rate between cane and final sugar output and taxes on sugar production. As a result of the pricing formula, the company estimated revenue per ton of cane purchased from the farmers is $30. Since the plant is almost never at capacity, the marginal processing costs are quite low, with an estimated upper bound of $5 per ton of cane purchased. As a result, the company profit per additional tons of cane is $25. On the other hand, farmers make around $30 per extra ton of cane, computed as the difference between the cane price and the harvesting and

3 Ratooning leaves the root and lower parts of the plant uncut at the time of harvesting. Yields typically fall across ratoons. A contract typically spans two or three ratoons.

transport charges per ton of cane.

Each plot is typically matched to one parcel as defined by the Kenyan land registry. In addition, accounts are aggregated into *fields*, sets of plots that are usually treated homogeneously for land preparation, input provision, and harvesting, in order to exploit economies of scale in these activities. Typically, on farmer is contracted on each plot, though there is a small fraction of ``joint plots", cultivated by two or more farmers. While the majority of the farmers live in the same area where the plot is located, we estimate that 15-20% of the contracting farmers in the scheme are ``telephone farmers", who reside away from the plot (typically in larger towns) and, for the most part, hire labor to complete the cane farming tasks.

The factory is concerned about farmers exerting low level of effort and engaging in input diversion (e.g., use of fertilizer on crops other than sugarcane or re-selling). Some farmers complain about poor performance of company staff and contractors and about the delays in input provision and payments. Moral hazard concerns in the company hierarchy are also likely to be relevant. For instance, managers need to monitor field staff in order to ensure that the scheduling of input delivery occurs timely. The ability of the company to deliver information to the farmers was traditionally limited by the low ratio between field staff members and farmers, in the order of 1 to 1,000.[4] In addition, the distance between farmers' residences and the company premises implies farmers would need to bear high transport costs in order to report concerns at the company premises. As a result, most farmers report few interactions with company staff. The two interventions described in the next sections used mobile phones to increase the flow of information between the company and the farmers.

3. The Farmer SMS Intervention

4 In Kenya, the extension agent to farmer ratio is 1:1,500. Figures are even lower in other countries in SSA (for instance, BenYishai and Mobarak (2018) on Malawi).

3.1 Experimental Design and Implementation

The SMS experiment was designed in close collaboration with the agronomy department of the partner company. The intervention team compiled a list of messages to be sent to farmers subscribing for the service. The content of these messages was primarily based on the age of the cane and on the harvest cycle (i.e., plant vs. ratoon). The messages warned the farmer about the need to complete a task on the plot. For instance, with regards to weeding: ``Hello Mr./Ms. {farmer name}. It is 12 weeks since you planted, your plot may have weeds by now from the last time you weeded your plot; Please remember to weed this week. This message is from Mumias Sugar Outgrowers Helpline". Similar messages concerned other tasks such as trashlining (i.e. sorting of the leaf trash from the previous harvest), intercropping, and parasite controls. Other messages were prompted by the timing of delivery of company provided inputs, such as fertilizer: “Hello Mr./Ms. {farmer name}, fertilizer (UREA) will be delivered in your field/bloc shortly/soon. Please prepare to receive and apply in time because timely fertilizer application is essential for good cane growth. This message is from Mumias Sugar Outgrowers Helpline".

The experiment took place in two rounds. The first round, which ran from mid 2011 to early 2013, targeted 2,327 plots in 354 fields. According to the company records, these plots were about to enter a new plant cycle or a new ratoon cycle. The second round, which ran from late 2012 to mid 2014, targeted 8,081 plots in 1,089 fields.[5] In both rounds, randomization was conducted at the field level and split across (approximately) monthly waves. Within each round-wave, Stratification occurred by harvest cycle type (plant vs. ratoon), two geographic zones in which the contract farming catchment area was divided, average yield groups (only for round 1) and variable capturing the field-level average response rate to a phone survey done before the intervention (only for round 2).[6]

[5] The second round was cross-cut with another treatment, the farmer hotline intervention, which we discuss in a separate paper (Casaburi et al., 2019).

6 Baseline yield data are available for 81.5% of the plots targeted by the study.

Table 1 and 2 show that, in the two rounds of the SMS intervention, the randomization achieved balance across several baseline variables, although for a handful of covariates there are significant differences between treatment and control groups.[7]

Company staff managed the recruitment for the treatment fields. The recruitment strategy varied between the two rounds. In the first round, the company staff held at least one meeting in each field, inviting all the farmers listed for selection. The take-up rate for the SMS project was 65.7%. The majority of the non-compliance is due to farmers not attending the recruitment meetings, as opposed to farmers explicitly turning down the offer (the acceptance rate conditional on showing up to the meeting was 87%). Table 3 shows that take-up was substantially lower for telephone farmers. In the second round company officers recorded farmers' phone numbers at the time of the harvest of the cycle that preceded the experiment. During the recruitment for the intervention, which was conducted before the randomization, 3,768 (out of 8,081 belonging to the study fields), recorded their cell phone number and qualified as eligible for the service in the case in which their field was randomized into the treatment group.[8] For the analysis of the round 2 intervention we look at the treatment impact on both eligible and non-eligible plots

About 24% of the plots (20.5% in round 1 and 24% in round 2) ended up not completing the cane cycle targeted by the experiment, and therefore we do not have outcome data for them. This was primarily due to the fact that the company did not complete land preparation or that farmers opted to use the plot for other crops. In the Appendix, we show that there is no significant impact of treatment on the likelihood of completing the cycle among plots in round 1 and among *eligible* plots in round 2. However, among *non-eligible* plots in round 2, the likelihood of completing the harvest cycle is lower for treatment plots (p=0.08). The remainder of the analysis focuses on plots that completed

[7] All of our key results are robust to controlling for these covariates.
[8] Below, we conduct the analysis of the treatment impact separately for eligible and non-eligible farmers

the harvest cycle.

3.2 Impact on Plot Yields

In Table 4, we study the impact of having access to the SMSs on plot yields. All the regressions include stratification dummies. Standard errors are clustered at the field level, the unit of randomization. The results vary across the two waves. Panel A shows the results for round 1. Plots in the treatment group achieve yields that are 3.33 tons per hectare larger than the control group, or 8% the control group mean. The treatment on treated for compliers is equal to 11.5% of the control group mean yields. In column (2) we add to the regression model a vector of plot-level controls, which include zone, cane cycle, baseline yields, and plot size. The first three are just finer versions of the strata variables. Plot size have high explanatory power given the presence of decreasing returns in this setting (Casaburi, Kremer, and Mullainathan (2014)). Adding these controls increases estimate precision while not changing the coefficient of interest significantly. The results are unchanged in column (3), where we further add a dummy for telephone farmers and a dummy for leased plot.

Table 5 shows the treatment effect on yields in round 2. Here, we do not detect a significant impact, though, due to low power, our 95% confidence intervals include large impacts of the treatment (e.g., a 7\% increase relative to the control mean).[9] In the conclusion, we discuss the difference betweent he two rounds.[10] In the conclusion, we return to the difference in the results of the two rounds.

Table 6 presents several robustness checks for the results of round 1. In column (1), we

[9] The treatment estimates in round 2 are not significantly different from the estimates in round 1 (p-value=0.27 when comparing ITT estimates for round 1 to the treatment effect for eligible farmers in round 2; p-value=0.18 when comparing TOT estimates for round 1 to the treatment effect for eligible farmers in round 2).

[10] Given some slight imbalance in baseline covariates, the coefficient size and magnitude change once we include controls (especially for non-eligible plots), though they are never significantly different from zero or from the specification without controls.

redefine our outcome variable to equal zero if a plot never entered the harvest cycle targeted by the experiment. We find that the coefficient displays little changes relative to the main specification. In columns (2) and (3) we winsorize our outcome variable at the 99^{th} and 95^{th} percentile, respectively, in order to show that outliers in the yield distribution do not drive the results. The estimated coefficients are similar to the one of the main specification and both significant at 95%. In column (4), we use the natural logarithm of yields. The point estimate suggests in the logarithmic regression, 0.07, is consistent with the percent increase estimated in the level regression. In column (5), we drop from the sample plots that are below 0.2 acres, reducing the sample size by 7.3%. The coefficient on the cell phone group remains similar, confirming that very small plots do not drive the results. Finally, we also run our regressions dropping one at a time each of the six randomization waves and each of the five zones in which the catchment area is split. We verify that the ITT estimates are quite stable across these specifications, thus confirming that our results are not driven by any specific sub-sample (results available on request).

3.3 Heterogeneity Analysis

In order to shed light on the economic mechanisms that could drive the yield impact in round 1, we use farmer survey data collected around the beginning of the cycle, before the randomization occurred.[11] We have a baseline survey with the above information for 1,719 farmers from 1,676 plots, 72% of the study sample. Farmers for which we lack baseline data were absent both in the initial meeting and in subsequent revisit and tracking attempts. Therefore, the sample for which baseline survey data are available is a non-representative sample of the study sample. For instance, 57% of the farmers for which we are missing survey data are ``telephone farmers" while these make only 18% of the overall sample. Importantly, the proportion of farmers surveyed is 69% for the

[11] We did not conduct farmer surveys in round 2.

treatment group and 75% for the control, a difference significant at 5%.[12]

In this survey, we asked whether the farmer had attended agronomy training in the previous 12 months. If one of the effects of the SMSs is to increase information about the range, timing, and frequency of agronomic tasks, then we would expect their impact to be lower for farmers that had received such training. We also gathered information on whether the farmer interacted with a company field assistant around the beginning of the cycle (i.e., in the month preceding the survey).

In Table 7, column (1) and (2), we run our ITT yield regression on the sub-sample of plots for which we have survey data. The point estimates of the intention-to-treat effect are slightly higher than the ones in the main sample (3.59 vs. 3.32 in the baseline specification and 3.87 vs. 3.33 with control), though within one-third of a standard error. This difference primarily arises from the fact that the survey subsample includes a lower proportion of telephone-farmers, who are less likely to take-up the option to receive the SMS (as we reported in Table 3). Consistent with this observation, the treatment-on-treated effect is comparable across the two samples (4.8 in the full sample and 4.7 in the survey subsample).

In columns (3) and (6), we interact the treatment variable with the field assistant contact and training dummies, respectively. Consistent with the hypothesized channels, the coefficient on the interaction terms are negative and significant at 1% and 5%, respectively. In Columns (4) and (7) we add the vector of plot controls. The point estimates are still significant at 5%, though the coefficient on the interaction with company staff dummy shrinks. The results are similar when we further add a full vector of interactions among the plot controls and the treatment dummy (columns 5 and 8).

12 Later in the paper, we verify that the impact of the treatment on yields is very similar when restricting the sample to plots that completed the baseline survey.

The interpretation of these heterogeneity results must take into account that the interaction variables could be correlated with other unobserved plot characteristics. With this caveat in mind, we argue that these interaction terms are consistent with the fact that SMSs operate through an information channel. We also note that the result on the interaction with the sugarcane company staff may also arise from a monitoring effect if the farmers perceive that the company is observing their harvest cycle when they receive the text messages. However, the company did not specifically conduct plot inspections following the sending of the text messages.

4. Contract Farming and Information Provision

Our findings provide some evidence that ICT-driven services can affect efficiency in agricultural supply chains chain. For instance, the LATE estimate of the impact of the SMS program on plot yields in the first round is comparable to 20-30% of the increase in yields expected from the introduction of high-yielding sugarcane varieties in other Sub-Saharan African countries (Chambi and Isa, 2010; SASRI, 2013) and to 30% of the estimated increase in yields from soybean intercropping, a commonly recommended practice to alleviate sugarcane nitrogen requirement (Shoko, Zhou, and Pieterse, 2009).

Our partner, a large contract farming scheme, was particularly well positioned to design and pilot such interventions. First, as discussed above, the company has an incentive to research and invest in ICT solutions because, as the price paid to the farmer is below the marginal revenue product, it profits from the additional plot productivity. Given the low-cost of the text messages ($0.02 per text message, for a total of around $0.3 per plot), the intervention was not only extremely cost-effective but it raised profits for the company as well as farmer revenues. Given the average plot size (0.52 ha.), the SMS intervention increased production in the average plot by 1.73 tons. Using the figures provided in Section 2, we estimate that the first round of the SMS intervention increased company profits by $43 and the farmer revenues (net of additional harvesting and transporting costs) by $54.

Second, there are significant economies of scale in information production (agronomy trials, data collection, management, and analysis). A large company is better positioned to bear some of the potentially large fixed costs involved in these activities.

Third, farmers may be more likely to perceive the company as credible information.[13] We investigate the importance of credibility concerns a survey response experiment. In a pilot program, we ran several polls via SMS. These asked questions about farmer preferences (e.g. ``would you be interested in receiving chemical herbicides on credit from the company"), farmer information about company practices (e.g. "where are the company weigh-bridges?"), and farmer characteristics (e.g. "do you have a saving account"?). The response rates to these polls are quite low. In a basic treatment where farmers receive the SMS from a dedicated short-code and pay for answering, the overall response rate is 7%. We introduce several variations of this basic treatment in order to shed light on the importance of credibility of the source. In one treatment, we deliver a company brochure about the survey to a subset of farmers. In another subsample, we increase the uncertainty about the source by sending SMS from a regular 10-digit number as opposed to the dedicated short-code. These long codes are more likely to be associated with less reliable and respectable sources. Finally, we waive the SMS cost to another subsample of farmers.

Table 8 presents the results of the survey response trials. The comparison across the different treatments is presented in column (1). We find that providing farmers with a brochure increases response rates by 3.6 percentage points, or 51% of the basic group. mean. This amounts to 64% of the increase we observe when waiving the SMS price to the farmer (5.6 percentage point). We argue that the brochure reduces uncertainty about the source. However, it could also affect response rates by inducing farmers to pay more attention to the messages (a "de-cluttering" effect). In addition, we find that sending

13 On the other hand, Duflo, Kremer, and Robinson (2008) show that information on fertilizer dosage provided by a government affiliated research center leads negative returns.

SMSs from a long-code lowers response rates by 2.1 percentage points (relative to the standard short-code). Finally, for a subset of survey polls, we vary the nature of the question sent to different farmers. Specifically, in these polls, a subset of questions is labeled as confidential, as farmers were asked about their account, input charges and payment terms. In column (2) of Table 8, we show that the impact of the long-code on response rates is significantly more negative when the SMS surveys request the farmer to include confidential information in their response. We interpret the results from these trials as consistent with the hypothesis that credibility of the source is an important determinant of the volume of information flows across agents in the value chain. We argue that, relative to other agents such as the government or commercial information providers, a large processor has more immediate gains from delivering accurate information and that the farmers will take into account this incentive when responding to the information provided.

5. Conclusion

The results of the paper suggest that ICT can increase efficiency in agricultural supply chains, at least in the context of this study. In an initial pilot study, sending text messages with agricultural advice to smallholder farmers increases yields by 11.5% relative to the control group. These effects are concentrated among farmers who had no agronomy training and had little interaction with sugar cane company staff at baseline. The intervention generated large returns in terms both of farmer earnings and company profits.

The lack of significant yield results in a second pilot obviously induces caution on the generalizability of the results. The two samples are different along important characteristics, like baseline yields and crop cycle, as well as harvest season. In addition, shortly after the end of the second round, the company started experiencing major management and financial problems (see Casaburi et al., 2019 for details). While the two estimates are not statistically different, this may be due to low power. Differences in season, farmer characteristics, and other features of the underling environment may well imply that the effect of the intervention was truly different across the two rounds (in line with the insights by Rosenzweig and Udry, 2019). The differences in the results of the two rounds motivate current work that the research team is undertaking on the use of ICT in agricultural value chains in several countries through the *Precision Agriculture for Development* program (http://precisionag.org/).

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Table 1: SMS (Round 1): Balance

	Control	SMS	p-value	N
Plant Cycle	0.45	0.43	0.49	2327
	(0.50)	(0.49)		
Ratoon 1 Cycle	0.15	0.11	0.53	2327
	(0.36)	(0.31)		
Ratoon 2 Cycle	0.40	0.46	0.44	2327
	(0.49)	(0.50)		
Plot Size (ha.)	0.53	0.53	0.88	2327
	(0.39)	(0.45)		
Zone 1	0.24	0.32	0.22	2327
	(0.43)	(0.46)		
Zone 2	0.16	0.18	0.45	2327
	(0.37)	(0.39)		
Zone 3	0.21	0.18	0.68	2327
	(0.41)	(0.38)		
Zone 4	0.16	0.16	0.69	2327
	(0.36)	(0.37)		
Zone 5	0.23	0.16	0.23	2327
	(0.42)	(0.37)		
Leased Plot	0.03	0.02	0.33	2327
	(0.16)	(0.14)		
Telephone Farmer	0.18	0.18	0.81	2327
	(0.38)	(0.38)		
Baseline Yields	49.15	50.25	0.66	1898
	(27.36)	(26.37)		
Spoke to Company Staff in Last Month	0.31	0.30	0.67	1627
	(0.46)	(0.46)		
Agronomy Training in Last 12 Months	0.15	0.16	0.98	1643
	(0.36)	(0.36)		

Notes: All the regressions include field-level stratification dummies. Standard errors are clustered at the field-level. * p<0.1, **p<0.05, ***p<0.01.

Table 2: SMS (Round 2): Balance

	Eigible Plots				Non-Eigible Plots			
	Control	SMS	p-value	N	Control	SMS	p-value	N
Plot Size (ha.)	0.43	0.44	0.43	3768	0.43	0.44	0.56	4313
	(0.29)	(0.31)			(0.31)	(0.32)		
Ratoon 1	0.29	0.25	0.41	3768	0.29	0.27	0.11	4313
	(0.45)	(0.43)			(0.45)	(0.45)		
Ratoon 2	0.33	0.25	0.80	3768	0.35	0.26	0.49	4313
	(0.47)	(0.43)			(0.48)	(0.44)		
Ratoon 3	0.10	0.09	0.37	3768	0.10	0.07	0.10	4313
	(0.31)	(0.28)			(0.30)	(0.26)		
Zone 1	0.10	0.12	0.25	3768	0.07	0.09	0.29	4313
	(0.31)	(0.33)			(0.25)	(0.29)		
Zone 2	0.26	0.25	0.27	3768	0.29	0.27	0.59	4313
	(0.44)	(0.43)			(0.45)	(0.45)		
Zone 3	0.25	0.27	0.25	3768	0.26	0.28	0.26	4313
	(0.43)	(0.44)			(0.44)	(0.45)		
Zone 4	0.18	0.19	0.08*	3768	0.18	0.18	0.20	4313
	(0.39)	(0.39)			(0.38)	(0.38)		
Zone 5	0.20	0.17	0.18	3768	0.21	0.18	0.00***	4313
	(0.40)	(0.38)			(0.40)	(0.39)		
Baseline Harvest: Yield	59.08	56.45	0.74	3141	54.64	53.03	0.40	3533
	(27.95)	(28.02)			(28.71)	(30.32)		
Baseline Harvest: Urea Delivered	0.87	0.87	0.86	3141	0.87	0.90	0.39	3533
	(0.33)	(0.33)			(0.34)	(0.30)		
Baseline Harvest: Urea Delivered in Time	0.65	0.57	0.20	3141	0.69	0.60	0.09*	3533
	(0.48)	(0.50)			(0.46)	(0.49)		

Notes: All the regressions include field-level stratification dummies. Standard errors are clustered at the field-level. * p<0.1, **p<0.05, ***p<0.01.

Table 3: SMS (Round 1): Take-up

	(1)	(2)
Take-up cell Treatment Group	0.657***	
	[0.014]	
Ratoon 1 Cycle		0.043
		[0.051]
Ratoon 2 Cycle		-0.025
		[0.034]
Plot Size (ha.)		-0.027
		[0.031]
Zone 1		-0.087**
		[0.042]
Zone 2		-0.081*
		[0.047]
Zone 3		-0.080*
		[0.047]
Zone 4		-0.093*
		[0.048]
Leased Plot		-0.108
		[0.101]
Telephone Farmer		-0.243***
		[0.036]
Baseline Yields		0.000
		[0.001]
Observations	1172	1172

Notes: Column 1 is the take-up rate in the cell-phone group. Column 2 reports take-up determinants among the cell-phone group. Column 2 also includes a binary variable equal to one if baseline yields are missing. * p<0.1, **p<0.05, ***p<0.01.

Table 4: SMS (Round 1): Yield Regressions

	Yields		
	(1)	(2)	(3)
SMS	3.326*	3.339**	3.331**
	[1.719]	[1.536]	[1.532]
Plot Controls	N	Y	Y
Extra Controls	N	N	Y
Mean Y Control	41.625	41.625	41.625
Observations	1849	1849	1849

Notes: The table reports intention-to-treat estimates. *Yields* are measured in tons/hectare. The sample includes the 1,849 plots that entered the project cycle (out of the 2,327 included in the randomization). *Plot Controls* include plot size zone fixed effects, cane cycles fixed effects, baseline yields and a dummy for whether baseline yields are available. *Extra Controls* include a telephone farmer dummy and a leased plot dummy. All the regressions include field-level stratification dummies. Standard errors are clustered at the field-level. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table 5: SMS (Round 2): Yield Regressions

	Eligible		Non-Eligible	
	(1)	(2)	(3)	(4)
SMS	0.854	-0.494	1.534	-1.125
	[1.395]	[1.211]	[1.698]	[1.418]
Mean Y Control	55.046	55.046	52.397	52.397
Controls	N	Y	N	Y
Observations	2819	2819	3178	3178

Notes: *Yields* are measured in tons/hectare. The sample includes the 2,808 eligible plots and the 3,185 non-eligible plots that entered the project cycle in round 2 (out of the 3,768 eligible plots and the 4,313 non-eligible ones included in the randomization). *Controls* include plot size zone fixed effects, cane cycles fixed effects, baseline yields and a dummy for whether baseline yields are available. All the regressions include field-level stratification dummies. Standard errors are clustered at the field-level. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table 6: SMS (Round 1): Yield Regressions Robustness

	With zeros	Winsor Top 99	Winsor Top 95	Log	Drop Plots <.2ha
	(1)	(2)	(3)	(4)	(5)
SMS	3.297*	3.106**	2.749**	0.071*	3.047**
	[1.766]	[1.451]	[1.320]	[0.040]	[1.550]
Average Y Control	33.084	41.379	40.642		40.583
Observations	2327	1849	1849	1849	1714

Notes: In the column *With zeros*, yields equal zero for plots for which we do not observe yields. All the regressions include the following controls: plot size, zone fixed effect, cane cycle, baseline yields, telephone farmer dummy, leased plot dummy, and a dummy for whether baseline yields are available. All the regressions include field-level stratification dummies. Standard errors are clustered at the field-level. * p<0.1, **p<0.05, ***p<0.01.

Table 7: SMS (Round 1): Heterogeneity by Baseline Survey Variables

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
SMS	3.589*	3.867**	5.999***	5.381***		4.299**	4.588**	
	[1.909]	[1.749]	[2.128]	[1.943]		[2.073]	[1.865]	
SMS*Spoke to Company Staff			-8.402***	-5.579**	-6.057**			
			[2.929]	[2.583]	[2.623]			
Spoke to Company Staff			4.950**	4.722**	4.831***			
			[2.106]	[1.871]	[1.858]			
SMS*Agronomy Training						-6.075*	-7.528**	-7.556**
						[3.374]	[3.048]	[3.014]
Agronomy Training						2.107	2.848	2.773
						[2.373]	[2.275]	[2.258]
Controls	N	Y	N	Y	Y	N	Y	Y
Controls Interactions	N	N	N	N	Y	N	N	Y
Mean Y Control	41.871	41.871	42.124	42.124	42.124	41.885	41.885	41.885
p-value main coeff+interaction			0.396	0.938		0.558	0.303	
Observations	1391	1391	1343	1343	1343	1342	1342	1342

Notes: The dependent variable is plot yields. The variable *Spoke to Company Staff* is equal to one if the respondent spoke to a member of the company staff in the previous month. The variable *Agronomy Training* is one if the respondent attended an agronomy training in the previous 12 months. The columns with *Controls* include a vector of plot level controls (plot size, telephone farmer dummy, leased plot dummy, zone fixed effect, cane cycle, baseline yields and a dummy for whether baseline yields are available). The columns with *Controls Interactions* include the above controls and their interaction with the treatment status. These controls include continuous variables such as plot size and yields. Therefore, for these columns, we do not report the baseline coefficient on *SMS* since this would capture the ITT effect of the experiment when all these covariates are equal to zero. All the regressions include field-level stratification dummies (wave, plant cycle, macro-zone, baseline average productivity). Standard errors are clustered at the field-level. * p<0.1, **p<0.05, ***p<0.01.

Table 8: Farmer-Polls: Response Rates

	(1)	(2)
Brochure	0.036***	0.037**
	[0.006]	[0.015]
Brochure*Confidential		-0.005
		[0.021]
Long Code	-0.021***	-0.035***
	[0.006]	[0.012]
Long Code*Confidential		-0.050***
		[0.018]
Free SMS	0.056***	0.070***
	[0.007]	[0.015]
Free SMS*Confidential		-0.008
		[0.021]
Confidential		0.058***
		[0.013]
Mean Y Control	0.070	0.094
Observations	57615	7139

Notes: The dependent variable is a dummy equal to one if the farmer respond to the specific poll. The variable *Brochure* equals one if the respondent receives a brochure about the polls at the beginning of the intervention. The variable *Long Code* equals one if polls are sent from a standard 10-digit number, as opposed to the dedicated short-code. The variable *Free SMS* equals one if answering the poll is free for the farmer. All the regressions include field level stratification dummies. Standard errors clustered at the field level. * p<0.1, **p<0.05, ***p<0.01.

A Appendix Tables

Table A.1: SMS (Round 1): Entry into the Project Cane Cycle

	(1)	(2)	(3)
SMS	0.024	0.017	-0.074
	[0.029]	[0.027]	[0.097]
Ratoon 1 Cycle		0.247	0.239
		[0.175]	[0.180]
Ratoon 1 Cycle*SMS			0.007
			[0.059]
Ratoon 2 Cycle		0.025	0.007
		[0.171]	[0.178]
Ratoon 2 Cycle*SMS			0.039
			[0.062]
Plot Size (ha.)		0.089***	0.104***
		[0.021]	[0.036]
Plot Size (ha.)*SMS			-0.028
			[0.041]
Zone 1		0.156***	0.051
		[0.052]	[0.069]
Zone 1*SMS			0.211**
			[0.097]
Zone 2		-0.112	-0.112
		[0.088]	[0.091]
Zone 2*SMS			0.048
			[0.088]
Zone 3		-0.028	-0.069
		[0.086]	[0.090]
Zone 3*SMS			0.119
			[0.083]
Zone 4		-0.066	-0.074
		[0.066]	[0.080]
Zone 4*SMS			0.045
			[0.107]
Leased Plot		-0.037	-0.055
		[0.046]	[0.062]
Leased Plot*SMS			0.029
			[0.090]
Telephone Farmer		0.004	0.020
		[0.023]	[0.033]
Telephone Farmer*SMS			-0.030
			[0.045]
Baseline Yields		0.004***	0.004***
		[0.000]	[0.001]
Baseline Yields*SMS			0.000
			[0.001]
Mean Y Control	0.795	0.795	0.795
Observations	2327	2327	2327

Notes: All the regressions include field-level stratification dummies (wave, plant cycle, macro-zone, baseline average productivity). Standard errors are clustered at the field level. *p<0.1, **p<0.05, ***p<0.01.

Table A.2: SMS (Round 2): Treatment Effect on the Probability of Completing the Harvest Cycle

	Eligible		Non-Eligible	
	(1)	(2)	(3)	(4)
SMS	-0.018	-0.028	-0.038	-0.046**
	[0.022]	[0.021]	[0.024]	[0.023]
Mean Y Control	0.773	0.773	0.766	0.766
Controls	N	Y	N	Y
Observations	3768	3768	4313	4313

Notes: All the regressions include field-level stratification dummies (wave, plant cycle, macro-zone, baseline average productivity). Standard errors are clustered at the field level. *p<0.1, **p<0.05, ***p<0.01.