The Role of Price Information in Agricultural Markets: Experimental Evidence from Rural Peru

Eduardo Nakasone

International Food Policy Research Institute (IFPRI)
Markets, Trade and Institutions Division
2033 K St NW
Washington, DC 20006.

University of Maryland
Department of Agricultural and Resource Economics
Symons Hall, Rm 2200
College Park, MD 20742

Selected Paper prepared for presentation at the Agricultural & Applied Economics Association's 2013 AAEA & CAES Joint Annual Meeting, Washington, DC, August 4-6, 2013.

The Role of Price Information in Agricultural Markets: Experimental Evidence from Rural Peru

Eduardo Nakasone*

University of Maryland and IFPRI

Preliminary and Incomplete. Please do not cite.

This version: May 2013

Abstract

This paper presents new experimental evidence on the role of price information in agricultural markets. For this purpose, I set up a Randomized Control Trial (RCT) in the central highlands of Peru. A group of farmers in randomly selected villages got access to detailed price information for the most relevant local crops in six regional markets through cell phone SMS. The information was delivered throughout the four-month period immediately after harvest, where they sell most of their production. I find that the beneficiaries got higher sales prices for their products, compared to households in the control group. The effect is robust to different specifications. I also find that this effect was mostly driven by increases in the prices for relatively more perishable crops, for which information could be more valuable. Additionally, information made farmers more likely to sell their production (extensive margin). Albeit not statistically significant, the estimate for sales on the intensive margin are positive and quite large. Finally, I also investigate the possibility of information spillovers by examining marketing outcomes of households who did not receive the information but lived in villages where others did. I do not find any significant effects among households in this group.

* *Doctoral student at the Department of Agricultural and Resource Economics, University of Maryland. Senior Research Analyst at the Markets, Trade and Institutions Division of the International Food Policy Research Institute. Address: 2033 K St NW. Washington, DC 20006. E-mail: e.nakasone@cgiar.org.* *I thank Maximo Torero, Ken Leonard, Vivian Hoffman, Carlos Martins-Filho, Miguel Almanzar and Veronica Frisancho for their comments. All remaining errors are mine.*

1 Introduction

There is consensus regarding the adverse consequences that imperfect information can have on market performance and welfare. Though not exclusive, such imperfections seem to be especially prevalent in developing countries, where communication technologies and infrastructure are mostly deficient. There is also a belief that, within developing countries, information imperfections are particularly acute in agricultural markets and primarily affect small farmers. In particular, there is the notion that these small farmers — usually living in remote areas and without access to adequate infrastructure — are less informed about market conditions. As they sell their products to middlemen, they face (among others) one considerable disadvantage: much more informed traders can exploit this information asymmetry and pay lower farm gate prices. In this spirit, there is an interest to understand if enhanced market price information can increase farmers' sales prices. All in all, the evidence has been somewhat mixed: while Svensson & Yanagizawa (2009) and Goyal (2010) find positive impacts; Fafchamps & Minten (2012), Aker & Fafchamps (2011), and Mitra, Mookherjee, Torero & Visaria (2013) do not find any effect.

I provide new experimental evidence about the role of agricultural information on marketing outcomes. I conducted a field experiment in the central highlands of Peru where I randomly allocated price information among agricultural households in 58 villages. Twenty seven villages were assigned to the treatment group, while the others remained as controls. Within villages in the treatment group, I randomly provided cell phones to around 110 households.

I collected detailed price information for seventeen different crops by quality in six different relevant markets. Those who received cell phones were sent price information through Short Message Service (SMS) for four months, immediately after the rainy season in the highlands. This is the period in which farmers have already harvested their crops and make most of their sales decisions. Therefore, the intervention allows me to capture the effects of price information on marketing strategies, isolated from any production decisions. To make information more digestable — rather than providing a massive number of SMS — farmers only received information for the crops they harvested.

The intervention also ensured that the farmers solely benefited from enhanced market price information. In general, mobile phones provide users with a wide array of commercial benefits, besides access to price information (e.g. they facilitate coordination, direct bargaining of sales conditions with clients; arrangements with input providers; collaboration with other producers, etc.). To avoid these parallel benefits, the devices provided to farmers had an important service restriction. At least for the duration of the intervention, these devices were only able to receive SMS and calls from a phone number managed by the project. Participants were able to keep the devices as pre-paid phones with no further obligation after this period.

Within this setting, I test two hypotheses. First, I analyze the causal effect of famers' access to market price information on their sales prices. For this purpose, I compare the prices of the beneficiaries who directly received the price information through their cell phones with those of households in the control villages. Second, I investigate if there are any spillover effects of information. To examine this possibility, I investigate the marketing outcomes of households who did not receive any price SMS, but lived in villages where others did. The idea is that those in this group might have been exposed indirectly to the price information, even when they did not receive it directly.

This paper presents four main contributions to the literature relating price information and farmers' agricultural market performance. First, I am able to isolate the short-run effect on farmers' marketing strategy by appropriately phasing the timeline of the intervention. Second, as opposed to some previous work that has focused on Information and Communication Technologies in general, the nature of the intervention allows me to disentangle the sole effect of market price information (stripped from any other potential benefit). Third, in contrast to previous papers who restrict their attention on households that had previous access to a certain technology (e.g. previous cell phone ownership, radio, etc.), this intervention encompassed the provision of such technology. This allows me to explore to what extent selection bias may have led previous results, since households with previous access to technology tend to be wealthier and more educated. Fourth, this paper improves the contents provided to farmers, which can provide guidance as to what type and level of detail would boost the impact of future price information policies. The price information was very detailed, household-specific and provided in a digested way.

Preliminary results suggest that price information has a large and sizeable impact: farmers who directly received the information experienced 13%-14% increases in their sales prices. This result is robust to different specifications and variations in the sample. This effect was mostly driven by increases in prices for relatively more perishable products (for which information is more valuable). Consistent with higher prices, farmers with information also experienced increases in sales. Among households who received the price information, there was an increase of about 12% in the probability of engaging on a commercial transaction for their crops (regardless of the quantity traded). This suggests that the treatment had important effects on the extensive margin. On the extensive margin (i.e. the volume sold by households who report a commercial transaction), the estimate for the information was quite large (19%), but not statistically significant.

I find no differential effects by previous ownership of a cell phone. This suggests that those less familiar with this technology can also benefit from a price dissemination policy. All in all, I do not find any evidence to support the presence of spillover effects: there are no apparent price benefits to farmers who did not receive the information directly but were in villages where someone else did. Even when village-level spillovers might be somewhat broad areas for information exchange, this result is consistent when I refine potential areas for social interaction (e.g. geographic distance, crop restrictions, etc.). This work will be extended to analyze if price information also had impacts on farmers' marketing channels (i.e. whether they improve their bargaining position against a middleman, sell to a different middleman or directly sell their products in markets).

The remainder of the paper is organized in four sections. Section 2 discusses some of the related literature on the impact of market price information in rural areas of developing countries. Section 3 describes the RCT in the central highlands of Peru. Section 4 presents a simple theoretical model to frame some of the impact of information on marketing outcomes. Section 5 presents the empirical strategy and some preliminary results. Finally, Section 6 contains some concluding remarks.

2 Related Literature

This section presents a brief discussion of the recent literature thata analyzes the impact of market price information on market performace in developing countries. A first group of papers have analyzed the availability of mobile phone service to improve the functioning of rural markets in developing countries. The idea is that, among other types of information, mobile phones can significantly facilitate timely access to market prices and unexploited opportunities to sell / buy goods. In this spirit, Jensen (2007) analyzes the introduction of mobile phone service among fishermen in Kerala. He finds that this led to compliance with the law of one price across different markets, fewer wasted fish and a reduction in prices. On the demand side, this price reduction increased consumers' surplus. On the supply side, price reductions were dwarfed by growth in sale volumes (from reduced wastage), so fishermen's surplus increased as well. Aker (2010) studies the rollout of mobile service coverage in Niger. Using data from national markets and traders, she finds that mobile service reduced price dispersion between millet markets and increased middlemen's profits. Later studies show that these benefits might not have translated into improvements for farmers, though. In a complementary study, Aker & Fafchamps (2011) find that mobile phones did not lead to increases in cowpea prices for producers in the same context. However, the authors do find evidence of reduced intra-annual price variability. Muto & Yamano (2009) use a household panel dataset to identify the impact of cell phone coverage on farmers' participation in maize and banana markets in Uganda. They find that mobile coverage has a positive impact on the sales of bananas but no effect for maize. They argue that these results might be driven by the higher perishability of the former crop compared to the latter.

A secong group of papers have analyzed the impact of other types of Information and Communication Technologies (ICTs) in rural contexts. Svensson & Yanagizawa (2009) study the impact of the Market Information System (MIS) in Uganda, which disseminated agricultural prices through radio stations. Exploiting cross-sectional data, the authors compare households with and without radios in districts that were and were not covered by MIS. They find that access to information increased farm-gate prices for maize by 10%-15%. Goyal (2010) investigates the impact of internet kiosks installed by a large processor in Madhya Pradesh, India; which provided soybean price information. She finds that this led to an increase of 1-3% in the prices received by farmers. It also increased the farmers' land allocated to soybeans by 19%, suggesting a substitution away from other crops.

All these papers rely on quasi-experimental data, where the variation comes from the introduction of different technologies. This literature has been contested by a third group of papers that set up interventions based on randomized controlled trials (RCT). First, Futch & McIntosh (2009) envisaged an experimental evaluation of a village phone program in Rwanda. In their pipeline design, villages were randomly assigned different times of phone installation. Unfortunately, after the baseline, actual phone installation considerably deviated from the original design. The analysis of this (non-random) data suggests that the project had no significant effect on agricultural prices[1]. They posit that — given that the diversion from the original design created a bias in favor of the control group — these estimates are probably upwardly biased and, thus, cannot explain the lack of impact. They argue this negligible impact might be explained by the presence of a very similar program that was previously set in place in most of their area of study. Second, Fafchamps & Minten (2012) conducted a field experiment providing one-year free subscriptions to an SMS-based agricultural information service (provided by Reuters Market Light) in Maharashtra, India.

[1]While the focus of their study was the microenterprise performance, they did gather information about agricultural prices in a community survey.

The subscriptions were randomly allocated among farmers who already had cell phones in this region. The service included price information in different markets, as well as weather forecasts and crop advisory. They find that such service did not lead to increases in agricultural prices for those who received it. Third, Mitra et al. (2013) study the impact of price information on potato farmers in West Bengal, India. They test the efficacy of two alternative strategies for market price dissemination: a private one where a group of randomly selected farmers received SMSs with this information and a public one where prices where posted in public notive boards in some villages. The authors find that neither of these strategies improved farmers' market performance.

I provide new experimental evidence about the role of agricultural information on marketing outcomes. The evidence is novel with respect to the previous literature in at least four respects. First, I focus on the sole impact of information on farmers' marketing outcomes. Through important restriction services on the devices, I rule out the effect of any parallel benefits of the mobile phones (e.g. facilitate coordination, direct bargaining and discussion of sales conditions with clients; arrangements with input providers; collaboration with other producers, etc.) and make sure that market price information is the only mechanism in play. I do not provide any other benefits with the intervention (e.g. cropping advice, weather forecasts, etc.) which could hamper a clean identification of this effect. Second, the timing of the intervention allows me to investigate the short-term impact of information on marketing decisions, isolated from changes in production patterns. Third, I provided information that was very detailed, specifically relevant to each household and presented in a digestable format. Fourth, even with respect to the recent experimental literature, I can rule out potential sample selection that emerges from restricting the intervention to those who already had mobile phones.

The design of my RCT also allows me to investigate if there are any spillover effects of information[2]. The idea of this potential effect has been present for a while. Though applied to a consumer problem, in the early sixties, Stigler had already noted that: "Information is pooled when two buyers compare prices: if each buyer canvasses s sellers, by combining they effectively canvas $2s$ sellers, duplications aside... in fact, pooling can be looked upon as a cheaper form of search" Stigler (1961, p. 219). While others have also laid out the same idea[3], no empirical evidence to support it has been provided so far. To examine this possibility, I exploit the fact that the treatment was randomized at the village level in the first stage. In particular, I investigate the marketing outcomes of households who did not receive any price SMS, but lived in villages where others did. The idea is that those in this group might have been exposed indirectly to the price information, even when they did not receive such information directly.

3 The Intervention

The main problem with disentangling the causal effect of the impact of agricultural information on marketing decisions is the endogenous nature of this relationship. In a non-experimental setting, assume that one finds that access to information leads to better sales outcomes. This relationship could be driven by any number of factors and not necessarily by the information itself. For example, the ones seeking information may be precisely those who find more profitable to do so, may have better entrepreneurial skills or may be more market-oriented. In this sense, this relationship would be merely correlational and not causal.

To tackle this obstacle, I conducted a field experiment. The experiment randomly allocated

[2] This question in closely related to technology adoption through social networks. See: Besley & Case (1993), Foster & Rosenzweig (1995), Munshi (2004), Bandiera & Rasul (2006), Conley & Udry (2010), and Duflo, Kremer & Robinson (2010), among others.

[3] For example, Muto & Yamano (2009, p. 1888) argue for Uganda that: "Note that only one banana farmer needs to have access to a mobile phone to benefit from this new arrangement because the person can make arrangements for fellow farmers in the village and act as an intermediary. Indeed this is a typical arrangement according to our field interviews with banana traders"

cell phones to some farmers in the central highlands of Peru. Through these cell phones, I provided price information in nearby markets for the main crops in this region. Farmers received this information for four months, throughout the period during which they sell most of their agricultural production. Hence, the intervention provides me with exogenous variation in access to information among similar households. The objective is to investigate whether this information leads to better marketing outcomes.

The intervention took place in the five provinces of the Mantaro Valley in the Central Highlands of Peru (Figure 1). This valley has several ideal features. First, it has fertile soils and is one of the most productive areas in Peru. Second, it has considerable presence of small landholders. Third, it is a relatively dynamic commercial setting. Among others, there are three large permanent markets and three important weekly trade fairs or *ferias*. Fourth, sales in this area are standardized by quality for each product: prices are higher for first quality (usually larger and with better appearance) than for second, third or fourth qualities. There is agreement between buyers and sellers about these qualities, and both can readily identify them.

An important characteristic of this area is the agricultural year (see Figure 2). Farmers in the highlands of Peru usually sow their crops around mid-November, at the start of the rainy season. The rainy season typically extends until March or April. The growing periods vary between different products, but harvest is generally between late March and May. For farmers without irrigation, this is their only cropping cycle in the year and an important source of income. Those with irrigation can start an additional cropping cycle in May or June. However, even those with irrigation take advantage of the rainy season, which yields their largest production in the year.

I selected 58 villages in the Mantaro Valley that met the following criteria in the 2007 Peruvian Census: (a) were in the highlands, (b) were in a rural area, (c) had at least 60 households, (d) had at most 35% of cell phone coverage[4]. Data from a random sample of households in each of these villages was collected in December 2009, when the rainy season had already started and farmers had already sown their crops for the 2009/2010 agricultural cycle. I collected information about socio-economic characteristics (household composition, education, income, expenditures, etc.), agricultural land, social networks (participation in organizations) and location (GPS location of dwelling and main agricultural plot). Importantly, I gathered retrospective data about their previous (2008/2009) agricultural cycle: production, sales volume, prices, and marketing decisions. The questionnaire also asked them which products they had already planted for the 2009/2010 season.

The baseline survey included 790 households in the 58 villages where the intervention took place. Rather than randomly allocating the cell phones among the full roster of households, the villages were assigned either to a treatment or a control groups in a first stage (Figure 3) . This initial assignment of treatment by cluster has two advantages. First, it minimized the risk of contamination of the control group - if treatment and control households were in the same village, this would increase the possibility of beneficiaries passing price information along to control households. Second, this provides a framework to investigate the existence of spillover effects in the treatment villages. Thus, the 58 villages were were randomly assigned to a treatment (27) and a control (31) group.

There were 410 households in the treatment villages, from which 111 were randomly selected to receive a cell phone. These cell phones were handed out even when the household already had one. The devices were distributed in early April, during the early harvest. For four months (mid-April to mid-August), a team of undergraduate students collected price information of 17 different products (by quality): peas, lima beans, barley, four types of corn, two types of *olluco* (a popular Andean tuber), and eight types of potato. The information was gathered in three permanent markets (Huancayo, Jauja and Tarma) and three

[4] While the rates in the 2007 Census were substantially lower, I found that cell phone penetration had already reached about 50% during the intervention.

weekly *ferias* (Chupaca, Huayucachi and Zapallanga). The calendar of price distribution is presented in Table 1. Once the information was collected, it was compared with the list of products that households planted for the 2009/2010 season according to the baseline information. During the same morning, only the information of the relevant products for each participant was sent through SMS to the number of the cell phone the intervention provided. An example of a text message with price information is presented in Figure 4a. The text message included the date, market, product, quality and price quote.

I tried to ensure that participants understood the information they were being sent. Along with the devices, the participants were provided with two manuals. The first one explained how to use the cell phone[5]. The second one had explanations on the price information that would be sent out. It included a calendar with the weekdays in which information for each market would be distributed and detailed instructions on how to read the text messages with the prices. They also received a chart to help them keep track of the prices they received (Figure 4b). The team went through the manuals with each participant and answered any questions doubts they had.

The participants were informed of an important service restriction: during the first few months (until late August), their mobiles would only receive calls and text messages from a number authorized by the project. Through this restriction, I can rule out any other potential uses of the mobile phone that could drive the results (i.e. communication with input providers, collusion with other producers, coordination with traders, etc.). In this way, the treatment does not encompass the full advantages of a mobile phone, but only being able to receive price information in different markets. Participants were also required to answer periodic calls to check if there were any problems with the devices, whether the price SMS were being delivered appropriately, and whether they had any problems

[5]Beneficiaries were expected to be able to use a cell phone, either by themselves or had someone else in the household who could help them. However, just in case, they were also provided with a manual - with pictures and detailed instructions - of their basic functions (how to charge them, how to know if there are any new text messages, how to open them, etc.).

reading the information. All in all, besides being able to receive periodic check-up calls, these devices did not have any capabilities beyond those of a pager during the intervention period. However, after August, full capabilities of the cell phones would be restored and they would operate as regular pre-paid phones. Participants were told they would be able to keep the devices without any further obligation. These phones were distributed to all selected households, even to those who already owned one. No one who was offered a cell phone declined to participate in the project.

In September 2010, a follow-up survey was conducted. The questionnaire included information about production, sales volumes and prices in the 2009/2010 agricultural season. This provides me with a panel of households, where I can compare the outcomes of the 2008/2009 (before the intervention) and 2009/2010 agricultural season (after the intervention) among those who received the intervention *vis-à-vis* those who did not. This analysis is provided in the following sections.

4 Theoretical Model

This section presents a simple theoretical model to understand the role that information deficiencies can play in agricultural marketing decisions. It is framed in a negotiation between a farmer and a trader. The model highlights the role of information asymmetry when traders are more informed about market prices than traders. It explains how the intervention (by making price information more readily available to farmers, and therefore making it more symmetric between parties) can have an important role on marketing outcomes.

I initially present a negotiation model with no information problems. Subsequently, these results are compared to a case where the trader is more informed than the farmer. The comparison between the latter and the former scenarios provide a notion of why the intervention can alter sales decisions in this setting.

Suppose a farmer and a trader face uncertain market prices for an agricultural product.

For simplicity, assume there are two possible states of nature: the market price is either high (p_H) with probability λ or low (p_L) with probability $(1-\lambda)$, with $0 < \lambda < 1$. Before the market prices are unveiled, both parties establish a contract. Such contract determines the sales quantity (s_i) and total payment (Y_i) the farmer would receive for each state of nature, where $i = H, L$. Assume that the farmer offers a contract to the trader, establishing combinations Y_H, s_H (if the market price is high) and Y_L, s_L (if it is low). The trader can either accept or reject it. If he rejects the contract, there is no sale. If he does accept it, the parties verfy if the state was H or L and the corresponding combination is enforced.

The farmer has a fixed production of $\overline{Q}$ units of the agricultural products. He sells s_i units of the product to the trader and the remaining $\overline{Q}-s_i$ units are destined to self-consumption. Nervertheless, there is a limit of a units that can be self-consumed by the household, such that $\overline{Q} - s_i \leqslant a$ for $i = H, L$. The particular value of a depends on each crop: a is smaller for perishable crops that might rot before they can be fully consumed by the household. Also suppose the farmer has a quasilinear utility function: $Y_i + u(\overline{Q} - s_i)$, with $u'(.) \geq 0$ and $u''(.) \leq 0$. The trader earns: $p_i s_i - Y_i$. With no agreement, trader gets 0 and farmer gets $u(\bar{Q})$.

4.1 Symmetric Information

First, I consider a benchmark situation in which the farmer and the trader can both observe the market prices after establishing the contract. The farmer's objective is to maximize his utility, subject to the individual rationality constraints that would make him accept the contract.

$$\underset{s_H,Y_H,s_L,Y_L}{MAX} \lambda \left[Y_H + u(\overline{Q} - s_H)\right] + (1-\lambda)\left[Y_L + u(\overline{Q} - s_L)\right] \tag{1a}$$

$$s.t. \; p_H s_H - Y_H \geq 0 \tag{1b}$$

$$p_L s_L - Y_L \geq 0 \tag{1c}$$

$$\overline{Q} - s_H \leq a \tag{1d}$$

$$\overline{Q} - s_L \leq a \tag{1e}$$

In this case, it is straightforward to see that the farmer will push the trader to his reservation utility in both market price scnearios[6], so constraints (1b) and (1c) bind with equality. When the self-consumption constraints (1d) and (1e) are not binding, the farmer offers:

$$s_H^{SI} = \overline{Q} - u'^{-1}(p_H); \quad Y_H^{SI} = p_H\left[\overline{Q} - u'^{-1}(p_H)\right] \qquad \text{if the price is high} \tag{2a}$$

$$s_L^{SI} = \overline{Q} - u'^{-1}(p_L); \quad Y_L^{SI} = p_L\left[\overline{Q} - u'^{-1}(p_L)\right] \qquad \text{if the price is low} \tag{2b}$$

In this case, the implicit farm-gate prices in the contract ($r_i^{SI} = \frac{Y_i^{SI}}{s_i^{SI}} = p_i$ for $i = L, H$) are precisely those prevaling in the market.

More perishable products are more likely to face quantity restrictions for self-consumption. There are two possibilities in this case[7]. First, denote $\left\{(s_H^{SI}\text{'}, Y_H^{SI}\text{'}); (s_L^{SI}\text{'}, Y_L^{SI}\text{'})\right\}$ as the optimal contract offered by the farmer if (1e) binds and (1d) does not. Then,

$$s_H^{SI}\text{'} = \overline{Q} - u'^{-1}(p_H);\; Y_H^{SI}\text{'} = p_H\left[\overline{Q} - u'^{-1}(p_H)\right];\; r_H^{SI}\text{'} = p_H \;\text{ if the price is high} \tag{3a}$$

$$s_L^{SI}\text{'} = \overline{Q} - a;\; Y_L^{SI}\text{'} = p_L\left[\overline{Q} - a\right];\; r_L^{SI}\text{'} = p_L \;\text{ if the price is low} \tag{3b}$$

---

[6]For simplicity, assume that, when the trader is indifferent between accepting or rejecting the farmer's offer, he will accept the contract.

[7]Note that if (1d) is not binding, (1e) can not be either. If this was the case, then $u'(a) > P_H > P_L = u'(\bar{Q} - S_L)$. But the concavity of $u(.)$ would imply that $a < \bar{Q} - S_L$.

Analogously, when both (1d) and (1e) bind, the optimal contract offered by the farmer is provided by $\left\{(s_H^{SI"}, Y_H^{SI"}); (s_L^{SI"}, Y_L^{SI"})\right\}$.

$$s_H^{SI"} = \overline{Q} - a;\ Y_H^{SI"} = p_H \left[\overline{Q} - a\right];\ r_H^{SI"} = p_H \ \text{ if the price is high} \tag{4a}$$

$$s_L^{SI"} = \overline{Q} - a;\ Y_L^{SI"} = p_L \left[\overline{Q} - a\right];\ r_L^{SI"} = p_L \ \text{ if the price is low} \tag{4b}$$

In general, even when there are restrictions on the quantities for self-consumption, the farmer would still get the prevailing market prices due to the symmetric information.

4.2 Asymmetric Information

Now suppose that there is asymmetric information. The contract is established before the market price is known by the agents. However, once the market price is unveiled, only the trader can observe it and the farmer has to rely on what the trader reports to him . If prices turn out to be high, note that both the farmer and trader know that if they use the contract from the Symmetric Information case, the trader has an incentive to report that prices are low. The farmer's objective is to establish a contract that encourages the trader to reveal the state of nature truthfully.

Therefore, this is a model of hidden information. The farmer solves the following problem:

$$\underset{Y_H, Y_L, s_H, s_L}{MAX} \quad \lambda \left[Y_H + u(\overline{Q} - s_H)\right] + (1-\lambda)\left[Y_L + u(\overline{Q} - s_L)\right] \tag{5a}$$

subject to restrictions on the maximum quantity that can be allocated to self-consumption,

$$\overline{Q} - s_H \leq a \tag{5b}$$

$$\overline{Q} - s_L \leq a \tag{5c}$$

individual Rationality (IR) constraints that ensure that the trader is provided with his reservation utility under both states of nature (and would be willing to accept the contract),

$$p_H s_H - Y_H \geq 0 \tag{5d}$$

$$p_L s_L - Y_L \geq 0 \tag{5e}$$

and the following Incentive Compatibility (IC) constraints:

$$p_H s_H - Y_H \geq p_H s_L - Y_L \tag{5f}$$

$$p_L s_L - Y_L \geq p_L s_H - Y_H \tag{5g}$$

IC constraint (5f) states that if p_H is the pevailing market price, the trader is better off revealing the true outcome (and enforcing combination s_H, Y_H) rather than cheating (and enforcing combination s_L, Y_L). Constraint (5g) works analogously for low market prices.

In an optimum, (5e) and (5f) should bind with equality, while (5d) and (5g) are slack conditions of the problem[8]. Then, there are three possibilities depending on the perishability of the product:

1. Neither (5b) nor (5c) is binding;
2. (5c) is binding while (5b) is not;
3. (5b) and (5c) are both binding.

First, assume that the crop has a low degree of perishability and poses no limits on self-consumption levels, i.e. neither (5b) nor (5c) is binding. Denote $\left\{(s_H^{AI}, Y_H^{AI});\ (s_L^{AI}, Y_L^{AI})\right\}$ as the optimal contract offered by the farmer in this situation and $r_H^{AI} = \frac{Y_H^{AI}}{s_H^{AI}}$ and $r_L^{AI} = \frac{Y_L^{AI}}{s_L^{AI}}$ as the implicit per-unit farm gate prices when market prices are high and low, respectively.

[8]Note that, constraints (5f) and (5e) imply that: $p_H s_H - Y_H \geq p_H s_L - Y_L \geq p_L s_L - Y_L \geq 0$, so (5d) is redundant. To solve the problem, I initially solve the problem ignoring (5g). It can be shown later that an optimal solution complies with this constraint.

Solving and comparing with (2a) and (2b) for high market prices yield:

$$
\begin{aligned}
s_H^{AI} &= \bar{Q} - u'^{-1}(p_H) = s_H^{SI} \\
Y_H^{AI} &= p_H\left[\bar{Q} - u'^{-1}(p_H)\right] - (p_H - p_L)\left[\bar{Q} - u'^{-1}(\frac{p_L - \lambda p_H}{1-\lambda})\right] < Y_H^{SI} \\
r_H^{AI} &= p_H - (p_H - p_L)\left[\frac{\bar{Q} - u'^{-1}(\frac{p_L - \lambda p_H}{1-\lambda})}{\bar{Q} - u'^{-1}(p_H)}\right] < r_H^{SI}
\end{aligned} \tag{6a}
$$

Analogously, for low prices:

$$
\begin{aligned}
s_L^{AI} &= \bar{Q} - u'^{-1}(\frac{p_L - \lambda p_H}{1-\lambda}) < s_L^{SI} \\
Y_L^{AI} &= p_L\left[\bar{Q} - u'^{-1}(\frac{p_L - \lambda p_H}{1-\lambda})\right] < Y_L^{SI} \\
r_H^{AI} &= p_L = r_L^{SI}
\end{aligned} \tag{6b}
$$

Under the optimal contract with asymmetric information, the farmer uses the implicit farm-gate prices and quantities as instruments to find out the true state of nature. If prices are high, the trader gets a price premium (the farmer sells at a lower farm-gate price). These informational rents induce the trader to reveal that the market price is high. The quantity sold to the trader remains the same as the one with symmetric information. In contrast, when market prices are low, the trader cannot exploit any informational rents: the farm gate price remains p_L. However, the farmer reduces the quantity he sells under asymmetric information. If the trader wants to lie and claim that prices are low (when they are actually high), the farmer limits his supply to reduce the trader's profits, reducing his incentives to cheat.

If the product is relatively more perishable, then one possibility is that the self-consumption constraint holds when the price is low but not when it is high: i.e. (5c) is binding while

(5b) is not. If this is the case, then solving for the optimal contract yield:

$$\begin{aligned} s_H^{AI\prime} &= \overline{Q} - u'^{-1}(p_H) = s_H^{SI\prime} \\ Y_H^{AI\prime} &= p_H \left[Q - u'^{-1}(p_H) \right] - (P_H - P_L) \left[\overline{Q} - a \right] < Y_H^{SI\prime} \\ r_H^{AI\prime} &= p_H - (P_H - P_L) \left[\frac{\overline{Q} - a}{\overline{Q} - u'^{-1}(p_H)} \right] < r_H^{SI\prime} \end{aligned} \tag{7a}$$

$$\begin{aligned} s_L^{AI\prime} &= \overline{Q} - a = s_L^{SI\prime} \\ Y_L^{AI\prime} &= p_L \left[\overline{Q} - a \right] = Y_L^{SI\prime} \\ r_L^{AI\prime} &= p_L = r_L^{SI\prime} \end{aligned} \tag{7b}$$

Note that, when there are limits to self-consumption and the market prices are low, there is no difference between the symmetric and asymmetric information cases in terms of sales volumes, payments or per unit farm-gate prices. As opposed to the case of asymmetric information without self-consumption restrictions, the farmer cannot threat the trader with smaller sales volumes because his perishable crops would rot if he does not sell them. Because he can no longer restrict quantities as an incetive for the trader to truthfully reveal the state of nature, he can solely rely on higher rents for the trader when the prices are high. Compare the results of (7a) with those in (3a). Note that, while sales volumes do not change (i.e. $s_H^{AI\prime} = s_H^{SI\prime}$ and $s_L^{AI\prime} = s_L^{SI\prime}$) the per unit price and income gaps (comparing symmetric and asymmetric information) widen when the product is relatively perishable:

$$(Y_H^{SI\prime} - Y_H^{AI\prime}) - (Y_H^{SI} - Y_H^{AI}) = (p_H - p_L) \left[u'^{-1}(\frac{p_L - \lambda p_H}{1 - \lambda}) - a \right] > 0 \tag{8a}$$

$$(r_H^{SI\prime} - r_H^{AI\prime}) - (r_H^{SI} - r_H^{AI}) = (p_H - p_L) \left[\frac{u'^{-1}(\frac{p_L - \lambda p_H}{1 - \lambda}) - a}{\overline{Q} - u'^{-1}(p_H)} \right] > 0 \tag{8b}$$

For even more perishable products where there are restrictions in sales volumes even for high market prices (i.e. where 5c and 5b are binding), it can be shown that the farmer no longer has the ability to have the trader truthfully reveal the market price. The optimal contract in this case would be to offer $s^{AI"} = (\overline{Q} - a)$ units for a payment of $Y^{AI"} = p_L(\overline{Q} - a)$. Thus, even when the offer would entail the same quantities as $s_H^{SI"}$ and $s_L^{SI"}$ in (4a) and (4b), the farmer is only able to get a farm-gate price of $p_L$regardless of the true market value.

4.3 Discussion of the Model and Predictions

In this section, I present a model where a farmer negotiates with a trader a contract to sell his agricultural output. Such contract establishes the quantity and payment (and, implicitly, a per unit farm gate price) for their transaction. For simplicity, the model assumes that there are only two possible states of the nature: either market prices are low (p_L) or high (p_H). The farmer offers the trader two options of quantities (s) and payments (Y): (s_H, Y_H) and (s_L, Y_L). If the trader reports that market prices are high, then the combination (s_H, Y_H) is enforced. Analogously, (s_L, Y_L) is enforced if he reports low market prices. The per unit farm-gate prices for each option are determined implicitly by $r = \frac{Y}{s}$. The farmer keeps the remaining production he has not sold for self-consumption $\bar{Q} - s_H$ or $\bar{Q} - s_L$ in each state.

To highlight the role of asymmetric information on marketing outcomes, I discuss the results of the model under two different scenarios. On one hand, Section (4.1) presents the results of the model when there is symmetric information about market prices between both parties. On the other, Section (4.2) analyzes the case where the farmer is uncertain about market prices, but the trader does know whether the market price is p_H or p_L.

Under symmetric information, the optimal quantities are traded and the farmer sells his production for the actual market prices. However, when there is asymmetric information, the trader has an incentive to cheat by telling the farmer that market prices are low when they are actually high. There are two (costly) mechanisms for the farmer to elicit this information. First, he can offer the trader an informational rent when prices are high allowing him to purchase his crops at a lower per-unit farm gate price. Second, he can restrict the quantity he would sell under low prices (increasing his household's self-conumption), effectively reducing the trader's profits. This leads to a couple of testable hypotheses: per unit farm gate prices for the farmer and sales volumes should increase with improvements of the information on market prices (i.e. $r^{SI} \geq r^{AI}$ and $s^{SI} \geq s^{AI}$).

Nonetheless, there are limits to which the farmer can exert his strategy to elicit market prices : when the products are perishable there is a limited quantity of his production that can be self-consumed before it rots. Perishability, thus, limits his ability to restrict the quantity he offers to the trader. In this line, the model also predicts that the impact of market price information should be larger for perishable products but his sales volumes should not increase as much as with perishable products. The following sections provide an empirical analysis of these predictions.

5 Empirical Strategy and Preliminary Results

This sections presents some preliminary results for two hypotheses: (a) does the direct provision of price information through SMS increase farmers' sales prices and volumes? and (b) are there any spillover effects of this price information within villages? For the former, I compare the changes in marketing outcomes between the 2008/2009 and 2009/2010 agricultural seasons of those who received the SMS directly to those in control villages (where no one received the SMS). For the latter, I compare the changes in outcomes of those who did not receive the SMS but lived in a village where someone else did to those in control villages.

Throughout the analysis, consider the definitions of the following variables:

* *Info* takes a value of 1 if the household is in a treated village and received the price SMS. It takes a value of zero otherwise.
* *Spill* takes a value of 1 if the household is in a treated village but did not receive the price SMS (i.e. excludes *Info=1*) . It takes a value of zero otherwise.

* The remaining households (i.e. those with *Info=0* and *Spill=0*) are those in control villages.

5.1 Baseline Comparisons

In this subsection, I show that the randomization process delivered three similar groups: those who directly received price information, those who lived in treated villages but did not receive information directly, and those in control villages. I compare the baseline characteristics of those who received information and those in the spillover group with respect to the control group, with the following Ordinary Least Squares (OLS) Regression:

$$Y_{i0} = \alpha_0 + \alpha_1 Info_i + \alpha_2 Spill_i + \mu_i \tag{9}$$

where Y_{i0} is a characteristic of the ith household before the intervention and $\mu_i \sim N(0, \sigma^2)$. The coefficients α_1 and α_2 provide estimates of the differences in Y_{i0} of the *Info* and *Spill* groups relative to the control group. Sample means of the information, spillover and control groups — as well as estimates for Equation (9) — are presented in Table 2. The sample is relatively well balanced in terms of characteristics of the household head (age; gender; years and level of education), land, household expenditure, and cell phone ownership (prior to the intervention).

I also analyze the crop distribution in the information, spillover and control groups. Table 3 compares the proportion of households that cultivated seventeen important crops during the 2008/2009 (baseline) agricultural season. The first three columns present the proportion of households that grew each crop in each of the three groups. The last two columns report the differences of the information and spillover groups, relative to the controls. The standard errors of the differences are estimated using a similar approach to that in Equation (9). However, because this variable is binary, I estimate marginal effects from a probit model rather than OLS. While the sample is not balanced for all seventeen crops, it is for the vast majority of them, and the differences are small when significant (for one variety of olluco and one of potato). In any case, all the subsequent analysis will include crop controls.

Next, I present the baseline differences in production, sales volumes and prices among the three groups of interest. It is worth noting that my sample is not stratified by crop, so I cannot draw any inferences from a specific agricultural product. As a matter of fact, if I were to restrict my sample to households who produced the most popular crop in the region (Yungay potato), the sample size would drop by more than half. Thus, rather than drawing specific comparisons for each product, I take advantage of the full sample and estimate a regression with crop fixed effects: this comparison exploits the variations between treatment groups within each crop. For this purpose, I estimate the following equation:

$$Y_{ic0} = \alpha_0 + \alpha_1 Info_i + \alpha_2 Spill_i + \sum_c \delta_c D_c + \varepsilon_i + \mu_{ic} \tag{10}$$

where Y_{ic0} is the marketing outcome (production, probability of sales, volume of sales and price) of the ith household in the baseline (2008/2009 season) for crop c and D_c is an indicator variable for each crop. The equation allows for correlation of error terms within the same household (across crops) through $\varepsilon_i \sim N(0, \sigma_\varepsilon^2)$. It also includes an error term that varies by household and crop: $\mu_{ic} \sim N(0, \sigma_\mu^2)$. The results for Equation (10) are presented in Table (4). For each outcome, the first column reports estimates using crop controls. The second one includes quality controls as well as crop controls[9]. All in all, they show that households did not exhibit significant differences among treatment statuses before the intervention.

[9]Note that about 16% of the observations drop out of the production regression when we control for quality. This is because farmers do not necessarily sort all their harvest. Production that is sold is necessarily graded by quality. However, households who do not sell (i.e. allocate their harvests to self-consumption, seed, by-products, etc.) do not necessarily do so.

5.2 The Effect of Information on Agricultural Prices

I calculate the impact of the treatment on agricultural prices through a Difference-in-Differences model, including crop (and quality) controls and random effects at the household level. Namely, I estimate the following regression:

$$Log(P_{ict}) = \alpha Info_i + \theta Spill_i + \gamma t + \beta_1 Info_i t + \beta_2 Spill_i t + \sum_c \delta_c D_c + \varepsilon_i + \mu_{ict} \qquad (11)$$

where $Log(P_{ict})$ is the logarithm of the price of household i for crop c in period t. The variable t takes a value of zero for for the 2008/2009 season (before the treatment) and a value of one for the 2009/2010 season (after the treatment). $Info_i$ and $Spill_i$ are the (time-invariant) treatment statuses for each household. D_{ict} is an indicator dummy of whether household i harvested crop c in period t. Additionally, the error term has two components. The first one (ε_i) accounts for the fact that the errors within the same household are not independent from one another. The second one (μ_{ict}) is a purely idiosyncratic error can varies across households, crops and time. In particular, $\mu_{ict} \sim N(0, \sigma_u^2) = 0$ and $\varepsilon_i \sim N(0, \sigma_\varepsilon^2)$. For consistency, this specification requires ε_i to be uncorrelated with other explanatory variables. This is a plausible assumption in this setting because of the random assignment of the treatment[10]. Additionally, standard errors are clustered at the village level to allow for any covariate shocks.

In this framework, β_1 captures the changes (in percentage points) of prices experienced by those who received the information compared to those in the control villages, within each crop. β_2 provides an analogous estimate for those who may have benefited from information spillovers, relative to the control group. The results of this estimation are reported in Table (5). They suggest that there were sizeable impacts for those who benefited directly from the

[10]When the individual effect is uncorrelated with the explanatory variables, both fixed and random effects estimates are consistent. Actually, the fixed effects estimates (not shown) yield very similar results. However, I prefer the random-effect estimate because they are more efficient.

information: prices at which they were able to sell their production increased by 12% (with crop controls) to 13% (with crop and quality controls). The results show little evidence of spillover effects at treated villages: the estimates for β_2 are close to zero and statistically insignificant.

Estimations in Table (5) include all households who report sales either in the baseline or the follow-up survey. However, they do not necessarily sell in both periods. For example, there would not be a price for any transaction in the baseline for a farmer that did not sell any of his output in the 2008/2009 season. This farmer would not appear in the regression in the $t = 0$ period. However, if he did trade any of his production in the 2009/2010 season, he would be included in (11), at least for $t = 1$. Due to the random assignment of the treatment, there are no significant differences in the proportion of housholds selling their output in the baseline (Table 4). However, as I will show later, a higher proportion of households in the treatment group sold at least some of their production when they were provided with price information. A plausible concern is that the treatment prompts different households to appear in the regression for $t = 1$, inducing a bias in the estimates. Thus, I run some alternative specifications.

The most simple approach to address this concern is to estimate equation (11) with a sample of households that sold in both periods. The results are reported in the first two columns of Table 6. The coefficients remain relatively stable and, if anything, they increase slightly (17% with crop controls and 18% with crop and quality controls). The estimates for the spillover groups remain small and indistinguishable from zero. However, households who sell their production in both periods might represent a self-selected group with different characteristics than the original sample. The larger coefficient for the treatment variable suggests that this group might be comprised of farmers with the most marketing experience in the sample (and who would take more advantage of the price information).

I present an additional specification separating households who sold their production in the baseline from those who did not. If the treatment prompts households to sell their production, a bias would arise if those who are induced to sell (and would not have sold otherwise) differ from other farmers and obtained significantly different prices. To test for this possibility, I use the sales information in the follow-up survey and construct two subsamples: those who registered at least one transaction for any of their crops and those who did not. I estimate the following regression for both subsamples:

$$Log(P_{ic,t=1}) = \beta_1 Info_i + \beta_2 Spill_i + \sum_c \delta_c D_c + \varepsilon_i + \mu_{ic} \tag{12}$$

These results are presented in Table (7). The first two column shows the results for all households in the follow-up survey. Because the treatment and control groups were similar in the baseline, I find virtually identical effect of 13%. The subsequent columns show the differences in the price effect for the group who sold in both periods and the one who only sold after the treatment. All in all, it appears that those were encouraged to sell after the treatment (and did not sell before) might not have benefited from the information (the point estimates are about 1%-5% increases, but are statistically insignificant) as much as those who sold in both rounds. One explanation for this is that the new sellers might be as less experienced or less engaged in market activities. If the treatment is differentially attracting more of this "newbies", this would suggest that, if anything, my previous estimates are downwardly biases and underestimate the overall effect of the information.

Another challenge for this results is that a large proportion of households does not harvest the same product in both rounds of the survey. For example, a farmer who grows a low-cost variety of potatoes in the baseline might have shifted his production to a high value commodity (such as peas) in the following round. The estimates would be biased if those in the treatment group were systematically the ones incurring such changes. This is unlikely because of the timing of the intervention: the information was provided close to the harvest season, so it should not have affected planting patterns or altered input use in the crops. To confirm this possibility, I estimate a difference-in-differences regression for the proportion of households in the treatment and control groups that grow each crop:

$$c_{it} = \alpha Info_i + \theta Spill_i + \gamma t + \beta_1 Info_i t + \beta_2 Spill_i t + \mu_i + \varepsilon_{it} \tag{13}$$

where $c_{it} = 1$ if household i grows crop c in period t, and $c_{it} = 0$ otherwise. I show the results for equation (13) in Table (8): out of the seventeen crops in the sample, there were significant changes in the composition in two of them for the treatment and spillover groups (compared to control households). I also confirm this idea with a more restricted (though considerably smaller) sample of households that sold the same items (same product with same quality) in both periods. In this line, I estimate the following regression on my restricted sample through Ordinary Least Squares[11]:

$$Log(P_{ic,t=1}) = \gamma P_{ic,t=0} + \beta_1 Info_i + \beta_2 Spill_i + \epsilon_{ict} \tag{14}$$

Columns 3-5 of Table 6 present the results for regression (14). The results remain relatively stable with coefficients between 0.12 and 0.17 for $\beta_1$and close to zero for β_2. Therefore, even within this smaller sample with a much more narrowly restricted comparison, the results remain relatively stable.

5.3 Effects on Production and Sales

Next, I estimate the impact of price information on production, probability and volume of sales. In particular, I test for two hypotheses. First, due to timing of the intervention (see Section 3), I do not expect the price information to induce any change in harvests. This is what allows me to dissect the pure marketing effect of information from its production incentives. Second, following the theoretical model in Section 4, I examine if there is any positive impact of price information on the probability and volumes of sales.

[11] Note that this specification does not include individual error terms (because $Log(P_{ic,t=0})$ would be correlated to these individual error by construction). Also, the specification requires that $Corr(P_{ic,t=0}, \epsilon_{ict=1}) = 0$, $Corr(\varepsilon_{ic,t=1}, \varepsilon_{ic,t=0}) = 0$ and $|\gamma| < 1$ for convergence.

I follow the procedure described in Equation (11). The estimates are reported in Table (10). As expected, because of the timing of the intervention, the information did not have a significant effect on production volumes (Columns 1-2) of the treatment or spillover groups.

The next two columns present the results for the probability that a household had sold (at least part of) its harvest of a certain crop: the variable takes a value of one if there was any commercial transaction for this crop and a value of zero otherwise. The third column, which includes crop controls, show that those in the treatment group were 12% more like to have engaged in any commercial transaction. When additional quality controls are included in the fourth column, the sample size drops by about one thousand observations: many households that do not sell any of their production do not grade their harvest (i.e. a disproportionate number of zeroes in the variable are dropped). Even in this case, the coefficient is similar to the one in Column 3 (10%), but it is not statistically significant. These estimations seem to support the idea that information has a considerable impact on the extensive margins of sales.

The last two columns show the estimation for sales volumes (within those who report any sales). While the impact of information is not statistically significant, the coefficient is positive and rather large (about 19%) but noisy. Albeit less clear, this suggests that the treatment also had some impact on the extensive margin of sales.

All in all, this evidence lends some support to the idea proposed by the theoretical model. With asymmetric information, traders extract informational rents from farmers and pay them lower prices for their crops. Farmers retaliate and use sales volumes restrictions as a bargaining mechanism against traders. However, when farmers are provided with better price information, they are able to get higher prices and do not need to restrict their sales.

5.4 Heterogeneous Treatment Effects

5.4.1 Differences by Perishability of Crop

The model in Section 4 predicts that improvements of market price information should have different effects for relatively perishable and non-perishable products. The idea is that there is a limit on the amount of the latter that farmers can self-consume before they spoil. This imposes a limit to which farmers can use supply restrictions to obtain better prices from traders. Thus the model predicts that price increases should be larger for perishable products but sold quantities should not increase as much.

This is consistent with previous work that finds that the impact of price information should be more valuable for farmers who sell more perishable crops (e.g. Muto & Yamano 2009, Aker & Fafchamps 2011). While there might be a set of other factors in play (e.g. market structure, context-specific features, etc.), it is possible that differences between perishable and non-perishable crops might also explain why **?**do not find any impact of their price transmission intervention with farmers growing potato (a relatively less perishable crop) in India.

To test for this possibility, I examine the degree of perishability within the seventeen crops in the sample. All in all, there are two that are clearly more perishable than others: lima beans and green peas (which spoil much more qinformation uickly than maize, potatoes and *olluco*). To capture differences in the effect for these groups, I use the following variation of Equation (11):

$$\begin{aligned} Log(Y_{ict}) = & \alpha Info_i + \theta Spill_i + \gamma t + \beta_1 Info_i t + \beta_2 Spill_i t + \\ & \delta_1 Perish_c + \delta_2 Perish_c Info_i + \delta_3 Perish_c Spill_i + \\ & \delta_4 Perish_c t + \delta_5 Perish_c Info_i t + \delta_6 Perish_c Spill_i t + \varepsilon_i + \mu_{ict} \end{aligned} \quad (15)$$

where Y_{ict} is either prices or sales volumes, $Perish_c = 1$ for lima beans and green peas and $Perish_c = 0$ for all other crops. In the case of those who received price information directly, the DID estimators for (relatively) non-perishable and perishable crops are $\beta_1$and $(\beta_1 + \delta_5)$, respectively[12]. Analogously, the DID estimators for the spillover group are β_2 and $(\beta_2 + \delta_6)$.

Results for equation (15) are reported in Table (11). In the regression for prices (in Columns 1 and 2), I find that the effect of the treatment is driven by a large and significant effect on perishable products (δ_5). In fact, the coefficient for other crops is small (β_1) and not distinguishable from zero, while δ_5 is large and statistically significant. However, this is not the case in the regression for sold quantities. Column 3 (which includes controls for crops) shows that, among households with information, increases in sales volumes of perishable crops were smaller than those of the non-perishable ones. But this difference is rather small. However, when controls for crops and quality are included, it becomes more apparent that perishable crops experienced much smaller increases in sales volumes.

Overall, I find that perishable crops hosueholds with information experienced larger price gains and somewhat smaller volume increases for perishable crops. This is consistent with the predictions of the theoretical model.

5.4.2 Differences by (Previous) Cell Phone Ownership

As explained previously, one of the differences with previous papers exploiting RCTs in this area is that I do not restrict the treatment to those who already had a cell phone. In fact, cell phones were distributed regardless of previous ownership, and actually about half of my sample already had one prior to the intervention. This allows me to estimate the effects for both groups. I split my sample in two groups: those who already had a phone and those who did not, and estimate Equation (11) for both. The estimate on the former sample is roughly the one I would have obtained had my treatment been randomized only among those with mobile service. Indeed, because of variations in the intervention and the

[12] Note that δ_5 is a difference-in-difference-in-difference estimator. It is the difference of the DID estimators for the treatment and control groups estimated for perishable and non-perishable crops.

information provided, it is not strictly comparable to those in Fafchamps & Minten's (2012) study. However, they do provide an idea of what would have happened had my intervention (with the variations in the treatment) been restricted like theirs.

These results are presented in Table 12. The coefficients are similar in both groups, providing evidence that this selection is not driving my results. Thus, I posit that the divergence in these results should be due to difference in the particular contexts of the RCTs, the relevance of information provided or how the information was displayed. However, I cannot distinguish among these competing hypotheses.

5.4.3 Additional Regressions for Spillover Effects

The results in the previous sections do not support the presence of strong spillover effects. One possibility is that villages are somewhat broad areas for information exchange: if the nearest neighbor with information is still too far away, there might be no possibility for communication. In this spirit, I provide some estimates that restrict the spillover effect through geographic distances. I collected the GPS position of each household in the baseline that allow me to control for this. For each household living in a treated village but did not receive the market price information, I estimate the distance to each household that directly received the information through an SMS. Within treated villages, I can determine the distance of each household to its nearest neighbor with information within treated villages. With this information, I create quintiles of distance to the nearest source of information. Denote D_q as dummy variables for each of these quintiles, where $q = 1$ is the group with closest neighbors that directly received the information and $q = 5$ is the most distant.

To capture heterogenous treatment effects with respect to the distance of each household to the closest source of information, I estimate the following equation:

$$Log(P_{ict}) = \alpha Info_i + \sum_{q=1}^{5} \theta_q (D_q Spill_i) + \delta t + \beta Info_i t + \sum_{q=1}^{5} \gamma_q (D_q Spill_i t) + \varepsilon_i + \mu_{ict} \quad (16)$$

I present these results in Table 13. If the geographic distance were to play an important role in price transmission, then we would expect $\gamma_1 > \gamma_2 > ... > \gamma_5$. However, the estimated coefficients do not suggest this pattern. All the coefficients are still small and not statistically different from zero.

Another possibility is that lack of spillover effects is driven by crop differences between the group that directly received the information and the one that could potentially benefit from them indirectly. For example, a farmer in treated village might be getting price information for a certain crop. Because there are 17 different relevant crops in the sample, his neighbor (who is not receiving the information) might be harvesting a different product. To account for this, I construct a variable $Match_{it}$ for households in the treated villages that did not receive the information directly. $Match_{itc} = 1$ if any other household in the farmer's village is directly receiving price information for crop c in period t, and takes a value of zero otherwise. I estimate the following equation:

$$\begin{aligned} Log(P_{ict}) &= \alpha Info_i + \delta_0 Spill_i + \delta_1 Spill_i Match_{ict} + \gamma t + \\ &\quad \delta Info_i t + \theta_0 Spill_i t + \theta_1 Spill_i Match_{ict} t + \varepsilon_i + \mu_{ict} \end{aligned} \tag{17}$$

If the previous results — where there was no evidence of spillover effects for the transmission of prices — were driven by product differences, we would expect $\theta_0 = 0$ and $\theta_1 > 0$. However, the results in Table 14 show that both coefficients are small and not statistically significant. This additional piece of evidence seems to confirm the absence of spillover effects and the idea that farmers do not share the market information they receive privately with others.

6 Conclusion

The objective of this paper is to analyze the effect of agricultural price on marketing outcomes. I present a model where a farmer has a fixed production and has to decide how much to sell and how much to keep for self-consumption. He bargains with a trader over the quantity he would sell and the payment he would receive for such sale. It discusses a situation where the trader knows the market prices, but the farmer does not. The only way for the farmer to realize the market prices is through the trader. The model predicts that under asymmetric information the farmer would use two mechanisms for the trader to truthfully reveal the market prices: (a) the farmer restricts the quantity he would sell when the trader tells him that the market prices are low; and (b) pays him an informational rent (through lower farm-gate prices) when he tells him that market prices are high.

I compare these results with the ones under symmetric information. If both parties have access to market price information, there is no need for the farmer to exercise these truth-telling mechanisms. Aa a consequence, both farm-gate prices and sales volumes should increase. It also predicts that, if there are self-consumption limits to perishable products (that cannot be fully consumed within the household before they rot), access to information would have a differential impact on this type of crops. First, increases in sales volumes shold not be as large as with non-perishable products because farmers are not able to considerably restrict their sales volumes. Second, because even with asymmetric information they were not able to use sales restrictions as truth-telling mechanisms, perishable products would experience larger price increases than their counterparts.

For this purpose, I set up a RCT where I give access to price market information to farmers in the central highlands of Peru. The intervention provided cell phones to beneficiaries, allowing those without mobile service to participate. Detailed prices by quality for seventeen important local crops in six different relevant markets were collected. Beneficiaries received this price information through text messages during the four-month period in which most of their commercial activity takes place. To make information more digestable, each farmer only received information for the crops he or she planted.

During the duration of the intervention, the devices had service restrictions by which they could only receive calls and text messages from a number authorized by the project. This number was used to send the text messages with price information. Service restrictions assure that these devices were only acting as means to convey the information that the intervention provided and rule out any other additional cell phone benefits (e.g. set up appointments with input providers, coordination with other producers, bargaining with clients, etc.). In this spirit, my results should be interpreted as the sole effect of having access to price information.

I find that households with access to information are able to get better prices for their crops: their sales prices increase by 13%-14% relative to those of their counterparts. The result is robust to different samples and specifications. These increases in prices concur with increases in sales. On the extensive margin, I find a positive and significant impact of information (about 12%) on the probability that households with information engage in commercial transactions for their crops. On the intensive one, within those that report commercial transactions, traded larger volumes are for households who receive information. This estimate is not statistically significant, but rather large (19%). Consistent with the theoretical model, I find that price increases in the treatment group are mostly driven by perishable crops. In contrast, non-perishable products are somewhat more important to explain increases in sales volumes.

I also analyze if there are any information spillover effects. Direct beneficiaries might have shared the information they received with their neighbors and lead to indirect gains by others. To test for this possibility, I examine the marketing outcomes of households who did not receive the information but lived in villages where others did. All in all, I do not find any significant impact on marketing outcomes among households in this group.

With these results in mind, I propose to extend this work in two areas of action. First, I also collected some information on the relationship with the middleman or market trader (if this is a relative or friend, years of knowing the buyer, if the trader had previously provided them with credit or inputs, etc.). Farmers might engage in repeated games or long-term contractual relationships with traders. One possibility is to examine if better price information of outside options make reneging these agreements more likely. I would try to exploit the data available in the survey to ascertain this hypothesis. Second, I also collected data on farmers' marketing mechanisms (i.e. sales to middlemen and direct market sales in each period). Thus, one possibility is to analyze if the treatment has an impact on prices through enhanced bargaining with middlemen or by making households more likely to go to markets by themselves.

Figure 1: Location of the Intervention

Figure 2: Agricultural Season and Timeline of the Intervention

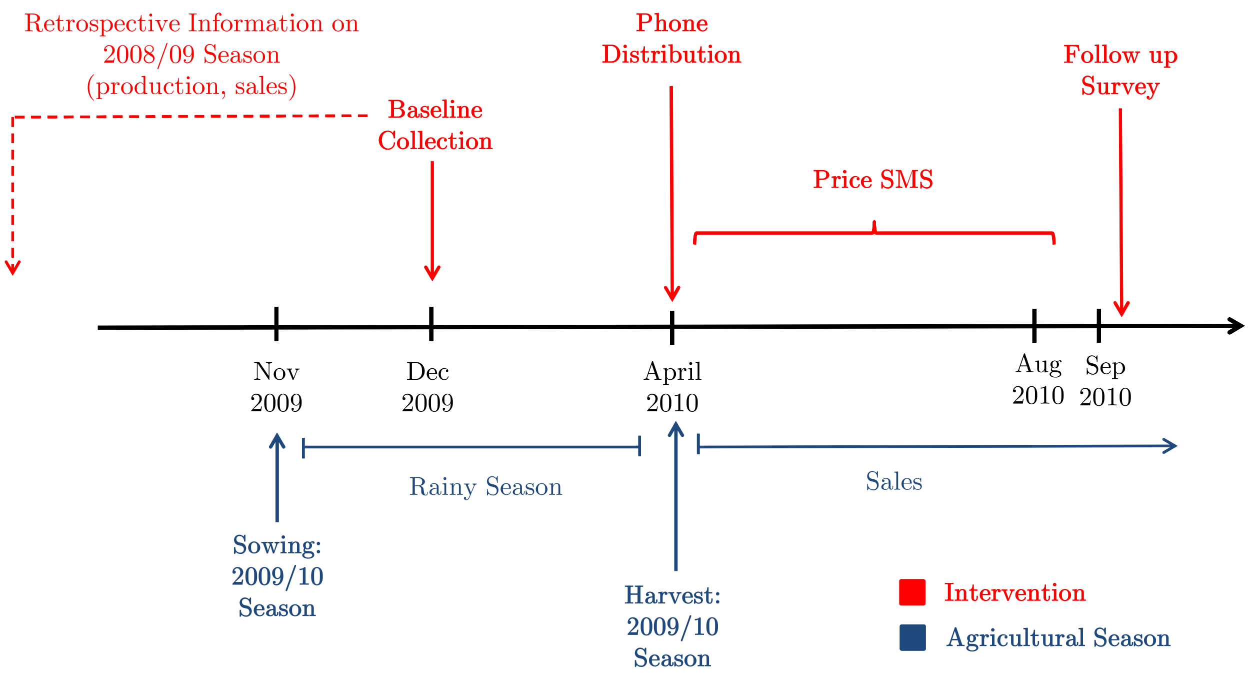

Figure 3: Location of Markets, Treatment and Control Villages

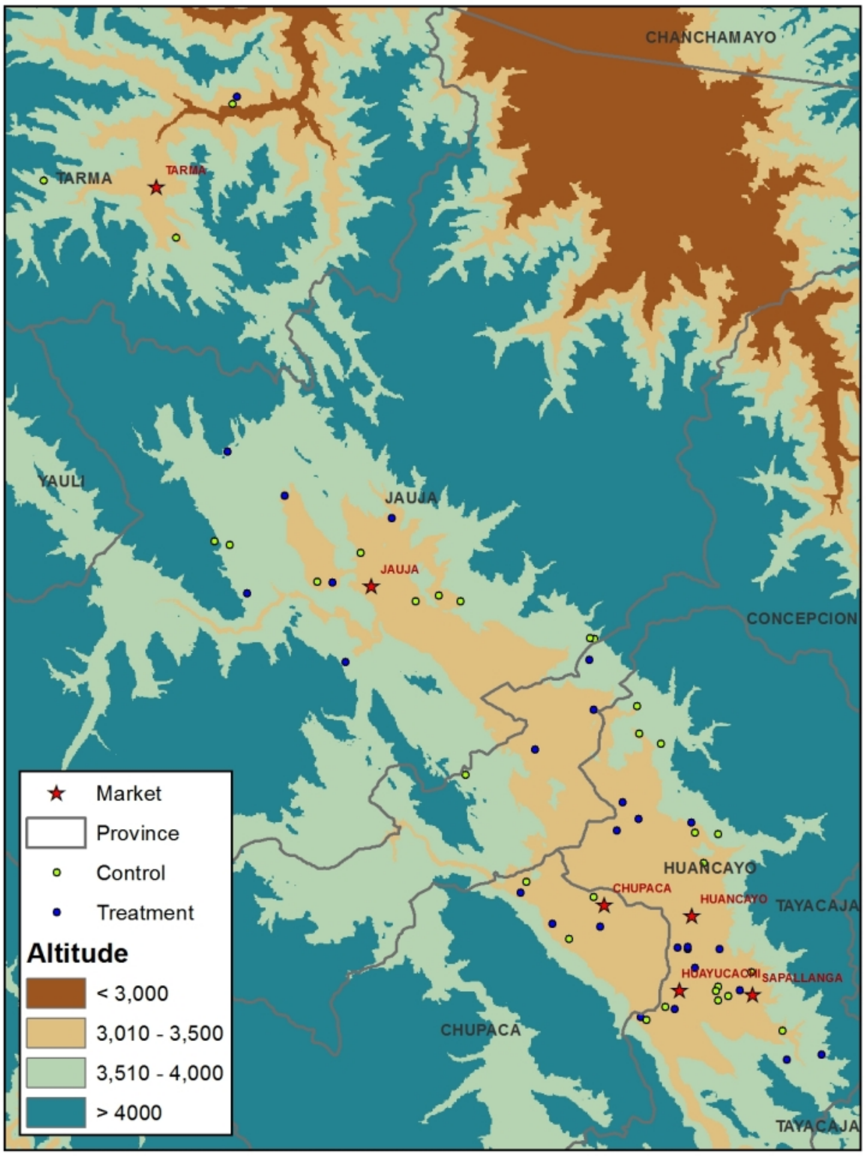

Table 1: Calendar of Price Distribution by Permanent Markets and Ferias

	Mon	Tue	Wed	Thu	Fri	Sat	Sun
Permanent							
Huancayo	X		X		X		
Tarma	X		X		X		
Jauja			X				X
Ferias							
Chupaca						X	
Huayucachi	X						
Zapallanga				X			

Figure 4: Cell Phone and Price Distribution

(a) Example of Price SMS

(b) Distribution kit: Cell Phones, Manuals and Charts

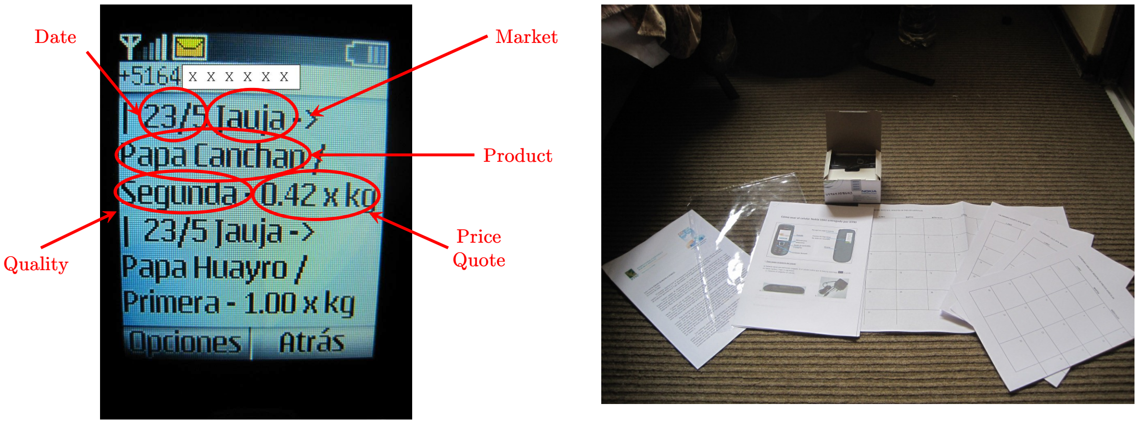

Table 2: Household Characteristics in Baseline

	Info (I)	Spill (S)	Control (C)	Diff[1] (I)-(C)	(S)-(C)
HH Head Characteristics					
Age	50.53	51.45	49.91	0.62	1.54
	(12.81)	(15.62)	(14.54)	(1.69)	(1.77)
Head is male	0.86	0.80	0.84	0.02	-0.05
	(0.34)	(0.40)	(0.37)	(0.04)	(0.03)
Years of education	7.45	6.89	7.51	-0.06	-0.62
	(3.92)	(4.14)	(4.03)	(0.50)	(0.44)
Primary [2]	0.45	0.46	0.45	0.00	0.01
	(0.50)	(0.50)	(0.50)	(0.05)	(0.05)
Secondary [2]	0.42	0.37	0.40	0.02	-0.03
	(0.50)	(0.48)	(0.49)	(0.06)	(0.05)
Technical [2]	0.06	0.06	0.06	0.00	0.00
	(0.24)	(0.23)	(0.23)	(0.03)	(0.02)
College [2]	0.04	0.03	0.05	-0.01	-0.02
	(0.19)	(0.18)	(0.22)	(0.02)	(0.02)
Any member has Cell Phone [2]	0.46	0.50	0.51	-0.05	-0.01
	(0.50)	(0.50)	(0.50)	(0.07)	(0.06)
Log PC HH Exp	4.69	4.61	4.70	-0.01	-0.09
	(0.48)	(0.49)	(0.45)	(0.08)	(0.06)
Log Land	8.37	8.17	8.32	0.05	-0.15
	(1.36)	(1.50)	(1.50)	(0.36)	(0.36)
Has land with irrigation [2] [3]	0.28	0.29	0.26	0.02	0.03
	(0.45)	(0.45)	(0.44)	(0.11)	(0.10)
N	111	299	380		

[1] For the first three columns, the means and standard deviations of each variable in the information, spillover and control groups are reported. In the last two columns, the differences were calculated using the following regression: $Y_i = \alpha_1 Info_i + \alpha_2 Spill_i + \mu_i$. Regression standard errors are reported in parentheses.
[2] In the case of discrete variables the linear regression was replaced for a probit model.
[3] The variable takes a value of one if the household has at least one plot with irrigation.

Significance levels of the differences between the treatment and spillover groups (with respect to the control group) denoted by: *** 1%, ** 5%, * 10% .

Table 3: Crop Compostion in Baseline

	Treat	Spill	Control	Difference[1]	
	(T)	(S)	(C)	(T)-(C)	(S)-(C)
Peas	0.16	0.12	0.16	0.00	-0.04
	(0.37)	(0.33)	(0.24)	(0.08)	(0.06)
Barley (common)	0.30	0.24	0.23	0.06	0.00
	(0.46)	(0.43)	(0.42)	(0.08)	(0.08)
Lima Beans	0.12	0.09	0.17	-0.06	-0.08
	(0.32)	(0.29)	(0.38)	(0.08)	(0.08)
Corn - White	0.42	0.37	0.26	0.17	0.12
	(0.50)	(0.48)	(0.44)	(0.12)	(0.13)
Corn - Cusqueado	0.03	0.03	0.03	0.00	0.00
	(0.16)	(0.17)	(0.17)	(0.02)	(0.02)
Corn - Cusqueno	0.05	0.02	0.01	0.03	0.01
	(0.21)	(0.15)	(0.10)	(0.02)	(0.01)
Corn - San Jeronimo	0.04	0.04	0.02	0.01	0.01
	(0.19)	(0.19)	(0.15)	(0.02)	(0.02)
Olluco - Yellow	0.07	0.06	0.09	-0.02	-0.04
	(0.26)	(0.23)	(0.29)	(0.04)	(0.04)
Olluco - Dotted	0.03	0.01	0.04	-0.02	-0.03
	(0.16)	(0.12)	(0.21)	(0.02)	(0.02)
Potato - Yellow	0.03	0.02	0.01	0.01	0.01
	(0.16)	(0.15)	(0.11)	(0.02)	(0.02)
Potato - Andean	0.02	0.05	0.03	-0.01	0.02
	(0.13)	(0.21)	(0.17)	(0.03)	(0.04)
Potato - Canchan	0.07	0.03	0.07	0.01	-0.03
	(0.26)	(0.18)	(0.25)	(0.03)	(0.02)*
Potato - Huayro	0.02	0.03	0.02	0.00	0.01
	(0.13)	(0.18)	(0.14)	(0.02)	(0.02)
Potato - Perricholi	0.25	0.21	0.35	-0.10	-0.14
	(0.44)	(0.41)	(0.48)	(0.15)	(0.14)
Potato - Peruanita	0.05	0.04	0.01	0.04	0.03
	(0.23)	(0.20)	(0.11)	(0.03)	(0.03)
Potato - Unica	0.01	0.00	0.04	-0.03	-0.03
	(0.09)	(0.06)	(0.19)	(0.02)	(0.02)*
Potato - Yungay	0.41	0.47	0.45	-0.04	0.02
	(0.49)	(0.50)	(0.50)	(0.09)	(0.10)
N	111	299	380		

[1] For the first three columns, the proportion of households that grew each crop is reported (standard deviation in parentheses). In the last two columns, the differences were calculated using a probit model: $Prob[Crop_{ic} = 1] = \Phi(\alpha_1 Info_i + \alpha_2 Spill_i)$ for each crop c. Regression standard errors are reported in parentheses.

Significance levels of the differences between the treatment and spillover groups (with respect to the control group) denoted by: *** 1%, ** 5%, * 10% .

Table 4: Agricultural Production and Sales Comparison in Baseline

	Log Production		Prob Sales[1]		Log Sales		Log Price	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Info	0.10	0.10	0.02	0.02	0.07	0.05	-0.02	-0.01
	(0.12)	(0.13)	(0.04)	(0.04)	(0.15)	(0.16)	(0.04)	(0.04)
Spill	-0.06	0.04	-0.01	0.02	0.02	-0.00	0.01	0.00
	(0.09)	(0.10)	(0.03)	(0.03)	(0.11)	(0.12)	(0.03)	(0.03)
Constant	4.72	5.37	0.47	0.81	5.56	5.88	-0.15	-0.01
	(0.13)***	(0.14)***	(0.05)***	(0.06)***	(0.19)***	(0.18)***	(0.07)**	(0.06)
Product Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quality Dummies	No	Yes	No	Yes	No	Yes	No	Yes
Observations	2426	2059	2426	2059	1048	1046	1048	1046
Households	790	700	790	700	500	500	500	500

[1] This variable takes a value of one if the household has sold at least some of his harvest of a certain product. Marginal effects calculated from a Probit Model.

Significance levels denoted by: *** 1%, ** 5%, * 10% .

Table 5: DID Estimation for Prices

	(1)	(2)
Info	0.01	-0.00
	(0.080)	(0.069)
Spill	0.05	0.05
	(0.072)	(0.067)
t	0.11**	0.13**
	(0.053)	(0.057)
Info x t	0.13*	0.14*
	(0.073)	(0.079)
Spill x t	-0.02	-0.02
	(0.061)	(0.069)
Constant	-0.11	0.03
	(0.080)	(0.057)
Product Dummies	Yes	Yes
Quality Dummies	No	Yes
Observations	2,125	2,111
Number of households	601	600

Regressions include household random effects. Standard errors are clustered at the village level.

Significance levels denoted by: *** 1%, ** 5%, * 10%.

Table 6: Price Regression for Households with Sales in Both Periods[1]

	(1)	(2)
Info	-0.02	-0.02
	(0.074)	(0.062)
Spill	0.06	0.05
	(0.068)	(0.062)
t	0.08	0.11*
	(0.051)	(0.058)
Info x t	0.17**	0.17**
	(0.070)	(0.081)
Spill x t	-0.01	-0.01
	(0.066)	(0.079)
Constant	-0.09	0.05
	(0.071)	(0.056)
Product Dummies	Yes	Yes
Quality Dummies	No	Yes
Observations	1,579	1,567
Number of households	311	311

[1] Includes households who sold in both periods, regardless of the product and quality.
All regressions include household random effects. Standard errors are clustered at the village level. Significance levels denoted by: *** 1%, ** 5%, * 10% .

Table 7: Cross Sectional (Follow-up) Regression for Prices

	All[1]		Both Periods[2]		Follow-up Only[3]	
	(1)	(2)	(3)	(4)	(5)	(6)
Info	0.13***	0.13***	0.14***	0.16***	0.04	0.01
	(0.04)	(0.04)	(0.04)	(0.05)	(0.10)	(0.08)
Spill	0.05	0.03	0.06	0.06	-0.01	-0.04
	(0.04)	(0.03)	(0.04)	(0.04)	(0.10)	(0.08)
Constant	0.08	0.26***	0.11**	0.28***	0.02	0.21**
	(0.06)	(0.05)	(0.05)	(0.05)	(0.13)	(0.09)
Product Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Quality Dummies	No	Yes	No	Yes	No	Yes
Observations	1,075	1,063	876	866	199	197
Number of households	411	406	311	307	100	99

[1] Includes all households who sold at least part of the production in the follow-up survey, regardless of their sales in the baseline.
[2] Includes households who sold at least part of the production of any of their crops both in the baseline and the follow-up survey.
[3] Includes households who did not sell any of the production of their crops in the baseline, but did in the follow-up survey.

Significance levels denoted by: *** 1%, ** 5%, * 10%. Regressions include household random effects. Standard errors are clustered at the village level.

Table 8: Changes in Crop Composition

	DID Coefficient[1]	
	Info	Spill
Peas	0.01	0.01
	(0.042)	(0.026)
Barley	-0.04	0.02
	(0.057)	(0.043)
Lima Beans	0.00	-0.04
	(0.040)	(0.036)
Corn - White	-0.06	-0.02
	(0.061)	(0.053)
Corn - Cusqueado	0.12***	0.03*
	(0.046)	(0.016)
Corn - Cusqueno	-0.01	-0.01
	(0.029)	(0.014)
Corn - San Jeronimo	0.00	-0.01
	(0.019)	(0.015)
Olluco - Yellow	-0.03	-0.05**
	(0.026)	(0.020)
Olluco - Dotted	-0.01	0.00
	(0.030)	(0.025)
Potato - Yellow	0.03	0.00
	(0.028)	(0.009)
Potato - Andean	0.03**	0.03
	(0.016)	(0.021)
Potato - Canchan	-0.01	-0.02
	(0.028)	(0.020)
Potato - Huayro	0.01	-0.02
	(0.025)	(0.016)
Potato - Perricholi	0.08	-0.02
	(0.063)	(0.027)
Potato - Peruanita	0.02	-0.00
	(0.020)	(0.009)
Potato - Unica	-0.06	-0.06
	(0.051)	(0.051)
Potato - Yungay	0.02	-0.03
	(0.052)	(0.054)

[1] The DID coefficients are β_1 and β_2 estimated through the a regression for each crop: $c_{it} = \alpha Info_i + \theta Spill_i + \gamma t + \beta_1 Info_i t + \beta_2 Spill_i t + \mu_i + \varepsilon_{it}$, where $c_{it} = 1$ when the household planted crop c in period t.

All regressions include household random effects. Standard errors are clustered at the village level. Significance levels denoted by: *** 1%, ** 5%, * 10% .

Table 9: Price Regression (Restricted Sample)[1]

	(1)	(2)	(3)
Info	0.17***	0.12*	0.12*
	(0.06)	(0.07)	(0.07)
Spill	0.01	0.01	0.02
	(0.06)	(0.07)	(0.06)
$P_{i,t=0}$	0.54***	0.32***	0.09
	(0.08)	(0.08)	(0.06)
Constant	-0.18***	0.17***	0.34***
	(0.05)	(0.06)	(0.06)
Product Dummies	No	Yes	Yes
Quality Dummies	No	No	Yes
Observations	263	263	263
Households	176	176	176

[1] Includes households who sold in both periods, regardless of the product and quality.

All regressions include household random effects. Standard errors are clustered at the village level. Significance levels denoted by: *** 1%, ** 5%, * 10% .

Table 10: Difference-in-Differences Estimations for Production and Sales

	Production		Prob Sales [1]		Log(Sales Volume)	
	(1)	(2)	(3)	(4)	(5)	(6)
Info	0.04	0.09	0.01	0.01	0.05	0.01
	(0.239)	(0.242)	(0.057)	(0.065)	(0.209)	(0.222)
Spill	-0.12	-0.05	-0.03	-0.02	-0.00	-0.01
	(0.218)	(0.219)	(0.043)	(0.052)	(0.212)	(0.221)
t	-0.55***	-0.42***	-0.12***	-0.01	-0.43***	-0.39***
	(0.156)	(0.133)	(0.033)	(0.039)	(0.143)	(0.125)
Info x t	0.08	0.05	0.12**	0.10	0.19	0.19
	(0.205)	(0.194)	(0.057)	(0.064)	(0.178)	(0.182)
Spill x t	0.04	0.03	0.08	0.07	0.19	0.19
	(0.183)	(0.174)	(0.058)	(0.065)	(0.168)	(0.160)
Constant	5.26***	5.80***	0.48***	0.90***	5.68***	5.87***
	(0.208)	(0.274)	(0.068)	(0.110)	(0.269)	(0.308)
Product Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Quality Dummies	No	No	No	Yes	No	Yes
Observations	5,236	4,212	5,236	4,212	2,122	2,108
Number of households	789	755	789	755	600	599

[1] Linear Probability Model.

Regressions include household random effects. Standard errors are clustered at the village level. Significance levels denoted by: *** 1%, ** 5%, * 10% .

Table 11: Effects by Product Perishability

	Log(Price)		Log(Sales Volume)	
	(1)	(2)	(3)	(4)
Info	0.04	0.02	0.03	-0.01
	(0.081)	(0.071)	(0.223)	(0.233)
Spill	0.05	0.04	0.00	-0.01
	(0.074)	(0.071)	(0.216)	(0.226)
t	0.11**	0.14**	-0.43**	-0.39**
	(0.056)	(0.065)	(0.183)	(0.162)
Info x t	0.10	0.10	0.19	0.20
	(0.078)	(0.089)	(0.214)	(0.209)
Spill x t	-0.03	-0.04	0.20	0.20
	(0.066)	(0.079)	(0.204)	(0.193)
Perishable [1]	0.73***	0.70***	-1.25***	-1.26***
	(0.076)	(0.059)	(0.301)	(0.327)
Perishable x Info	-0.24***	-0.19***	0.14	0.19
	(0.064)	(0.061)	(0.258)	(0.255)
Perishable x Spill	0.03	0.06	-0.08	-0.00
	(0.138)	(0.111)	(0.244)	(0.245)
Perishable x t	-0.02	-0.12	0.02	0.02
	(0.044)	(0.086)	(0.431)	(0.442)
Perishable x Info x t	0.30**	0.36***	-0.02	-0.17
	(0.144)	(0.124)	(0.545)	(0.569)
Perishable x Spill x t	0.04	0.16	-0.11	-0.16
	(0.204)	(0.180)	(0.519)	(0.514)
Constant	-0.83***	-0.68***	6.94***	7.11***
	(0.070)	(0.066)	(0.221)	(0.253)
Product Dummies	Yes	Yes	Yes	Yes
Quality Dummies	No	Yes	No	Yes
Observations	2,125	2,111	2,122	2,108
Households	601	600	600	599

[1] Perishable Products: lima beans and green peas. All other crops (i.e. all types of maize, barley, olluco and potatoes) are considered less perishable.

Regressions include household random effects. Standard errors are clustered at the village level. Significance levels denoted by: *** 1%, ** 5%, * 10% .

Table 12: Effect by (Previous) Cell Phone Ownership[1]

	All Households[2]	No Cell Phone	Cell Phone
$Info_i$	-0.02	-0.03	-0.03
	(0.07)	(0.07)	(0.06)
$Spill_i$	0.02	0.02	-0.01
	(0.07)	(0.08)	(0.06)
t	0.12	0.25	0.01
	(0.06)**	(0.06)***	(0.04)
$Info_i$x t	0.15	0.17	0.14
	(0.08)*	(0.09)**	(0.07)**
$Spill_i$ x t	0.00	-0.06	0.07
	(0.07)	(0.08)	(0.08)
Constant	0.25	0.34	-0.35
	(0.14)*	(0.17)**	(0.06)***
Observations	2014	939	1175
Households	612	290	322

[1] Households with at least one member who owned a cell phone in the baseline. All regressions include product and quality controls.
[2] Corresponds to the results shown in Table 5

Regressions include household random effects. Standard errors are clustered at the village level. Significance levels denoted by: *** 1%, ** 5%, * 10% .

Table 13: Effects by Distance to Nearest Neighbor with Information[1]

	(1)	(2)
$Info_i$	-0.01	-0.02
	(0.08)	(0.07)
$Spill_i$ x Q1	0.03	0.02
	(0.09)	(0.08)
$Spill_i$ x Q2	-0.03	-0.06
	(0.08)	(0.07)
$Spill_i$ x Q3	0.07	0.07
	(0.09)	(0.08)
$Spill_i$ x Q4	0.10	0.08
	(0.09)	(0.08)
$Spill_i$ x Q5	-0.05	-0.04
	(0.09)	(0.09)
t	0.10	0.12
	(0.05)*	(0.06)**
$Info_i$ x t	0.14	0.15
	(0.07)*	(0.08)*
$Spill_i$ x Q1 x t	0.03	0.06
	(0.08)	(0.10)
$Spill_i$ x Q2 x t	-0.01	0.01
	(0.10)	(0.08)
$Spill_i$ x Q3 x t	-0.00	-0.01
	(0.09)	(0.09)
$Spill_i$ x Q4 x t	-0.06	-0.08
	(0.09)	(0.10)
$Spill_i$ x Q5 x t	0.03	0.02
	(0.09)	(0.09)
Constant	0.21	0.25
	(0.17)	(0.14)*
Product Dummies	Yes	Yes
Quality Dummies	No	Yes

[1] The quintiles to the nearest neighbor with information are created with the distance of each household in the spillover group (i.e. in a treated village but did not receieve the price SMS) to the closest household that directly received the price information.

Regressions include household random effects. Standard errors are clustered at the village level. Significance levels denoted by: *** 1%, ** 5%, * 10% .

Table 14: Effects by Crop Match to Households with Direct Information[1]

	(1)	(2)
$Info_i$	-0.01	-0.02
	(0.08)	(0.07)
$Spill_i$	0.05	0.03
	(0.08)	(0.07)
$Spill_i$ x $Match_{c,t=0}$	-0.03	-0.02
	(0.06)	(0.05)
t	0.10	0.12
	(0.05)*	(0.06)**
$Info_i$ x t	0.14	0.15
	(0.07)*	(0.08)*
$Spill_i$ x t	-0.05	-0.06
	(0.08)	(0.08)
$Spill_i$ x $Match_{c,t=1}$	0.03	0.05
	(0.07)	(0.06)
Constant	-0.07	0.05
	(0.07)	(0.06)
Product Dummies	Yes	Yes
Quality Dummies	No	Yes

[1] $Match_{ict} = 1$ if the household in the spillover group is in a village where another household has directly received market price information for crop c in year t. It takes a value of zero otherwise.

Regressions include household random effects. Standard errors are clustered at the village level. Significance levels denoted by: *** 1%, ** 5%, * 10% .

References

Aker, J. (2010), 'Information from Markets Near and Far: Mobile Phones and Agricultural Markets in Niger', *American Economic Journal - Applied Economics* **2**(3), 46–59.

Aker, J. & Fafchamps, M. (2011), How does mobile phone coverage affect farm-gate prices? evidence from west africa. Oxford University and Tufts University. Unpublished Manuscript.

Bandiera, O. & Rasul, I. (2006), 'Social networks and technology adoption in northern mozambique', *The Economic Journal* **116**(514), 869–902.

Besley, T. & Case, A. (1993), 'Modeling technology adoption in developing countries', *American Economic Review* **83**(2), 396–402.

Conley, T. & Udry, C. (2010), 'Learning about a new technology: Pineapple in ghana', *American Economic Review* **100**(1), 35–69.

Duflo, E., Kremer, M. & Robinson, J. (2010), Why don't farmers use fertilizers: Evidence from field experiments in western kenya. MIT, Harvard University and UCSC Working Paper.

Fafchamps, M. & Minten, B. (2012), 'Impact of sms-based agricultural information on indian farmers', *World Bank Economic Review* . Forthcoming.

Foster, A. & Rosenzweig, M. (1995), 'Learning by doing and learning from others: Human capital and technical change in agriculture', *Journal of Political Economy* **103**(6), 1176–1209.

Futch, M. & McIntosh, C. (2009), 'Tracking the introduction of the village phone product in rwanda', *Information Technologies and International Development* **5**(3).

Goyal, A. (2010), 'Information, Direct Access to Farmers, and Rural Market Performance in Central India', *American Economic Journal: Applied Economics* **2**(July), 22–45.

Jensen, R. (2007), 'The digital provide: Information (technology), market performance, and welfare in the south indian fisheries sector', *The Quarterly Journal of Economics* **122**(3), 879–924.

Mitra, S., Mookherjee, D., Torero, M. & Visaria, S. (2013), Asymmetric information and middleman margins: An experiment with west bengal potato farmers. Mimeo. Boston, MA: Boston University.

Munshi, K. (2004), 'Social learning in hetereogeneous population: Technology diffusion in the indian green revolution', *Journal of Development Economics* **73**(1), 185–213.

Muto, M. & Yamano, T. (2009), 'The imact of mobile phone coverage expansion on market participation: Panel data evidence from uganda', *World Development* **37**(12), 1887–1896.

Stigler, G. (1961), 'The Economics of Information', *The Journal of Political Economy* **69**(3), 213–225.

Svensson, J. & Yanagizawa, D. (2009), 'Getting Prices Right: The Impact of the Market Information Service in Uganda', *Journal of the European Economic Association* **7**(2-3), 435–445.