Impact of SMS-Based Agricultural Information on Indian Farmers*

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Abstract

This study estimates the benefits that Indian farmers derive from market and weather information delivered to their mobile phone by a commercial service called Reuters Market Light (RML). We conduct a controlled randomized experiment in 100 villages of Maharashtra. Treated farmers associate RML information with a number of decisions they have made, and we find some evidence that treatment affected spatial arbitrage and crop grading. But the magnitude of these effects is small. We find no statistically significant average effect of treatment on the price received by farmers, crop value added, crop losses resulting from rainstorms, or the likelihood of changing crop varieties and cultivation practices. While disappointing, these results are in line with the market take-up rate of the RML service in the study districts, which shows small numbers of clients in aggregate and a relative stagnation in take-up over the study period.

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1 Introduction

Mobile phones are rapidly spreading all over the world. The global mobile penetration rate was estimated at about 76 per 100 inhabitants in 2010 (ITU, 2011). Poor developing countries are increasingly part of this widespread use of mobile phones and mobile phones are also making quick inroads to rural areas where most of the poor live. This rapid spread of mobile phones offers new possibilities for poor rural and agricultural households in developing countries as they allow users to overcome important barriers of physical distance and improve access to information and services.

Mobile phones are increasingly used in rural areas in Africa and Asia to disseminate daily prices of agricultural commodities. Better and timely price information may improve the welfare for small farmers in different ways. First, better information may lead farmers to make better allocation of production factors. When the farmers receive clear production incentives, they can better seize market opportunities through the adjustment of production plans. Second, information can improve the bargaining position of the small farmers and improve competition between traders. Thirdly, given the provision of alternative nearby markets, farmers can use the information to switch between end markets. And, finally, farmers can use the information to make choices around the timing of marketing. Consequently, erratic price variations should be reduced as arbitrage over time and space becomes easier and more widespread.

Following on the work of Roller and Waverman (2001), Torero and von Braun (2006) put together a series of case studies on the use of telecommunications for development and poverty reduction. In these case studies, they address the broad question of how ITCs affect economic development in low-income countries, how they affect the poor people in these countries in particular and what policies and programs might facilitate their potential to enhance development and the inclusion of poor constituents.[1] However, there are many methodological problems in this type of macro analysis and this limits the lessons that can be drawn from them (Straub, 2008).

The purpose of this study is to ascertain, at the micro level, whether the distribution of agricultural information through mobile phones generates important economic benefits in rural and agricultural settings. To this effect, we implement a randomized controlled trial of a commercial service entitled Reuters Market Light (RML), the largest and best established private provider of agricultural price information SMS services in India at the time of the experiment. Operating in Maharashtra and other Indian states, RML distributes market price information, weather updates, and crop advisory information through SMS phone messages. We offered a

[1] Unsing observational household data, Labonne and Chase (2009) estimate a large effect of mobile phone on welfare in the Philippines.

one-year free subscription to RML to a random sample of farmers to test whether they obtain higher prices for their agricultural output.

We also investigate the channels through which the price improvement comes from – e.g., better ability to arbitrage across space and time, better ability to bargain with traders, and increased awareness about quality premium leading to an improvement in quality through better agricultural practices and better post-harvest handling (e.g., grading, packing). We present simple models of the first two channels. Both models make testable predictions about farmer behavior in response to better price information. We test for the presence of these necessary channels. Given that RML also circulates weather and crop advisory information, we also examine whether farmer profits increase thanks to better crop management, reduced losses, or improved quality.

There is a voluminous literature documenting the apparent ignorance of price movements by small farmers in the developing world (see Fafchamps and Hill, 2008, for a recent example). Yet only a small number of studies have used micro-economic data to examine the impact of mobile phones on rural livelihoods. In a pioneering evaluation, Jensen (2007) showed the impact of mobile phones for poor fishermen in India. Between 1997 and 2001, mobile phone service was introduced throughout Kerala, a state in India with a large fishing industry. Using micro-level survey data, he showed that the adoption of mobile phones by fishermen and wholesalers was associated with a dramatic reduction in price dispersion, the complete elimination of waste, and near-perfect adherence to the Law of One Price.

Goyal (2010) has carried out one of very few studies which attempt to quantify the impact of improved market information through IT technology – in her case, computer terminals. Her work demonstrated that in areas where there was much improved access to and dissemination of market price information (through the presence of e-chopals), farmers obtain wholesale prices of between 1 to 5% higher (with an average of 1.6%) than in areas where market information was less transparent.

Aker (2008) examined the impact that the introduction of cell phones has had on grain trade throughout Niger. Using an original dataset that combines data on prices, transport costs, rainfall and grain production, she shows that cell phones reduce grain price dispersion across markets by a minimum of 6.5% and reduce intra-annual price variation by 10%. The primary mechanism by which cell phones affect market-level outcomes appears to be a reduction in search costs, as grain traders operating in markets with cell phone coverage search over a greater number of markets and sell in more markets. The results suggest that cell phones improved consumer welfare during Niger's severe food crisis of 2005, perhaps averting an even worse outcome.

Using producer price data, Aker and Fafchamps (2011) measure the impact of mobile phones on perishable and storable commodities. They find that mobile phones significantly reduce producer price dispersion across markets for cowpea, an important perishable cash crop in Niger. In contrast, for a storable subsistence crop such as millet, the effect of mobile phones on farm-gate price dispersion is limited to those markets located within a certain radius. They do not find evidence that mobile phones led to higher farm-gate prices. In an unpublished paper, Futch and McIntosh (2009) report the results of a randomized experiment in Rwanda and find no effect of price information on the average producer price.

Muto and Yamano (2009) uses panel data on rural Ugandan households at a time when the number of the communities covered by the mobile phone network was increasing rapidly. They find that, after the expansion of the mobile phone coverage, the proportion of the farmers who sold banana increased in communities more than 20 miles away from district centers. For maize, which is another staple but less perishable crop, mobile phone coverage did not affect market participation. These results suggest that mobile phone coverage induces the market participation of farmers who are located in remote areas and produce perishable crop. In a related vein, Svensson and Yanagizawa (2009) study the effect of a agricultural price radio broadcast on the spread of market information in Uganda. They find a significant impact on farm prices of having access to radio information.

Unlike Aker (2008) or Muto and Yamano (2009) who focus on poorly developed agricultural markets, we focus on a part of India where small scale commercial farming has been on the rise, with a growing emphasis on horticulture for urban domestic consumption. Because these crops are still relatively new to small farmers and markets are rapidly evolving in response to the growth of the economy, we expect price and crop information to be particularly useful to study farmers. This expectation is reinforced by the initial rapid take-up of RML in parts of Maharashtra where the service was first introduced.

Customer satisfaction data collected by RML and field visits by the authors at the onset of the study suggested that farmers benefit from the service in several ways. Customers report that information on prices in distant markets enables them to insist on higher prices from traders (if they sell locally) and to monitor the performance of commission agents (if they sell in a distant market). According to customer satisfaction data, RML enables them to better select the end market to which they send their products. Farmers also state that information on rainfall can be used to time field operations – particularly the harvesting, storage, and transport of fruits. Information on air moisture can be used to predict pest infestation and hence to trigger – or delay – pesticide application. Farmers also seem to derive satisfaction – and local prestige – from obtaining information faster than neighbors. Many farmers we visited made suggestions for expanding the range of information provided by RML. This suggests that farmers realize the value of information and have begun thinking of ways to further improve the service.

These observations are not limited to the RML service. There is a large interest and take-off of similar private and public programs in India and elsewhere (Mittal et al. 2010, Molony 2008, Mookherjee et al. 2011, Overa 2006, Shaffril et al 2009, Tollens 2006), indicating that it is important to understand the impact of these interventions.[2] Based on this, we expected to find a large and significant effect of RML on treated farmers.

These expectations, however, find less support than anticipated in the results of our randomized controlled trial (RCT). Many treated farmers state that they use the RML information, and the evidence indicates that they are less likely to sell at the farm-gate and more likely to change the market at which they sell their output. These findings are consistent with the idea that treated farmers seek arbitrage gains from RML information. We do, however, find no significant difference in price between beneficiaries of the RML service and non-beneficiaries. This result is robust to the choice of estimator and methodology. There is some evidence of heterogeneous effects: treated farmers who are young – and presumably less experienced – receive a higher price in some regressions, but the effect is not particularly robust.

Why we do not find a stronger effect of RML on the prices received by farmers is unclear. Over the study period there have been important changes in prices which are part of a general phenomenon of food price inflation in India (e.g., World Bank 2010). While rapidly changing prices should in principle make market information more valuable, it is conceivable that the magnitude of the change blunts our capacity to identify a significant price difference, given how variable prices are. Point estimates of the treatment effect on price received are, however, extremely small, and sometimes negative. We also find no significant evidence of an effect on transaction costs, net price, revenues, and value added.

In terms of other channels by which RML may affect farmers, we find a statistically significant but small increase of the likelihood of grading or sorting the crop before selling it. This is especially true for younger farmers, possibly explaining why they are able to achieve better prices. RML shows prices by grade to farmers and might have helped to inform them about the benefit from grading. With respect to the benefits from other RML information, we find no systematic change in behavior due to weather information and no change on the crop varieties grown or cultivation practices. Farmers who changed variety or cultivation practice do, however, list RML as a significant source of information used to inform their decision. To summarize, RML benefits appear minimal for most farmers, even though RML is associated with significant

[2] In India, apart from RML, other initiatives include IFFCO Kisan Sanchar Limited (IKSL), a partnership between Bharti Airtel and the Indian Farmers Fertilisers Co-operative Limited (IFFCO) as well as the fisher friend programme by Qualcomm and Tata Teleservices in partnership with the MS Swaminathan Research Found (MSSRF). In Western Africa, Manobi and Esoko, private ICT providers, have developed a number of SMS applications to facilitate agricultural marketing there.

changes in where farmers sell their crops. There is some evidence that young farmers may benefit, but the effect is not very robust.

The rest of the paper is organized as follows. In Section 2 we describe the RML service in more detail. The experimental design is presented in Section 3. The conceptual framework and testing strategy are discussed in Sections 4 and 5, respectively. The data are summarized in Section 6 while estimation results are presented in Section 7.

2 Description of the intervention

The Indian branch of Thompson-Reuters recently started an innovative service to distribute agricultural information to farmers in India. This service is called RML (Reuters Market Light). Subscribers are provided with SMS messages, in English or local languages, sent directly to their personal cell phone – in total amounting to some 75 to 100 SMS messages per month. Subscribers are offered a menu of information types that they can choose from – i.e., various agricultural markets and various crops. This menu of services is based on the company's market research of farmers' information needs, followed by a field test marketing program carried out in late 2006 and early 2007. Prior to the experiment, Reuters collected encouraging anecdotal information from farmers as to the impact of the service.

At the time the experiment was initiated, Reuters had about 25,000 RML subscribers in Maharashtra. The content offering of RML for Maharashtra was as follows:

1. Market information: Commodities and markets covered: soybean (16 markets), cotton (26), pigeon pea/*arhar* (17), wheat (21), green gram/*moong* (6), chick pea/*chana* (21), maize (11), onion (19), pomegranates (34), grapes (30), and oranges (34). An RML subscriber gets regular price and quantity updates for a commodity in three markets of his/her choice or in three default markets (1 benchmark market + 2 markets nearest to his location as per the pin code number of the subscriber's phone number).

2. Weather forecast: High Temperature, Low Temperature (Celsius), Relative Humidity (%), Rainfall precipitation (CM) and probability of rainfall (%). Offered at the *tehsil* level.

3. Crop advisory tips: RML Subscriber is entitled to crop advisory tips for one crop offered by the Content Team. In 2007-08, RML offered crop advisory for soybean, cotton, wheat, pomegranate, grapes and oranges.

4. Commodity news: Every subscriber receives daily RML News about agriculture and the price trends of commodities at the domestic and international level.

At the time of the study the price for the service was about $ 1.5 / month. Thompson-Reuters is a transnational corporation specializing in market information services, its core business. The corporation has a long experience collecting and selling market information, and a reputation for accuracy. Although Thompson-Reuters did not disclose how it obtains market price information, we have no information suggesting that the provided information is inaccurate. Given the enterprise's effort to market its products in India, any doubts about data accuracy would be detrimental to the firm's reputation, with immediate repercussions on its market prospects.

3 Experimental design

A randomized controlled trial (RCT) was organized by the authors to test the effect of the RML service on the price received by farmers. The RCT was conducted in close collaboration with Thompson-Reuters. Funding for the study was provided by the World Bank and Thompson-Reuters.

Five crops were selected as focus for the study: tomato, pomegranate, onions, wheat, and soybean. These crops were chosen because, in Maharashtra, they are all grown by smallholders primarily for sale.[3] The crops were selected for their differences. While wheat and soybean are storable, the other three crops are not, and hence should be subject to larger price uncertainty reflecting short-term fluctuations in local supply and demand (Aker and Fafchamps 2011). We therefore expect market information to be more relevant for perishable crops. Soybean has long been grown commercially in Maharashtra, but tomato, onions and pomegranate are more recent commercial crops. We expect farmers to be more knowledgeable about well established market crops – such as wheat and soybean – than about crops whose commercial exploitation is more recent – such as tomato, onions and pomegranate. Finally, pomegranate is a tree crop that is sensitive to unusual weather and requires pesticide application. We therefore expect the benefit from weather information and crop advisories to have a stronger impact on pomegranate farmers.

For each of the five chosen crops, we selected one district in Maharashtra where the crop is widely grown by small farmers for sale: Pune for tomato, Nashik for pomegranate, Ahmadnagar for onions, Dhule for wheat, and Latur for soya. All five districts are located in the central region of Maharashtra, avoiding the eastern part of the state where sporadic Maoist activity has been reported, and the western part of the state which is less suitable for commercial agriculture.

A total of 100 villages were selected for the study, 20 in each of five districts. The villages were chosen in close consultation with Thompson-Reuters to ensure they were located in areas

[3] Other commercial crops in Maharashtra, such as grapes, tend not to be grown by small farmers.

not previously targeted by RML marketing campaigns. Ten farmers were then selected from each village, yielding a total intended sample size of 1000 farmers.[4]

Two treatment regimes were implemented. In the first regime – treatment 1 – all ten farmers in the selected village were offered the RML service. In the second regime – treatment 2 – three farmers randomly selected among the village sample were offered the RML service. The purpose of treatment 2 is to test whether the treatment of some farmers benefit others as well. Farmers who are not signed-up are used to evaluate the externalities generated by the service, e.g., if they share the information others receive from RML.

Bruhn and McKenzie (2009) have shown that, in randomized controlled trials, stratification improves efficiency. Randomization of treatment across villages was thus implemented by constructing, in each district, triplets of villages that are as similar as possible along a number of dimensions that are likely to affect the impact of the treatment.[5] At the onset of the study, information was collected in a systematic manner on each of the selected villages. From the village questionnaire, a number of village characteristics were selected that are likely to affect the impact of the treatment and that show sufficient variation within the sample. These characteristics are: longitude, latitude, log(village population), log(distance to nearest national road), road availability index, log(distance to nearest vegetable market), log(distance to nearest fruit market), log(distance to nearest cell phone tower +1), and log(number of extension visits per year +1). Some of these characteristics are likely to be correlated, so we need a concept of similarity across villages that account for this correlation. To this effect, we adopt the Mahalanobis metric which is defined as follows. Let z_i and z_j denote the vector of relevant characteristics from villages i and j, respectively. The Mahalanobis distance between them is then defined as:

$$||z_i - z_j|| = ((z_i - z_j)'S^{-1}(z_i - z_j))^{1/2}$$

where S is the covariance matrix of characteristics z. Pairs of villages with a smaller Mahalanobis distance are more similar along these dimensions. Since characteristics are weighted by the inverse of the covariance matrix S, correlation between characteristics – e.g., between our various distance measures – is given less weight. We then select, within each district, the allocation of

[4] This sample size was determined as follows. The primary channel through which we expect SMS information to affect welfare is through the price received by producers. We therefore want a sample size large enough to test whether SMS information raises the price received by farmers. Goyal (2010) presents results suggesting that price information raises the price received by Indian farmers by 1.6% on average. Based on this estimate and its standard error, a simple power calculation indicates that a total sample size of 500 farmers should be sufficient to identify a 1.6% effect at a 5% significance level. To protect against loss of power due to clustering, we double the sample size to 1000. We did not have sufficient information to do a proper correction of our power calculations for clustering.

[5] More precisely, 6 triplets and one pair.

villages into triplets that minimizes the sum, over all triplets, of the Mahalanobis distances within each triplet.[6] Within each triplet, one village is then randomly assigned to control, one to treatment 1, and one to treatment 2.[7] For treatment 2, assignment to treatment was done by selecting a subset of farmers at random.

We also offered RML to randomly selected extension agents covering half of the treatment 1 and treatment 2 villages. In principle, extension agents could disseminate the relevant information they receive, making it unnecessary to distribute the same information to individual farmers. Whether this is the case in practice is unclear, given that extension agents visit villages only infrequently.

The ultimate intent of the RCT is to estimate the impact of RML on farmers who may voluntarily sign up for the service because they benefit from it. In villages targeted by RML only a small proportion of farmers sign up for the service. These tend to be larger farmers with a strong commercial farming orientation in the crops serviced by RML. There is no point estimating the effect of RML on people who are unlikely to benefit from such a service. For this reason, participating farmers are limited to those growing the district-specific selected crop for sale – e.g., with a large enough marketed surplus to amortize the time cost of information gathering and processing, and enough experience with the crop to benefit from agricultural information. This ensures that they are knowledgeable about the focus crops – and hence are more likely to benefit from agricultural information most relevant for one of the five selected crops. It nevertheless remains that the farmers to whom we offer the RML service did not express an initial interest in the product, and may therefore have discounted the value and usefulness of a service which they did not pay for.

Since having a cell phone is essential to access RML services, only farmers with a cell phone are potential customers. We therefore limit participating farmers to those owning a mobile phone at the time of the baseline survey. There does not appear to be strong differences across districts regarding cell phone towers and rural electrification: in Maharashtra, all villages in which Reuters has marketed RML have good cell phone service and widespread access to electricity. This was confirmed in the baseline village survey: all 100 selected villages have mobile phone coverage. RML staff estimate that in the target districts approximately half of the farmers have a mobile phone. This is roughly confirmed by mobile phone penetration figures reported at the state level.[8] We omit farmers who already were RML customer prior to the baseline survey since they

[6] Search is conducted using an algorithm that randomly tries different combinations of villages into triplets.

[7] In the pair, one village was assigned to treatment 1 and the other to treatment 2.

[8] According to Telecom Regulatory Authority of India (2010), page 7, rural teledensity (the number of phone numbers divided by the population) in Maharashtra was 35 at the end of June 2010 (i.e. the time of our survey). At the end of June 2009, the number of mobile phone subscribers in Maharashtra was 33.52 million. At the end of June 2010, this had increased to 49.75 million, a 48% increase in one year.

could not be used for impact analysis. Because we carefully avoided areas in which RML had already been actively promoted, very few farmers were eliminated due to this condition.

Within each village, a list of farmers satisfying our three criteria was constructed and a random sample drawn from within this list. The initial objective of sampling 10 farmers per village was achieved in most villages. In some villages a sufficient number of willing participants could not be found; other villages in the district then make up the difference. Each farmer was interviewed twice: first in June-July 2009 (baseline survey) and again in June-July 2010 (ex post survey). This allows us to cover one complete agricultural year and two cropping cycles (rabi and kharif) in detail. The choice of the June-July period for the survey was dictated by the desire to interview farmers after the main harvest when price information is still fresh in respondents' mind.

The questionnaire focuses on agricultural practices and on the use of SMS information. We collect data on: the price and time at which the farmer sold his output; the total revenue from the sale of RML-covered crops; crop yields in various crops; and input usage (e.g., type and quantity of pesticide) and costs. The same questionnaire was used in the two surveys to ensure comparability, except that a few questions about RML were added in the ex post survey. We also collected information on whether farmers changed their marketing and crop production practices between the two surveys.

In the baseline survey we collected the names and telephone numbers of all farmers. The names and telephone numbers of the farmers selected for treatment were then transmitted to RML who proceeded to sign them up for the service. Farmers were free to refuse the service – and some did. To control for this, impact evaluation relies primarily on an intent-to-treat approach.

In a number of cases, the phone number that was signed up for RML subsequently came to be used by someone else than the target farmer. Some control farmers also managed to obtain the service directly from RML. To ascertain the extent of contamination and non-compliance, we conducted an additional phone interview after the 2010 survey was completed. In this phone interview, information was gathered on the identity of the user of the phone number that was signed up for RML, and the relationship between this user and the target farmer. Results from the ex post survey are discussed in the empirical section.

4 Conceptual framework

Our primary objective is to test whether farmers benefit from the SMS-based market information and if so, how. Information gathered by Thompson Reuters and conversations with farmers and RML customers in several villages of Maharashtra suggest that farmers benefit from RML in several ways. Timely access to market price information at the time of harvest helps farmers decide where to sell, as in Jensen (2007). It also enables them to negotiate a better price with traders. To better motivate the empirical analysis, we model these two possible channels briefly below.

To illustrate how better informed farmers may obtain a higher average price thanks to better arbitrage, consider a farmer selecting where to sell his output. Imagine there are two possible markets i and j with transport costs τ_i and τ_j. We assume $\tau_j > \tau_i$, i.e., market j is the more distant market. To focus on arbitrage, let us assume that the distribution of producer prices $F(p)$ is the same on both markets. In particular, $E[p_i] = E[p_j] = \mu$. Given this, it is optimal for an uninformed farmer to always ship his output to market i since $EU(p_i - \tau_j) < EU(p_j - \tau_i)$ for any expected utility function $U(.)$.[9] The average price received by farmers is thus μ. A relevant special case is when a farmer sells at the farm-gate to an itinerant buyer. In that case the farmer incurs a zero transaction cost, receives a farm-gate price p_i, and does not learn the market price p_j.

Now suppose that the farmer is given information on the prices in i and j. Shipping to i remains optimal if $p_i - \tau_i \geq p_j - \tau_j$; otherwise, the farmer ships to j. The average farmer price now is:

$$\begin{aligned} E[p_i|p_i - \tau_i &\geq p_j - \tau_j] \Pr(p_i - \tau_i \geq p_j - \tau_j) \\ +E[p_j|p_i - \tau_i &< p_j - \tau_j] \Pr(p_i - \tau_i < p_j - \tau_j) \geq \mu \end{aligned} \tag{1}$$

Equation (1) holds with equality only if p_i is always larger than $p_j - (\tau_j - \tau_i)$, which arises only if $\tau_j - \tau_i$ is large relative to the variance of prices, or if prices in the two markets are strongly correlated. It follows that if price information allows farmers to arbitrage better across markets, the average farmer price should rise and we should observe farmers now selling in different, more distant markets.[10] In the case of farm-gate sales, obtaining information about p_j induces farmers to sell at the market if $p_j > p_i + \tau_j$ where p_i is the price offered by itinerant buyers.

There remains the issue of why $p_i \neq p_j$ in the first place: if farmers can arbitrage, so can traders in the two markets – or between the farm-gate and the nearest market. Let θ be the

[9] Since $p - \tau_i > p - \tau_j$ pointwise. Note that there is no role for risk aversion in this model.

[10] A similar reasoning applies to intertemporal arbitrage: uninformed farmers may prefer to sell immediately after harvest, while better informed farmers may choose to sell at a later date if the anticipated price is higher. Since RML does not disseminate information about future prices, however, we do not expect an intertemporal arbitrage effect, except perhaps in the immediate vicinity of harvest. Several of the studied crops are perishable; this further limits opportunities for intertemporal arbitrage.

trader shipment cost between i and j. With perfect information, trader arbitrage yields:

$$p_j + \theta \leq p_i \leq p_j - \theta$$

and thus $|p_i - p_j| \leq \theta$. The possibility of farmer arbitrage therefore arises whenever $\tau_j - \tau_i < \theta$, i.e., when farmers have access to reasonably cheap transport to markets.[11] However, if traders have a comparative advantage in transport – for instance because they ship larger quantities and benefit from returns to scale – then it is possible that $\tau_j - \tau_i > \theta$ for most if not all farmers. In this case, $\Pr(p_i - \tau_i < p_j - \tau_j) = 0$, which implies that farmers always sell at the closest market or location i, and the average farmer price is μ, as in the case without information.

Even when price information does not trigger farmer arbitrage, it may facilitate arbitrage by traders, thereby ensuring that $|p_i - p_j| \leq \theta$ (e.g., Aker and Fafchamps 2011). If farmers are risk averse, they would typically benefit from the reduction in the variance of prices,[12] irrespective of whether they receive market information p_i and p_j or not prior to deciding where to ship their output.

The second way by which farmers can benefit from price information is when they sell to traders who are better informed about market prices – e.g., when farmers sell at the farm-gate. To illustrate this in a simple way, consider price negotiation between an informed trader, who knows the market price realization p_i, and an uninformed farmer, who only knows the price distribution $F(p_i)$. To demonstrate how information can benefit the farmer, imagine that the farmer mimics an auction system and calls a decreasing sequence of selling prices until the trader accepts it. In a competitive market with many buyers – which makes collusion difficult – the selling price will be p_i. In a one-on-one negotiation, as would take place at the farm-gate for instance, the buyer correctly anticipates that the farmer will continue calling lower prices below p_i. He can thus wait for the farmer to reach his reservation price, which is given by the value of the farmer's next best alternative, namely, selling at the nearest market.

The expected payoff to an uninformed farmer of selling at the market is $EU(p_i - \tau_i)$. Let $\widetilde{p_i} \equiv p_i - \tau_i$ be the market price net of transport cost, and let $\widetilde{\mu} \equiv E[\widetilde{p_i}]$. The farm-gate reservation price of a risk averse farmer is the price $p_i^r = \widetilde{\mu} - \pi$ that solves:

$$U(\widetilde{\mu} - \pi) = EU(\widetilde{p_i})$$

[11] For instance, if transport costs per Km are identical for farmers and traders, condition $\tau_i - \tau_j < \theta$ holds generically on a plane, except when the farmer and the two markets are exactly in a straight line. With many farmers distributed randomly on the plane, this has Lesbegue measure zero.

[12] Except when they consume much of their output, something that is ruled out here since the empirical analysis focuses on commercial crops.

Using a standard Arrow-Pratt Taylor expansion, we get:

$$U(\widetilde{\mu}) - U'(\widetilde{\mu})\pi \simeq E[U(\widetilde{\mu}) + U'(\widetilde{\mu})(\widetilde{\mu} - \widetilde{p}_i) + \frac{1}{2}U''(\widetilde{\mu})(\widetilde{\mu} - \widetilde{p}_i)^2]$$

which, after some straightforward manipulation, we can solve for π:

$$\pi \simeq -\frac{1}{2}\frac{U''(\widetilde{\mu})}{U'(\widetilde{\mu})}\sigma^2 = \frac{1}{2}R\,CV^2$$

where R is the farmer's coefficient of relative risk aversion, σ^2 is the variance of the market price, and $CV \equiv \sigma/\widetilde{\mu}$ is the coefficient of variation of price. It follows that a buyer can always buy from an uninformed farmer at price $p_i^r = \widetilde{\mu} - \pi$. Only if the realized market price $p_i < p_i^r$ will the farmer be unable to find farm-gate buyers – in which case he will have to travel to the market and sell at $p_i < \widetilde{\mu} + \tau_i - \pi$ but incur transport cost τ_i. The average price received by an uninformed farmer is:

$$\begin{aligned}(\mu - \tau_i - \pi)\Pr(p_i &\geq \mu - \tau_i - \pi) \\ +E[p_i|p_i &< \mu - \tau_i - \pi]\Pr(p_i < \mu - \tau_i - \pi) \leq \mu - \tau_i\end{aligned}$$

It follows that the larger R – and thus π – is, the lower the average farmer price is. If risk aversion is negatively correlated with wealth, the above predicts that poor uninformed farmers receive a lower average price than non-poor uninformed farmers. Similarly, the larger CV is – for instance because the farmer is inexperienced and is unsure about the price distribution – the lower the average farm-gate price is.

Once we introduce price information, the farmer's farm gate reservation price becomes $p_i - \tau_i$ and buyers are no longer able to exploit farmers' risk aversion to buy below the market price. In this case the expected price received by farmers is $\mu - \tau_i$ if they sell at the farm-gate, or μ if they sell in the market. Hence the average price that informed farmers receive is unambiguously higher than that of uninformed farmers. The difference is largest when uninformed farmers often sell at the farm-gate. If farmers do not sell at the farm-gate at all, information has no effect on the average price farmers receive.

The reader will note that uninformed farmers would benefit if they could commit to sell at the market. If such a commitment mechanism is unavailable, however, farmers can always be tempted to sell at the farm-gate if offered a price above their reservation price. Of course a sophisticated but uninformed farmer should infer that, if a trader is willing to buy from him at the farm-gate, the market price must be above his reservation price, in which case he should sell at the market. If farmers are sophisticated, we should therefore observe few if any farm-gate sales by uninformed farmers. In this case, providing market information to farmers should make farm-gate sales more common.

The two models presented above do not exhaust all the possible channels by which price information may affect the price received by farmers. In the Indian context, one important possibility is excessive fees collected by commission agents (Minten et al. 2011). However, since RML circulates no information about market fees, it is unclear why it should lead to their reduction.

To summarize, model predictions regarding prices are as follows. If price information enables farmers to arbitrage across markets, treated farmers should receive a higher price for their output than uninformed farmers, but only if informed farmers start selling in distant markets. Otherwise we expect no difference between control and treated farmers. The introduction of RML, however, could reduce the variance of prices for everyone through trader arbitrage, as in Aker and Fafchamps (2011).

If price information helps farmers negotiate better prices with traders, treated farmers should receive a higher average price only if they were selling at the farm-gate prior to treatment. For these farmers, we expect a stronger treatment effect for poor and inexperienced farmers. For farmers who were selling primarily if not exclusively through wholesale markets, we expect no effect of the treatment on price. But treatment may nevertheless induce farmers to sell at the farm-gate for convenience reasons, or if traders have a comparative advantage in transporting produce from the farm-gate.

RML may also benefit farmers in other ways, which we do not model here because they are more straightforward. Better knowledge of price differentials driven by quality may also induce farmers to upgrade the quality of their output, for instance by grading or treating their crops. Weather information helps with farm operations. In particular, information about the probability of rainfall enables farmers to either delay (e.g., pesticide application) or speed up (e.g., harvest) certain farm operations. Information about air humidity is a good predictor of pest infestation and hence of the need to apply pesticide. Crop advisories assist farmers to opt for a more appropriate technology (e.g., choice of variety, pesticide, and fertilizer).

5 Testing strategy

We now describe how we test the above predictions. Since the data are balanced, we ascertain the effect of RML on various outcome indicators by comparing control and treatment in the ex post survey. Formally, let Y_i represent an outcome indicator – e.g., price received – for farmer

i. Let $W_i = 1$ if farmer i was offered a free subscription to the SMS-based market service, and $S_i = 1$ if farmer i signed up for the service. All treated farmers are in treated villages but the converse is not true: only some farmers in treatment 2 villages were offered the RML service. It is possible for treated farmers not to sign up for the service – i.e., for $S_i = 0$ even though $W_i = 1$. Although control villages were not targeted by RML marketing campaigns, it is also possible for non-targeted farmers to independently sign up, i.e., it is possible for $S_i = 1$ even though $W_i = 0$.

We are interested in estimating the direct effect of the RML service on customers, i.e., those with $S_i = 1$. Since S_i is potentially subject to self-selection while W_i is not, we begin by reporting intent-to-treat estimates that compare control farmers to those who were offered the free subscription. The estimating equation is:

$$Y_i = \theta + \beta W_i + e_i \tag{2}$$

Next we investigate the effect of receiving the RML subscription. As we will see, the likelihood of signing up is much higher among farmers who received the offer of a free subscription. This means that W_i satisfies the inclusion restriction and can be used as instrument for S_i. We thus estimate a instrumental variable model of the form:

$$Y_i = \theta + \alpha S_i + e_i \tag{3}$$

where e_i is an error term possibly correlated with S_i (sef-selection effect) but uncorrelated, by design, with the instrument W_i.

Provided that there are no defiers, we can interpret IV estimates from equation (3) as local average treatment effects or LATE. Assuming no defiers means that farmers who did not sign up for RML even though they were offered a free subscription would not have signed up for it if they had not been offered a free service. In our set-up, this assumption is unproblematic. We can therefore interpret α in equation (3) as the effect of RML for a farmer who would be induced to sign-up if offered the service for free. This is the IV-LATE approach.

Equation (2) can be generalized to investigate heterogeneous effects. Let X_i be a vector of characteristics of farmer i thought to affect the effect of the treatment on the outcome variable Y. In particular, we expect the benefits from RML to be larger for commercial farmers who put more emphasis on crops for which RML information is useful. We also expect less experienced commercial farmers to benefit from the service more. The estimated model becomes:

$$Y_{it} = \theta + \alpha W_i + \gamma X_i + \eta W_i(X_i - \overline{X}) + e_i \tag{4}$$

where $\overline{X}$ denotes the sample mean value of characteristic X_i. The average effect of the treatment is given by α while the heterogeneous effects of treatment on farmer with characteristic X_i is given by $\alpha + \eta(X_i - \overline{X})$. Using this methodology, we test whether the benefit from access to SMS-based market news differs across recipients.

When interpreting the results from models (2), (3), and (4), it is important to remember that identifying the value of information is difficult. This is because the value of information changes with circumstances. In particular, information is useful only when the recipient of the information can act upon it. For instance, up-to-date price information is most useful around harvest time. In contrast, crop advisory and input cost information are most useful at planting time. This means that for information to be useful it must be provided in a timely manner. How valuable information is depends on the context: because information is not useful in one year, this does not imply that it is never useful.

Secondly, information circulates through channels other than RML – e.g., farmers talk to each other and to commission agents, and they visit nearly markets to obtain up-to-date price information. For models (2), (3), and (4) to identify the impact of RML information, it must be that the circulation of information among farmers is not so rapid and so widespread that control farmers indirectly benefit from it as well. It is for this reason that we regard the village as the most appropriate treatment unit, because we believe that information exchange is more likely among neighboring farmers. We cannot, however, entirely rule out the possibility of spill-overs across villages.

Thirdly, price information may benefit farmers by improving their bargaining power with traders and commission agents. Since the latter cannot easily distinguish between RML and non-RML farmers, it is possible that they will adapt their behavior towards all farmers, for instance by making better price offers. If this is the case, then control farmers may benefit as much as treated farmers from the RML service. There is little we can do to protect against this form of contamination, except check informally how agricultural wholesale prices change over time as farmers become better informed.[13]

6 The context and data

Take-up of RML by Maharashtra farmers is one possible measure of the benefits from the service: presumably farmers would not sign up for a commercial service if they do not benefit from it.

[13] It is also conceivable (albeit unlikely) that RML clients indirectly create a negative externality for non-clients, for instance because the selling behavior of RML clients indirectly lowers the price received by non-clients, or because it raises the price for local consumers. If this were the case, we would overestimate the effect of RML by comparing RML and non-RML farmers within the same village. This is why we focus our analysis on comparisons across treatment and control villages.

We report in Table 1 the number of agricultural holdings in each of the study districts (2000/01 agricultural census) and the number of RML subscribers over the study period. We see that RML take-up has varied over time in the five study district. Take-up increased rapidly in all five districts between 2007 – the time at which RML was introduced – and 2009 – the time at which our experiment was initiated. Take-up never exceeded 0.5% of the total farmer population, however. The Table also shows that subscription levels have stabilized in recent years and have even come down in some districts in 2010. It is only in Nashik that we see a large increase in the number of subscribers between 2009 and 2010. This may be explained by the fact that Nashik has a nascent grape growing and wine making industry that has been rapidly growing in recent years. Since grapes are grown primarily by large farmers, they are not included in our study.

Next we report on contamination and non-compliance. Extensive contamination could indicate that many farmers find RML beneficial and sought out the service even though it was not marketed locally. In contrast, extensive non-compliance could suggest that treated farmers did not find the service useful. In Table 2 we compare the experimental design, or intent to treat, in the two upper panels to actual RML usage in the lower panel. The uppermost panel describes the original experimental design. This design assumes that 10 farmers would be found in each of the 100 villages selected for the study.

The middle panel of Table 2 describes how the experiment was implemented in practice. This represents what in the rest of the paper we call 'intent to treat'. All farmers in treatment 1 villages were offered RML free of charge for one year. In control villages, no farmer was offered RML and no marketing of RML was done by the provider. In treatment 2 villages, a randomly selected subset of farmers (3 out of 10) were offered RML while the others were not.

There was some attrition between the baseline and ex post surveys: 1000 farmers were interviewed in the baseline, 933 of whom were revisited in the ex post survey. There is some difference in attrition between the control and two treatment groups, i.e., 91% vs 96% vs 93%. To investigate whether there is anything systematic about attrition, we regressed an attrition dummy on household characteristics.[14] We find that onion producers (Ahmadnagar district) are more likely to drop out of the experiment, but none of the other variables is statistically significant. Triplet dummies are included as regressors throughout the analysis; they indirectly control for district/target crop.

In the next panel we report actual RML usage, as depicted by the 2010 survey and by the ex post phone interview. We immediately note a significant proportion of non-compliers: only 59% of those farmers who where offered RML actually ended up using it. Non usage has various proximate causes. Some subscribers simply refused the service. In the ex post phone

[14] I.e., household size, age of household head, education of household head, land owned, total land cultivated of the selected crop in 2009, and target crop/district dummies.

interview, respondents were asked the reason for refusal. Some indicate that they believed they would be charged for service later on; others were illiterate households who could not read SMS messages and thus could not use the service anyway. Another reason for non-usage was that subscribers never activated the RML service. To activate it, the subscriber had to indicate to RML which crops and which markets they wished to receive information on; some subscribers never completed the activation sequence. Non-usage was also partly due to changes in phone number and to migration – e.g., a household member leaving the farm and taking the phone number with them. The RML service is tied to a specific phone number, so if this phone number is no longer used by the household, the service no longer reaches its intended target. Finally, a number of Chinese-made phones could not display the Marathi script and households with such phones could not read the RML messages. While there is variation between them, all these proximate causes indicate a certain lack of interest in the service: if RML had been very valuable to these farmers, they would have made more of an effort to secure it – e.g., by keeping the SIM card and getting another phone.

There is also variation in non-compliance across districts: non-compliance is lowest in Nashik among pomegranate farmers (27%). This finding is consistent with the high take-up reported in Table 1, and indicates more interest in the RML service in that district. In contrast, the proportion of non-compliers is close to half among onion, tomato and wheat growers. While non-compliance is high, contamination is low everywhere: only 3.7% of control farmers – 10 out of 272 farmers – signed up for RML. This confirms that independent interest in the service among study farmers is limited.

At the bottom of Table 2 we report variation between the intended experimental design and realized treatment for extension agents. The intent was to offer one year of RML service free of charge to the extension agents serving a randomly selected sub-sample of 30 of the 70 treated villages. In practice, we only managed to locate and offer RML to extension agents serving 20 of the treated villages. In order not to introduce contamination, RML was not offered to extension agents serving control villages. This means that we can only measure the additional effect that an informed extension agent may have over and above an individual RML contract (treatment 1 villages) or in addition to treatment of other farmers in the same village (control farmers in treatment 2 villages).

In Table 3 we compare control and intent-to-treat farmers in terms of balance. Columns 4 and 5 report the mean value of each variable for the control group and their standard deviation, respectively. Columns 6 and 7 report the coefficient of an intent-to-treat dummy in a regression of each variable on triplet fixed effects.[15] Reported coefficients suggest good balance on all vari-

[15] Bruhn and McKenzie (2009) indicate that fixed effects for each stratification cell should be included in all regressions.

ables, including area planted to the target crop, marketing, transaction costs, past weather, and past technological innovation. We follow Deaton (2009) suggestion *not* to include unnecessary control variables in the analysis of randomized controlled trials, as it may artificially inflate t-values.

7 Empirical analysis

We now turn to the econometric analysis of the data. Unless otherwise stated, all analysis is conducted in terms of intent-to-treat, i.e., the 'treated' are those who were offered a free one-year subscription to the RML service, whether or not they accepted it. We also report local average treatment effect (LATE) results in which we instrument actual RML usage with random assignment to treatment. We refer to these results as IV or LATE estimates interchangeably. For much of the analysis we use both treatment 1 and treatment 2 farmers to improve efficiency. When using treatment 2 farmers, the intent-to-treat variable is set to 1 if a surveyed farmer in a treatment 2 village was randomly assigned to treatment, and 0 otherwise. All reported standard errors are clustered by village triplet (see experimental design section).

7.1 RML usage

We begin with RML usage as reported by farmers. In the baseline survey all respondents were asked to list their main sources of information for agricultural prices, weather forecast, and advice on agricultural practices. Answers are tabulated in Table 4. Own observation and experimentation is the main source of information reported by all respondents, followed by conversations with other farmers. Radio and television are mentioned as a common source of information on the weather, but much less so for crop prices. RML is not mentioned by anyone.

In the top panel of Table 5 we report the average difference in the proportion of respondents who mention RML as a source of information in the ex post survey. The average treatment effect on the treated or ATT is calculated using the nearest neighbor matching methodology described in Abadie at al. (2004) where matching is performed by triplet dummy. Reassuringly, treated farmers are significantly more likely to mention RML in all six categories. The difference is largest in magnitude for prices and weather, which are the primary focus of the RML service: 24% and 23% more treated respondents mention RML as source of information on crop prices and weather forecasts, respectively.

LATE-IV estimates are reported in the next panel of Table 5. These estimates are obtained using regression analysis. Dummies are included for village selection triplets. Since contamination is low (3.7%) but non-compliance is high (41%), we expected instrumented treatment effects to be larger than the intent-to-treat effect reported at the top of the Table. This is indeed what happens: we now find that farmers who were induced to use RML as a result of treatment are 46% percent more likely to mention RML as source of information on crop prices. The corresponding figures for weather prediction and for input use are 44% and 39%, respectively. This suggests that RML is seen as a source of information by a large proportion of participating farmers. Yet, the effect is not 100%, which means that, since non users do not list RML, a sizeable portion of treated respondents do not list RML as source of information.

In the second part of the Table we look for evidence of heterogeneous effects by farmer age and farm size. To this effect we estimate regression (4), including triplet fixed effects as suggested by Bruhn and McKenzie (2009). We find that farmers cultivating a larger area are significantly more likely to mention RML as source of information, and that this effect is limited to treated large farmers for crop prices, weather predictions, and input use. Farmer age is never significant.

Next we examine whether treated farmers appear more knowledgeable about crop prices. In the first four columns of results in Table 6, we present ATT estimates for knowing the sale price of the target crop before the day of the sale. Results show that treated farmers consider themselves more knowledgeable about crop prices in general, and the difference is significant in all four cases, that is, one day before sale as well as several months before sale. In the second panel of Table 6, we report IV-LATE estimates which, as for Table 5, are larger in magnitude than the intent-to-treat ATT. There is no evidence of heterogeneous effect along those two dimensions.

In the next column of Table 6 we investigate whether participating farmers report sharing more information about farming with other farmers. If RML information is valuable, we expect treated farmers to be more likely to share it with others. Results reported in Table 6 suggest that this is indeed the case, although the effect is not large in magnitude: the intent-to-treat estimated ATT is a 6% increase; the IV-LATE estimate is larger at 12%, but still relatively small. Both effects are statistically significant, however. There is no evidence of heterogeneous effects by farm size or farmer age.

In the last two columns of Table 6 we check whether treated farmers economize on search costs because of RML. To this effect, we examine whether treated farmers make less effort gathering price information, either by consulting with others, or by collecting price information in person at the time of planting. Contrary to expectations, results do not suggest that this is the case. The heterogeneous effect regression results reported at the bottom of the Table indicate that large farmers consult with more people and are more likely to collect price information at planting time, as could be expected: for these farmers, the gain from making a better informed decision are larger, hence more effort is made to gather relevant price information. But we find no significant evidence that RML helps large farmers economize on these costs. This may only be temporary, however: once farmers learn they can trust RML information they may decide to rely on it more. Young farmers consult fewer people about prices, but there is no evidence of heterogeneous effects by farmer age.

7.2 Price received

There is considerable price variation within village. Different crops have a different coefficient of variation: lower for non-perishable crops such as wheat (CV=0.07) and soya (CV=0.14), and higher for perishable crops such as tomato (CV=0.22), onions (0.44) and pomegranate (0.45). Based on this, we expect RML information to help farmers receive higher prices, and to be particularly beneficial for more perishable crops since their prices are more volatile and consequently information is potentially more valuable.

This is what we investigate in Tables 7 and 8. The dependent variable is the log of the unit price received by the respondent on average over all the sale transactions of the target crop during the 12 months preceding the survey. Similar results are obtained if we use the price level instead of the log. The unit of observation is the sales transaction. Most farmers report a single sale but some report more than one, which explains why the number of observations exceeds the number of participating farmers.

The first column of Table 7 reports the ATT obtained using nearest neighbor matching. Contrary to expectations, we find no beneficial effect of the treatment on price received: the treatment effect is negative and statistically significant. We worry that this may be due to the inclusion of treatment 2 villages in the comparison. Indeed, in these villages, the small number of randomly treated farmers may circulate the RML information to untreated farmers who would then also benefit from it. This may blur the comparison between control and treated farmers due to a confounding externality between control and treated farmers in treatment 2 villages. To investigate whether this is the reason for our result, we reestimate the ATT using only treatment 1 and control villages. The results are reported in the second panel of Table 7. We again find a negative treatment effect on farmer price but it is not statistically significant. We also checked (results not reported here to save space) whether farmers in treatment 2 villages received higher prices than in control areas – unsurprisingly given the lack of result for stronger treatment 1, they do not. The next column reports dif-in-dif ATT estimates, using nearest neighbor matching. Point estimates are now slightly positive, but nowhere near conventional levels of significance.

Next we examine whether the lack of effect is due to non-compliance. Indeed we have seen that many treated farmers did not end up using the RML service. To investigate whether this affected our results, we instrument actual RML usage with the intent-to-treat dummy and report the results in the IV-LATE column of Table 7. The estimated coefficient of receiving the RML service is still negative, although it remains non significant for the entire sample as well as for the sample without treatment 2 villages.

In Table 8 we repeat the ATT nearest neighbor matching and IV-LATE analysis for each crop separately. For the whole sample, ATT point estimates are negative for all crops, significantly so for onions. IV-LATE point estimates remain negative, but are not statistically significant. When we restrict the analysis to control and treatment 1 villages, we find negative ATT and IV point estimates for four out of five crops; except for one (IV for tomato), they are not significant.

We then examine whether intent-to-treat results may be affected by omitted variable bias. This is unlikely since treatment is randomly assigned, but we check it anyway. To this effect, we add controls for farmer age and farm size, as well as dummies for type of sale (i.e., whether sold in the village or to a trader, as opposed to sold in the local wholesale market or *mandi*). We also include a dummy equal to one if the extension agent serving the village received the free RML service. Results are reported in Table 7 under the 'OLS long model' column. Other controls are included as well, as detailed at the bottom of the Table, but their coefficients are not reported to save space. Again we find no evidence of a significant treatment effect. The coefficient of the extension agent treatment is similarly non significant.

In the last two columns of Table 7 we investigate the possible existence of heterogeneous effects. The OLS columns reports the heterogeneous intent-to-treat effect, equation (4), with controls. We also estimate an heterogeneous effect version of equation (3):

$$Y_i = \theta + \alpha S_i + \gamma X_i + \eta S_i(X_i - \overline{X}) + e_i \tag{5}$$

Wooldridge (2002) recommends estimating IV models of this kind as follows. Let $\widehat{S}_i$ be the predicted value of S_i from the instrumenting equation. We construct a variable $\widehat{S}_i(X_i - \overline{X})$ and we estimate (5) using $\widehat{S}_i$ and $\widehat{S}_i(X_i - \overline{X})$ as instruments.

In the OLS (intent-to-treat) results we now find a negative average treatment effect but a positive heterogeneous effect on young farmers. Treated young farmers received a price that is about 6% higher on average. In the IV results, the average treatment effect is non-significant but the heterogeneous age effect remains. This suggests that less experienced farmers gain something from RML. These findings, however, are not robust to dropping treatment 2 villages, as seen in the second panel of Table 7.

As robustness check, we correct for the possibility of non-random attribution by adding an inverse Mills ratio as additional regressor in the IV_LATE regression. This Mills ratio is obtained from the attrition selection regression mentioned in Section 4. Results, not shown here, are similar to those reported in Table 7, and the Mills ratio is not statistically significant from 0 in the full sample or when using treatment 1 only, suggesting that non-random attrition is unlikely to have affected our findings.

We also find that, in the OLS regression, farmers that grow more of the target crop get a significantly higher price on average. One possible explanation would be that, for small crop sales, farmers make less effort to obtain price information, and hence sell at a lower price. This effect, however, is not robust – it disappears in the IV regression or if we drop treatment 2 villages. Finally, consistent with expectations, we find that treated households receive a price that is 8-9% higher than non-treated households when they sell to a trader, in contrast of selling to a commission agent. This is in line with the idea that better informed farmers can negotiate better deals from buyers when they sell outside the relative safety of the mandi.

We also examined whether treatment reduced the variance – or more precisely the coefficient of variation – of the price received across farmers in the same village. We may indeed expect price variation across farmers to be less if they are better informed. Aker (2008), for instance, reports that the introduction of mobile phones in Niger facilitated price integration and reduced price dispersion. We do not find a similar effect for RML: the coefficient of variation of prices in treatment 1 villages is 0.320; in control villages it is 0.228, that is, smaller than in treated villages. The difference, however, is not statistically, as measured by a t-test: the t-value=1.52, with a p-value of 0.135.

To summarize, on average, the RML service does not increase the price received by farmers, except perhaps for younger, less experienced farmers. It also does not reduce the dispersion of prices received by farmers. These results raise the question of whether the experiment took place at a time when there were few price changes, in which case the information provided by RML may add little to what farmers already knew. To investigate this possibility, we report in Table 9 the evolution of prices as reported in the RML database, and as reported by surveyed farmers. The RML price data was collected via key informants located in relevant wholesale markets. Prices reported in that part of the Table therefore refer to wholesale prices at the *mandi* level. The survey price data comes from individual crop sale transactions reported by farmers.

We note that, from the baseline 2008-9 agricultural season to the 2009-10 season, there was a massive increase in the average wholesale price of pomegranates (+70%) and a large increase in the average wheat price (+20%).[16] If we compare average mandi prices to prices received as

[16] This is part of a general phenomenon of food price inflation in India during that period. This process is apparently driven by droughts and changes in procurement policies of the government, e.g., higher support prices as well as increased procurement after the global food price hike (World Bank 2010).

reported by surveyed farmers[17] we see that, for both of these crops, the share of the wholesale price that farmers receive fell between the two years: from 46 to 40% for pomegranate on average, and from 89 to 82% for wheat. In other words, the pass-through of the rise in wholesale prices to producer prices was incomplete, a finding that is reminiscent of that of Fafchamps and Hill (2008) for Uganda.

In contrast, there was little change in the average wholesale price for tomato, onion and soya between the two cropping seasons. For soya the producer/wholesale price ratio was already high in 2008-9 (91%) and it rises to 97% in 2009-10. For tomato, the ratio was much lower in 2008-9 (65%) but it rises to 110% in 2009-10. For onion, the ratio was already high in 2008-9 (109%) but rises to 170% in 2009-10. As indicated earlier, given that the data come from two different sources, these producer/wholesale price ratios should be taken as indicative of trends between the two survey years, not necessarily of the levels of these ratios in any given year.

To summarize, over the study period average producer prices rose for all crops. The rise is highest (+75%) for tomato, and lowest for soya (+4%) and wheat (+11%), with intermediate values for pomegranate (+46%) and onion (+44%). For the two crops for which the wholesale rose, farmers lost out a bit in terms of share of the wholesale price going to the producer. But they still enjoyed a higher price. For crops for which the wholesale price did not change, farmers received a higher share of that wholesale price. Taken together, these findings indicate that farmers, on the whole, gained between the two years. Given this, it is perhaps not surprising to find that farmers credit RML for providing them useful information: between the two surveys the price rose and farmers first heard about it through RML. But even control farmers ended up benefitting from the price increase as well. Why this is the case is unclear. It may be partly due to better information of farmers in general – either through RML or through the use of mobile phones more generally.

7.3 Costs and revenues

RML may affect farmers in ways other than prices. Transaction costs per transaction average 0.84 Rs/Kg. This compares to standard deviations for prices of 2.2, 17.1, 4.6, 0.9, and 3.1 Rs/Kg for tomato, pomegranate, onions, wheat, and soya, respectively. There is therefore room for farmers to increase revenues by reducing transactions costs.

[17] Given that mandi and farmer prices come from two different sources, this comparison should be taken as indicative only. Mandi prices are averaged over the year without weighting for traded quantities for which we have no information. Farmer prices are similarly averaged over transactions without weighting for quantity. We can only imperfectly control for differences in quality. For instance, onion wholesale prices at the mandi are reported for specific qualities of onion, but surveyed farmers did not report information on the quality of the onion they sold. These two sources of differences between the two prices are likely to affect the two years similarly. Consequently, the year-to-year comparison is probably more accurate than the comparison in levels within each year.

In the first column of Table 10, we report ATT and IV estimates for total transactions costs on the farmer's last crop sale. Transactions cost include transport, loading and off-loading, payment at check-points, personal transport, processing, and commissions. Point estimates are positive for the whole sample – suggesting that RML raises costs – but they become negative when we only use treatment 1 villages. In both cases, however, point estimates are not significant.

In the next column we investigate whether farmers received a higher net price. Mattoo, Mishra and Narain (2007) estimate that transport costs per truck in India are in the range of 0.09 to 0.13 Rs/kg/100 Km, which is small relative to total transactions costs. It thus seems that, in terms of transport cost at least, arbitraging over space is not prohibitively expensive relative to other transactions costs. If farmers use RML information to arbitrage across space, they may ship their crop to a more distant market, incur a higher transport cost, but obtain a higher price net of costs, as in Jensen (2007). This is not what we find: results remain resolutely non-significant, whether we include treatment 2 villages or not.

Farmers may gain not on the unit price but on total revenue. This is investigated in column 3. We find large positive point estimates, but no significant effect.[18] If we use logs instead to limit the possible influence of outliers, we again find no significant effect. The last column reports similar results for value added, that is, revenues minus monetary input costs such as fertilizer and pesticides. If weather information and crop advisories raise farmers' technical and allocative efficiency, we would expect value added to rise. Results are very similar to those for sale revenues: large positive point estimates, but nothing statistically significant. Similar findings obtain if we use logs instead.

7.4 Marketing

In the conceptual section we argued that if RML information is used by farmers to increase the price they receive, we should observe differences in marketing practices. The arbitrage model presented in Section 3 indeed suggests that if price information makes it possible for farmers to arbitrage across markets, we should observe systematic changes in where farmers sell. To investigate this possibility, we examine where farmers sell their crops.

We first note that most sales take place in a market, nearly always a wholesale market or *mandi*. The only exceptions are pomegranate for which, at baseline, 44% of sales are conducted at the farm-gate and, to a lesser extent, wheat, with 7% of farm-gate sales. For the other crops, farm-gate sales represent less than 2% of recorded sales. Secondly, market diversification varies from crop to crop. Sales of perishable crops tend to be geographically concentrated: 98%

[18] Sale values are large because quantities sold are large. This is especially true for onion and pomegranate where the average size of a single transaction is 10 metric tons. There is, however, a lot of variation around this average.

and 81% of all market sales of tomato and pomegranate, respectively, occur at a single district market. Concentration is less marked for other crops: for onions, 51% of all sales go to one district market. Corresponding figures for wheat and soya are 54% and 57%, respectively.

To investigate whether treatment changed where farmers sell their crops, we construct an overlap index that captures the extent to which a farmer sold to the same location in the baseline and ex post surveys. There are 39 wholesale markets in total in the data, and farm-gate sales are regarded as an additional location. The index is weighted by quantity. An index value of 1 means the farm sold in the same location in the two survey rounds; a value of 0 means that nothing was sold at the same place. We also construct an added market dummy, which takes value 1 if the farmer sold in a new market or location in the second survey round, and a dropped market dummy, which takes value 1 if the farmer stopped selling in a specific location in the second round.

Average treatment effects for the market overlap index and for the added and dropped market dummies are reported in Table 11. In the top panel of the Table we use the entire sample; in the second panel we only use treatment 1 and control samples. When we use the entire sample, we find treatment has a significant effect: treated farmers are 10 percentage points more likely to add a new location (market or farm-gate) where they sell their crop, and 9 percentage points more likely to drop one location. Treatment also reduces the overlap index by 10% on average. When we instrument RML usage with assignment to treatment, point estimates double and remain significant. These results are consistent with the predictions of the arbitrage model although, as we have seen in the previous two sub-sections, changing sales location does not appear to have resulted in a higher price on average. Point estimates are also slightly smaller when we limit the sample to treatment 1 and control villages (second panel of Table 11), but they are no longer statistically significant at the 10% level or better, perhaps because of the reduction in sample size.

We continue our investigation of crop marketing in Table 12. The unit of analysis is an individual sale transaction. We first examine whether farmers sell at a wholesale market or *mandi*. As we have discussed earlier, farmers may choose to sell at the mandi because it is the only way to obtain accurate price information, even though doing so raises transactions costs relative to farm-gate sales. If this is the case, the RML service may give farmers the confidence not to sell at the wholesale market, for instance because they can better negotiate with a farm-gate buyer.

To investigate this possibility, we test whether treated farmers are less likely to sell at the mandi. Results, reported in the first column of Table 12, indicate that this is not the case: the intent-to-treat ATT, reported at the top of column 1, is to raise the likelihood of selling at the mandi. In the rest of column 1 we examine whether the results are different when we use

IV-LATE estimates instead, or when we allow for heterogeneous effect by firm size and farmer age. Results are qualitatively similar. The magnitude of the effect, however, is small, probably because most farmers already sell at the mandi. When we differentiate the effect by crop, it is significant for pomegranate (point estimate 0.157 with z-value of 2.35) and – less so – for soya (point estimate 0.079 with z-value of 1.73); it is not significant for the other three crops. The fact that pomegranate is most affected is hardly surprising given that pomegranate is the only crop with a sizeable proportion of farm-gate sales at the baseline. Thus, if anything, RML makes farmers *more* likely to sell at the mandi.

Among farmers who sell at the market, however, Table 11 has shown a change in crop destination. To verify this further, farmers who sell at a particular wholesale market were asked whether they do so because this is the closest market. We see from the second column of Table 12 that treated farmers are less likely to say they sell at a market because it is closest. Taken together, the evidence therefore suggests that treated farmers are more likely to sell further away from home – either by switching from farm-gate to market sale, or by switching to a more distant mandi.

To investigate this further, we test whether treated farmers are more likely to sell directly to a trader (typically at the farm-gate) or without the help of a commission agent. If RML improves price information, farmers may be less reluctant to sell to a trader, knowing they can insist on a price commensurate to the price at the nearest mandi. By a similar reasoning, they may rely less on commission agents who are contractually obliged to help farmers get the best price but to whom a fee must be paid. Table 12 shows this is not the case for commission agents – the ATT is not significantly different from 0 in any of the three methods we report. For selling to a trader, we find a weakly significant ATT but significance disappears when we use the IV-LATE methodology or allow for heterogeneous effects. In the heterogeneous effect regression reported the last panel of Table 12, we see that young farmers are much more likely to report selling to a trader, but this relationship disappears with treatment, suggesting that young farmers learn not to sell to traders.

Taking columns 1 to 4 together, the evidence suggests that RML helped some farmers realize that they could obtain a higher price by going to a more distant mandi rather than selling at a closer market or at the farm-gate. It is possible that some farmers choose to sell locally because of uncertainty regarding the return to travelling to the more distant mandi. Providing information about the mandi price reduces the risk of travelling to the mandi, and the reduction in uncertainty may be what induced some farmers to incur the additional cost of travelling to the mandi. In contrast, the evidence does not support the hypothesis that better informed farmers are emboldened to sell in local markets or at the farm-gate because they can insist on receiving a price more in line with the regional wholesale price.

Finally, RML provides information on the price spread due to crop quality, i.e., it shows prices by grade. This may make farmers more aware of the benefit from grading because of the price premium that high quality commands. Consequently, we expect treated farmers to pay more attention to quality, for instance by grading or sorting their output into separate categories so as to obtain a better price on the top quality. This is what we find – see the last column of Table 12 – for the ATT where the effect is statistically significant. The magnitude of the effect, however, is small: treatment raises the proportion of farmers grading or sorting their output by 3 percentage points. The effect also disappears in the IV-LATE regression, but it resurfaces in the heterogeneous effect regression, but only when interacted with farmer age – i.e., young farmers are slightly more likely to grade or sort their output as a consequence of treatment.

More specific information was collected in the survey with respect to the last crop sale. Answers to these questions are analyzed in Table 13. Farmers were asked whether they speeded sale because they expected the price to fall. RML information may affect how farmers form price expectations – e.g., through information about market-relevant events. Better informed farmers may thus be able to 'beat the market' by selling faster, a bit like better informed investors can withdraw faster from a sinking stock. We find no evidence that this is the case for the average farmer in the sample. But as shown in the bottom panel of Table 13 young farmers report delaying sales if they expect the price to rise but speeding up harvest if they expect the price to fall.

To avoid price uncertainty farmers may enter into long term contracts with a buyer who guarantees a future price. This possibility has long attracted the attention of Indian policy makers who fear that buyers may use future delivery contracts to exploit farmers' risk aversion and secure lower prices. Providing farmers with better information may reduce subjective uncertainty and hence farmers' willingness to engage in forward crop sales. We find no evidence of this, as shown in the next column of Table 13: reduction in price uncertainty does not induce more (or less) usage of forward sales. This is consistent with findings reported in Fafchamps, Hill and Minten (2008) that, in India, forward sales of crops do not tie in the future price; the buyer agrees instead to pay the going price at the time of sale. This interpretation is confirmed by the next column in Table 13, which shows whether farmers agreed a price with a buyer before harvest: we find no statistically significant effect of treatment in any of the regressions.

Next we examine whether treatment helped farmers economize on transport costs to the market for themselves and their crop: better informed farmers may indeed choose to sell locally rather than in a distant mandi. We find no evidence that farmers economize on transport costs. Finally we investigate whether farmers economize on market fees by avoiding commission agents who are known to charge large fees (Banerji and Meenakski 2004, Minten et al., 2010). We find no evidence that treatment reduces the likelihood of paying market fees for the average farmer in the sample. We do, however, find that treatment increases the likelihood of paying market fees for young farmers, a result that is in line with our earlier finding on Table 12 that young farmers are more likely to sell at the mandi as a consequence of treatment. We also find that large farmers are more likely to pay the market fee as a result of treatment, but statistical significance is quite low.

7.5 Weather information

RML provides weather forecasts that are spatially disaggregated – and hence presumably more accurate than those publicized on the radio. Do RML forecast help farmers improve yields, for instance because farmers can take better anticipative action?

We investigate this question in Table 14. Farmers were asked whether or not they incurred unusually high rainfall event, such as a storm or heaving downpour. Some 58% said they did. The first column of Table 14 shows that the likelihood of reporting a storm is correlated with treatment in the IV and heterogeneous effect regressions: the results suggest that treated farmers are less likely to report that they did not incur a storm or heavy rain – or put differently, they are more likely to report incurring a storm. Since we have no reason to believe that the weather is correlated with treatment, this is most likely due to response bias: farmers who receive regular weather information may become more aware of unusually high rainfall events, and thus be more likely to report them to enumerators.

In the remaining columns of Table 14, we test whether treated farmers were able to reduce output loss or increase output following a storm. We find no evidence that this is the case. We also find little evidence of beneficial heterogeneous effects: young farmers report more output loss at harvest following a storm, not less.

7.6 Agricultural technology and practices

In addition to price and weather information, RML provides crop advisory messages relaying information on crop varieties, pesticide use, and cultivation practices. This information may be particularly valuable for sample farmers because some of our target crops are relatively new to them.

In Table 15 we examine whether farmers changed the variety of the target crop that they grow. Some 31% of respondents stated that they did change variety between the two survey years, but this proportion is the same irrespective of treatment. Of those who changed variety, 65% stated they did so to improve profitability. Again we find no statistical relationship with treatment. Farmers who stated they changed crop to improve profitability were asked whether they did so because of RML. Here we find a statistically positive treatment effect: depending on the estimation method, treated farmers are 14-20% more likely to list RML as inspiration for the change. This is reassuring, but not necessarily very conclusive given that treatment is found to have no effect on the propensity to change variety or on the reason for changing variety.

In the last two columns of Table 15 we turn to cultivation practices. In 2010 farmers were asked whether they changed anything to their cultivation practices since the previous year; 22% of respondents stated they did so. We find no evidence that treated farmers were more likely to change cultivation practices.

Those who did change were asked what made them change their practice. Of those farmers who report a change, a large proportion mention RML as the reason for the change. The effect is statistically significant and large in magnitude – e.g., a 20-41% higher likelihood of listing RML, depending on the estimator. As for crop variety, this evidence is reassuring but not very conclusive given that treatment has no noticeable effect on changing crop practices itself.

8 Conclusion

In this paper we have reported the results of a randomized controlled trial of the impact of an SMS-based agricultural information service in Maharashtra. This information service, called Reuters Market Light or RML, sends SMS to farmers with information on prices, weather forecasts, crop advice, and general news items. The price information is expected to improve farmers ability to negotiate with buyers and to enable them to arbitrage better across space (e.g., different sales outlet). Weather information is expected to help farmers reduce crop losses due to extreme weather events such as storms. Crop advisory information is expected to induce farmers to adopt new crop varieties and improve their cultivation practices.

The trial was conducted in collaboration with Thompson-Reuters, the provider of RML. The experiment involved 933 farmers in 100 villages of central Maharashtra. Treatment was randomized across villages and, in some cases, across farmers as well. Participating farmers were surveyed twice in face-to-face interviews. We also conducted a follow-up telephone survey to gather information on reasons for non-compliance. Randomization appears to have worked well in the sense that the control and treatment samples are balanced on most relevant variables. Although contamination is limited, non-compliance is common, which is why we reported intent-to-treat estimates throughout, i.e., with respect to having received the offer of a free RML service. We also reported IV estimates in which the offer is used to instrument RML usage.

We find no statistically significant average effect of treatment on the price received by farmers, on crop losses resulting from rainstorms, or on the likelihood of changing crop varieties and cultivation practices. Treated farmers do appear to make use of the RML service and they associate RML information with a number of decisions they have made. But, based on the available evidence, it appears that on average they would have obtained a similar price or revenue, with or without RML.

While somewhat disappointing, our results are ultimately in line with the market take-up rate of the RML service in the study districts. After a rapid expansion following the introduction of the service in 2007-2009, take-up numbers show a relative stagnation in take-up over the 2009-2010 study period, suggesting a possible loss of interest. We cannot, however, rule out that supply side factors played a role. We also suspect that some farmers do not know how to renew the service.[19]

While the absence of positive effect on price may surprise and disappoint, we do find evidence of an RML information effect on where farmers sell their crop: they are less likely to sell at the farm-gate – especially young farmers – and more likely to sell at a different, more distant wholesale market. These results contradict the idea that RML information enables farmers to negotiate better prices with itinerant traders, hence rejecting the second model presented in the conceptual section. The results, however, are consistent with using RML information to arbitrage across sales locations. Why arbitrage does not translate into higher prices on average is unclear, but some possible explanations arise from the data. First, very few farmers sold at the farm-gate at baseline – except for pomegranate – thereby limiting the number of farmers who could realize that selling at the market was more beneficial than selling at the farm-gate, as a few did. Second, before treatment crop sales were concentrated on a single wholesale market in each district. Spatial concentration probably limits the range of alternative market destinations nearby – and thus opportunities for arbitraging by farmers.

We find little evidence of other RML effects. If RML information helps farmers improve crop quality, we should observe treated farmers changing agricultural practices, especially with respect to crop varieties and grading; we do not, except for grading but the magnitude of the effect is very small. We also find no significant effect on transactions costs, revenues, and value added.

Taken together, the evidence is consistent and compelling. Surveyed farmers sell almost exclusively to a wholesale agricultural market nearby. If traders have a comparative advantage in transport, e.g. because farmers do not know anyone they can trust in other markets (Gabre-Madhin 2001), trader arbitrage across markets should pretty much ensure that farmers cannot fetch a more remunerative price by selling elsewhere. Hence it is optimal for farmers to sell to the nearest market. Similarly, if farmers fear being cheated when they sell at the farm-gate, it

[19] The provider has indeed encountered difficulties in setting up a reliable system for enabling clients to easily and reliably make repeat purchases of the RML service.

is optimal for them not to do so. Given this, it is perhaps not so surprising that better price information did not translate into higher farmer prices.[20]

If the above interpretation is correct, it has a number of implications for the external validity of our findings. Price information could help if spatial arbitrage across agricultural markets does not hold, e.g., because markets are disorganized, segmented, or too thin to attract a steady flow of buyers – or because sellers have a comparative advantage in transport, as in Jensen (2007). Even in such a case, however, price information is likely to be used first by traders, as documented for instance by Aker (2008). Price information could also help farmers who sell at the farm-gate, such as the coffee growers studied by Fafchamps and Hill (2008). A stronger effect on crop quality may be obtained if price information is detailed by variety and grade, and farmers are provided with complementary information on how to produce high price varieties and grades. These suggestions should help steer policy intervention towards regions and markets where the effect of price information may be beneficial, and avoid wasting resources on markets where it is unlikely to matter.

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[20] Nothing in this argument relies on buyer competition within each market: even if buyers act in a monopsonistic fashion, as documented by Banerji and Meenakski (2004) for Delhi wheat markets, giving farmers information about market prices would not improve farmer prices – unless buyers price discriminate across markets and spatial arbitrage does not hold.

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Table 1. Number of agricultural holdings and RML subscribers in the five districts studied in Maharashtra

District:	Crop followed in survey	Number of agricultural holdings*	Number of RML subscribers**			
			2007	2008	2009	2010
Ahmadnagar	onion	916,724	711	1,377	3,763	1,637
Dhule	wheat	230,216	108	1,296	1,028	840
Latur	soya	305,706	163	914	1,048	826
Nashik	pomegranate	591,763	2,176	1,561	3,934	6,514
Pune	tomato	667,365	392	653	3,495	781
Total		2,711,774	3,550	5,801	13,268	10,598

*: Government of India, Agricultural Census, 2000/01

**: Thompson-Reuters

Table 2: Compliance and Contamination

	All villages			Treatment 1		Treatment 2		Control	
	Number of	RML		RML		RML		RML	
	villages	yes	no	yes	no	yes	no	yes	no
Intended experimental design				Number of households					
All	100	455	545	350	0	105	245	0	300
Tomato growers	20	91	109	70	0	21	49	0	60
Pomegranate growers	20	91	109	70	0	21	49	0	60
Onions growers	20	91	109	70	0	21	49	0	60
Wheat growers	20	91	109	70	0	21	49	0	60
Soya growers	20	91	109	70	0	21	49	0	60
Realized design or intent to treat									
All	100	422	511	325	0	97	239	0	272
Tomato growers	20	84	107	64	0	20	49	0	58
Pomegranate growers	20	89	107	68	0	21	52	0	55
Onions growers	20	88	105	68	0	20	49	0	56
Wheat growers	20	86	102	67	0	19	47	0	55
Soya growers	20	75	90	58	0	17	42	0	48
RML usage (from 2010 survey and phone interview)									
All households	100	247	686	181	144	56	280	10	262
Tomato growers	20	44	147	35	29	9	60	0	58
Pomegranate growers	20	65	131	42	26	19	54	4	51
Onions growers	20	44	149	36	32	8	61	0	56
Wheat growers	20	48	140	33	34	11	55	4	51
Soya growers	20	46	119	35	23	9	50	2	46
Extension agents:									
Intended design		30	70	15	20	15	20	0	30
Realized design		20	80	10	25	10	25	0	30

Table 3: Balancedness of treatment versus control in the 2009 baseline data

	Unit	Number of observation	Control group Mean	St. Dev.	Treatment* Coeff.	t-value
Household characteristics						
Education level head of household	years	911	8.19	4.36	0.243	0.68
Household size	number	933	6.43	2.70	0.131	0.61
Share of children in household	share	933	0.26	-	-0.008	-0.72
Share of elderly in household	share	933	0.08	-	0.011	1.38
Age head of household	years	922	49.51	12.93	0.409	0.42
Farm experience	years	930	26.86	13.92	-0.379	-0.39
Land ownership and cultivation						
Land owned	acres	933	9.62	8.62	0.677	0.64
Land cultivated of tomato in Pune	acres	191	1.79	3.01	-0.154	-0.40
Land cultivated of pomegranate in Nashik	acres	196	3.64	3.50	0.508	0.62
Land cultivated of onions in Ahmadnagar	acres	193	2.06	1.87	0.258	0.65
Land cultivated of wheat in Dhule	acres	188	5.76	3.95	-1.073	-1.65
Land cultivated of soya in Latur	acres	165	5.76	4.09	0.841	0.39
Total crop area cultivated	acres	933	14.78	13.04	1.340	0.88
Marketing characteristics studied crop						
Know market price of studied crop:						
- the day before he sold it	share	910	0.78	-	0.037	1.13
- the week before he sold it	share	912	0.38	-	0.070	1.58
- a month before he sold it	share	912	0.08	-	0.029	1.33
- when he planted it	share	912	0.06	-	0.020	1.67
For each transaction:						
- Prices obtained in each transaction	Rs/kg	1563	13.22	10.20	-0.149	-0.44
- Quantities sold per transaction	log(kgs)	1563	7.11	1.57	-0.067	-0.75
- Produce is sold in the village	share	1554	0.15	-	-0.016	-0.57
- Head of household made sale	share	1561	0.85	-	-0.021	-0.58
- Crop was graded/sorted before sale	share	1561	0.70	-	0.036	0.84
- Produce is sold through commission agent	share	1555	0.40	-	0.032	0.83
Number of sale transactions per farmer	number	894	1.74	1.19	-0.001	-0.01
Transaction costs last transaction						
Paid for transport of produce	share	908	0.88	-	0.013	0.45
Paid for personal transport	share	797	0.11	-	0.022	0.90
Sold through commission agent	share	905	0.57	-	-0.036	-0.80
Weather in 12 months prior to survey						
Did not incur storm/heavy rainfall	share	933	0.53	-	-0.021	-0.63
Technology changes in 12 months prior to survey						
Changed crop varieties	share	933	0.34	-	-0.020	-0.57
Changed cultivation practices	share	933	0.28	-	-0.004	-0.11

All variables refer to 2009 data

*village triplet code dummies and intercept included but not reported

Table 4: Information sources used (baseline; % of farmers)

	crop prices	weather prediction	crops to plant	cultivation practices	input use#	post-harvest practices
Own observation/experimentation	80	89	87	85	64	84
Direct observation of other farmers	28	16	26	25	16	23
Seller of agricultural input	7	2	7	8	49	5
Seller of agricultural output	32	5	12	12	16	13
Conversation with parents/relatives	23	20	29	28	18	27
Conversation with other farmers	62	55	54	53	56	49
Government extension agent	3	3	4	5	5	5
Newspaper/magazine	37	42	24	24	17	21
Radio/television	28	66	23	24	20	22
KVK-agricultural campus students/lecturers	0	0	1	1	1	1
Others	1	1	1	0	1	1

#: fertilizers, pesticides, and herbicides

Table 5: Use of RML

Whole sample		Use RML as one of the sources of information for: crop prices	weather prediction	crops to plant	cultivation practices	input use (d)	post-harvest practices
Number of observations		925	931	925	925	918	924
Nearest neighbor matching (a)							
ATT	Coeff	0.243	0.231	0.106	0.086	0.200	0.054
	z-value	**10.600**	**10.530**	**6.550**	**5.390**	**9.360**	**3.970**
Regression results (b)							
1. IV-LATE							
Treatment	Coeff	0.463	0.439	0.206	0.172	0.386	0.113
	t-value	**10.460**	**10.530**	**5.230**	**4.710**	**8.940**	**3.220**
Intercept	Coeff	0.007	-0.021	-0.008	-0.001	-0.044	0.044
	t-value	0.840	**-2.530**	-1.000	-0.150	**-5.080**	**6.300**
2. Heterogeneous effects (c)							
Treatment	Coeff	0.239	0.225	0.107	0.089	0.198	0.057
	t-value	**8.760**	**9.330**	**5.580**	**4.880**	**8.140**	**3.160**
Intercept	Coeff	-0.034	-0.470	-0.034	-0.026	-0.066	0.027
	t-value	**-1.950**	**-2.770**	**-2.450**	**-1.870**	**-4.320**	**2.000**
Dummy young head of household	Coeff	0.024	0.022	0.007	0.005	0.014	-0.003
	t-value	1.270	1.670	0.650	0.450	1.520	-0.300
Total crop area cultivated	Coeff	0.001	-0.000	0.001	0.001	0.000	0.001
	t-value	1.200	-0.020	**1.700**	**1.890**	0.430	**1.680**
Interaction with treatment							
Dummy young head of household	Coeff	0.045	0.018	0.057	0.019	0.007	0.034
	t-value	1.040	0.390	1.580	0.530	0.150	1.210
Total crop area cultivated	Coeff	0.002	0.004	-0.000	-0.001	0.002	-0.000
	t-value	**2.340**	**4.300**	-0.060	-0.700	**2.110**	-0.380

(a) Matching based on village triplet code dummies
(b) Village triplet code dummies included but not reported
(c) Mean value substracted from those control variables interacted with treatment
(d) fertilizers, pesticides, and herbicides
t-values based on standard errors clustered by village triplet code;
t-values in bold significant at the 10% level or better.

Table 6: Knowledge and information sharing

Whole sample		Know price before sale: one day	one week	one month	at planting	Share information farming	Collect price info: No of people consulted	Collect price at planting
Number of observations		722	723	722	722	922	929	925
Nearest neighbor matching (a)								
ATT	Coeff	0.084	0.095	0.097	0.078	0.063	0.035	0.005
	z-value	**3.100**	**2.830**	**3.090**	**2.540**	**4.050**	0.580	0.150
Regression results (b)								
1. IV-LATE								
Treatment	Coeff	0.130	0.158	0.181	0.146	0.119	0.068	0.011
	t-value	**2.180**	**1.980**	**3.280**	**1.960**	**4.080**	0.500	0.160
Intercept	Coeff	0.717	0.304	-0.034	0.010	0.676	1.520	0.564
	t-value	**64.780**	**20.640**	**-3.280**	**0.720**	**116.330**	**55.390**	**39.010**
2. Heterogeneous effects (c)								
Treatment	Coeff	0.065	0.073	0.084	0.068	-0.063	0.014	0.007
	t-value	**2.110**	**1.780**	**2.930**	**1.780**	**-4.080**	0.200	0.190
Intercept	Coeff	0.702	0.265	-0.104	-0.061	0.665	1.593	0.561
	t-value	**23.330**	**7.140**	**-3.510**	-1.540	**42.340**	**24.530**	**14.020**
Dummy young head of hh	Coeff	-0.002	0.030	0.038	0.035	-0.004	-0.229	-0.041
	t-value	-1.220	0.690	1.090	0.890	-0.160	**-2.400**	-0.970
Total crop area cultivated	Coeff	0.001	0.002	0.004	0.004	0.001	0.007	0.002
	t-value	1.220	1.430	**2.850**	**1.910**	0.780	**3.150**	1.240
Interaction with treatment								
Dummy young head of hh	Coeff	-0.010	-0.059	-0.048	0.013	0.037	0.021	0.089
	t-value	-0.220	-1.080	-0.830	0.220	1.180	0.150	1.500
Total crop area cultivated	Coeff	-0.001	-0.001	-0.000	-0.003	-0.001	-0.002	-0.002
	t-value	-0.960	-0.450	-0.450	-1.420	-0.680	-0.590	-1.130

(a) Matching based on village triplet code dummies
(b) Village triplet code dummies included but not reported
(c) Mean value substracted from those control variables interacted with treatment
t-values based on standard errors clustered by village triplet code;
t-values in bold significant at the 10% level or better.

Table 7: Prices obtained (expressed in log(Rs/kg))

		ATT (a)	ATT (b)	IV-LATE	OLS long-model(c)	Heterogeneous effects(d) OLS	IV
For whole sample(e)	No obs.	1480	688	1480	1425	1464	1457
Treatment	Coeff	-0.031	-0.043	-0.062	-0.028	-0.034	-0.026
	t-value	**-2.000**	-0.520	-1.670	-1.510	**-1.860**	-1.430
Intercept	Coeff			2.260	2.159	2.248	2.249
	t-value			**309.620**	**21.250**	**93.64**	**99.080**
Dummy young head of hh	Coeff				0.021	-0.013	-0.013
	t-value				0.990	-0.500	-0.530
Total crop area cultivated	Coeff				0.005	0.001	0.001
	t-value				**5.720**	**1.900**	1.550
Dummy if sold to a trader	Coeff				-0.011	-0.006	-0.008
	t-value				-0.250	-0.190	-0.290
Treatment extension agent	Coeff				-0.013		
	t-value				-0.500		
Interaction with treatment							
Dummy young head of hh	Coeff					0.057	0.059
	t-value					**1.750**	**1.850**
Total crop area cultivated	Coeff					-0.001	-0.000
	t-value					-0.590	-0.240
Dummy if sold to a trader	Coeff					0.085	0.091
	t-value					**1.750**	**1.830**
For control/treatment 1 villages(	No obs.	947	443	947	909	938	931
Treatment	Coeff	-0.015	0.031	-0.079	-0.017	-0.046	-0.032
	t-value	-0.600	0.630	-1.600	-0.510	**-1.740**	-1.170
Intercept	Coeff			2.211	2.071	2.218	2.209
	t-value			**147.890**	**15.100**	**61.610**	**59.530**
Dummy young head of hh	Coeff				0.016	-0.014	-0.013
	t-value				0.490	-0.410	-0.360
Total crop area cultivated	Coeff				0.005	0.001	0.001
	t-value				**2.940**	0.950	0.800
Dummy if sold to a trader	Coeff				-0.053	-0.020	-0.016
	t-value				-0.970	-0.770	-0.510
Treatment extension agent	Coeff				-0.048		
	t-value				-1.000		
Interaction with treatment							
Dummy young head of hh	Coeff					0.041	0.038
	t-value					1.040	0.910
Total crop area cultivated	Coeff					0.000	0.000
	t-value					0.050	0.180
Dummy if sold to a trader	Coeff					0.101	0.093
	t-value					**2.210**	**1.820**

(a) impact survey only; using nearest neighborhood matching; the reported coefficient on treatment is the ATT
(b) diff-in-diff, nearest neighborhood matching; using average unweighted prices in baseline and impact survey
(c) including but not reported dummies for graded, sold through commission agent, sold to trader, immediate payments, and quantity sold, years of education head of household, social network in village, land owned, years of farm experience, area cultivated of studied crop
(d) Mean value substracted from those control variables interacted with treatment
(e) village triplet code dummies included but not reported
t-values based on standard errors clustered by village triplet code;
t-values in bold significant at the 10% level or better.

Table 8: Prices obtained (expressed in log(Rs/kg)), per crop

		Tomato	Pomegranate	Crop Onions	Wheat	Soya
For whole sample						
Number of observations		786	222	204	124	107
Nearest neighbor matching (a)						
ATT	Coeff	-0.006	-0.073	-0.125	-0.012	-0.006
	z-value	-0.380	-1.370	**-1.940**	-0.810	-0.170
Regression results (b)						
IV-LATE						
Treatment	Coeff	-0.019	-0.115	-0.247	-0.019	0.004
	t-value	-0.690	-1.140	-1.110	-1.250	0.080
Intercept	Coeff	2.324	2.672	2.300	2.520	3.044
	t-value	**182.010**	-	**53.570**	**998.510**	**-**
For control/treatment 1 villages						
Number of observations		493	149	132	76	65
Nearest neighbor matching (a)						
ATT	Coeff	-0.039	-0.043	-0.122	-0.025	0.049
	z-value	-1.880	-0.720	-1.520	-1.170	0.970
Regression results (b)						
IV-LATE						
Treatment	Coeff	-0.096	-0.027	-0.171	-0.053	0.090
	t-value	**-2.160**	-0.230	-0.590	-0.910	1.330
Intercept	Coeff	2.390	3.427	2.238	2.546	3.371
	t-value	**97.370**	**53.340**	**25.740**	**73.550**	-

(a) Matching based on village triplet code dummies
(b) Village triplet code dummies included but not reported
t-values based on standard errors clustered by village triplet code;
t-values in bold significant at the 10% level or better.

Table 9: Observed prices (in Rs/kg) in 2009 and 2010

	RML wholesale market data (a)				Household survey (b)			
	No of obs.	Mean	Median	St. Dev.	No of obs.	Mean	Median	St. Dev.
2008-2009								
Tomato	179	8.53	6.10	4.91	601	5.58	5.00	3.20
Pomegranate	106	56.16	56.00	4.81	425	25.89	25.00	8.41
Onions	64	6.59	5.85	2.03	201	7.16	7.00	3.25
Wheat	226	12.29	12.25	0.53	183	10.91	10.91	1.01
Soya	222	22.28	22.95	3.53	153	20.24	20.00	5.73
2009-2010								
Tomato	119	8.86	9.50	3.38	786	9.77	10.00	2.19
Pomegranate	24	95.50	90.00	34.47	222	37.84	35.00	17.15
Onions	37	6.06	6.45	1.31	204	10.31	9.00	4.58
Wheat	120	14.76	14.25	1.41	124	12.14	12.00	0.94
Soya	113	21.75	21.93	1.36	107	21.08	20.50	3.05

(a) prices of the crop studied in the selected district; price of "average" quality, except for wheat/soya ("high" used) as not enough observations for average quality in 2009-2010; prices of varieties of *pomegranate bhagwa* and *onion unhal large* reported

(b) average over all reported transactions of reporting households

Table 10: Profitability measures

		Transaction cost (c)	Net price (d)	Sale revenues	Value added (e)
For whole sample					
Number of observations		713	713	713	713
Nearest neighbor matching (a)					
ATT	Coeff	0.078	-0.760	48,247	46,352
	z-value	1.420	-1.480	0.580	0.580
Regression results (b)					
IV-LATE					
Treatment	Coeff	0.146	-1.450	87,074	84,530
	t-value	1.050	-1.730	0.880	0.910
Intercept	Coeff	1.576	8.906	66,545	59,235
	t-value	**59.060**	**55.350**	**3.500**	**3.320**
For control/treatment 1 villages					
Number of observations		458	458	458	458
Nearest neighbor matching (a)					
ATT	Coeff	-0.150	0.735	143,852	138,311
	z-value	-1.700	1.060	1.190	1.220
Regression results (b)					
IV-LATE					
Treatment	Coeff	0.159	-0.074	267,588	260,249
	t-value	0.439	-1.000	1.370	1.410
Intercept	Coeff	1.602	1.977	-22,749	-27,914
	t-value	**25.230**	**84.880**	-0.370	-0.480

(a) Matching based on village triplet code dummies
(b) Village triplet code dummies included but not reported
(c) last transaction only; costs for transport, loading, off-loading,
payments at check-point/toll or road-block, personal transport, processing, commission
expressed in Rs/kg
(d) last transaction only; gross price minus transaction costs expressed in Rs/kg
(e) sales minus monetary input costs (fertilizer, pesticides, spray, purchased seeds, manure)
t-values based on standard errors clustered by village triplet code;
t-values in bold significant at the 10% level or better.

Table 11: Spatial arbitrage and market changes

		Number of markets		Overlap
		added	dropped	index (c)
For whole sample				
Number of observations		691	691	691
Nearest neighbor matching (a)				
ATT	Coeff	0.099	0.087	-0.095
	z-value	**2.980**	**2.680**	**-3.030**
Regression results (b)				
IV-LATE				
Treatment	Coeff	0.208	0.194	-0.197
	t-value	**2.120**	**2.080**	**-2.090**
Intercept	Coeff	0.575	0.463	0.493
	t-value	**30.430**	**25.850**	**27.290**
For control/treatment 1 villages				
Number of observations		445	445	445
Nearest neighbor matching (a)				
ATT	Coeff	0.045	0.074	-0.077
	z-value	0.880	1.560	-1.650
Regression results (b)				
IV-LATE				
Treatment	Coeff	0.187	0.189	-0.198
	t-value	1.260	1.320	-1.400
Intercept	Coeff	0.629	0.503	0.489
	t-value	**13.540**	**11.230**	**11.110**

(a) Matching based on village triplet code dummies

(b) Village triplet code dummies included but not reported

(c) overlap index of sales location between years, weighted by quantity -- see text for details

t-values based on standard errors clustered by village triplet code;

t-values in bold significant at the 10% level or better.

Table 12: Other marketing characteristics, all transactions

		Sold in whole-sale market	if whole-sale market, chosen because closest	Sold through a commis-sion agent	Sold to trader	Crop was graded/ sorted before sale
For whole sample						
Number of observations		1477	1352	1482	1470	1478
Nearest neighbor matching (a)						
ATT	Coeff	0.030	-0.078	0.006	0.046	0.033
	z-value	**2.540**	**-3.220**	0.230	**1.740**	**2.260**
Regression results (b)						
1. IV-LATE						
Treatment	Coeff	0.063	-0.131	0.539	0.084	0.055
	t-value	**1.750**	-0.940	0.844	1.050	1.120
Intercept	Coeff	0.923	0.199	0.933	0.450	0.925
	t-value	**132.800**	**6.940**	**89.080**	**28.350**	**98.140**
2. Heterogeneous effects (c)						
Treatment	Coeff	0.032	-0.064	0.054	0.039	-0.029
	t-value	**1.820**	-1.010	0.620	1.010	-1.120
Intercept	Coeff	0.955	0.277	0.898	0.355	0.045
	t-value	**57.990**	**4.760**	**19.650**	**6.900**	**2.830**
Dummy young head of hh	Coeff	-0.013	-0.091	0.049	0.140	-0.024
	t-value	-1.080	**-2.100**	0.690	**2.950**	-1.180
Total crop area cultivated	Coeff	-0.002	-0.001	-0.001	0.001	-0.002
	t-value	-1.620	-0.400	-0.580	0.570	-1.410
Interaction with treatment						
Dummy young head of hh	Coeff	0.011	0.094	-0.015	-0.159	0.052
	t-value	0.590	0.460	-0.120	**-2.330**	**1.950**
Total crop area cultivated	Coeff	0.003	0.002	-0.008	-0.002	0.002
	t-value	**1.790**	0.460	-1.330	-1.130	1.380

(a) Matching based on village triplet code dummies
(b) Village triplet code dummies included but not reported
(c) Mean value substracted from those control variables interacted with treatment
t-values based on standard errors clustered by village triplet code;
t-values in bold significant at the 10% level or better.

Table 13: Transaction characteristics last transaction

		Delayed sales because expect price to rise	Speed up harvest because expect price to fall	Speed up sales because expect price to fall	Promised to sell to specific buyer at planting	Agreed upon price with buyer before harvest	Paid for transport of produce to market	of person to market and back	Paid market fee
Number of observations		722	715	714	721	721	701	630	700
Nearest neighbor matching (a)									
ATT	Coeff	-0.004	0.010	-0.019	0.020	0.002	0.003	-0.029	-0.013
	z-value	-0.190	0.870	-0.960	1.140	0.320	0.110	-0.850	-1.070
Regression results (b)									
1. IV-LATE									
Treatment	Coeff	0.001	0.016	-0.029	0.028	0.003	0.009	-0.064	-0.017
	t-value	0.010	0.810	-0.960	0.640	0.250	0.180	-0.970	-0.650
Intercept	Coeff	0.185	0.108	0.079	0.032	-0.001	0.998	0.382	0.080
	t-value	**15.380**	**30.52**	**14.180**	**3.960**	-0.250	**112.150**	**31.250**	**19.240**
2. Heterogeneous effects (c)									
Treatment	Coeff	0.000	0.008	-0.016	0.011	0.001	0.007	-0.046	-0.010
	t-value	0.000	0.830	-0.970	0.530	0.210	0.280	-1.510	-0.770
Intercept	Coeff	0.200	0.104	0.052	0.019	0.009	1.051	0.319	0.111
	t-value	**7.080**	**10.770**	**2.830**	0.820	0.980	**27.700**	**5.960**	**6.170**
Dummy young head of hh	Coeff	-0.051	0.006	0.042	0.007	-0.014	-0.046	0.044	-0.034
	t-value	**-1.810**	0.450	1.610	0.320	-1.120	-1.470	0.690	**-1.910**
Total crop area cultivated	Coeff	0.001	-0.000	0.000	0.001	-0.000	-0.002	0.004	-0.001
	t-value	1.230	-1.160	0.520	0.660	-0.120	-1.040	**1.700**	**-2.370**
Interaction with treatment									
Dummy young head of hh	Coeff	0.123	0.031	-0.077	0.017	0.001	0.033	-0.048	0.064
	t-value	**2.870**	**1.880**	**-1.700**	0.470	0.080	0.680	-0.860	**2.750**
Total crop area cultivated	Coeff	-0.001	0.000	-0.001	0.001	0.000	-0.000	-0.003	0.001
	t-value	-0.630	0.990	-0.930	0.670	1.010	-0.170	-1.160	**1.740**

(a) Matching based on village triplet code dummies
(b) Village triplet code dummies included but not reported
(c) Mean value substracted from those control variables interacted with treatment
t-values based on standard errors clustered by village triplet code;
t-values in bold significant at the 10% level or better.

Table 14: Weather information

For whole sample		Did not incur storm/ heavy rain	Because of storm, heavy rainfall, output loss before harvest	output loss at harvest	output increase
Number of observations		915	529	529	529
Nearest neighbor matching (a)					
ATT	Coeff	-0.031	0.029	-0.042	0.046
	z-value	-1.000	0.720	-1.240	1.620
Regression results (b)					
1. IV-LATE					
Treatment	Coeff	-0.108	-0.034	-0.025	0.095
	t-value	**-2.080**	-0.450	-0.310	1.390
Intercept	Coeff	0.288	0.690	0.369	0.024
	t-value	**27.740**	**39.630**	**19.860**	1.540
2. Heterogeneous effects (c)					
Treatment	Coeff	-0.062	-0.027	-0.009	0.050
	t-value	**-2.280**	-0.660	-0.220	1.380
Intercept	Coeff	0.294	0.723	0.337	0.006
	t-value	**7.730**	**20.820**	**8.140**	0.180
Dummy young head of hh	Coeff	-0.053	-0.050	0.044	0.015
	t-value	-1.130	-1.040	**1.710**	0.410
Total crop area cultivated	Coeff	0.003	0.000	-0.001	0.000
	t-value	**2.380**	0.390	-0.290	0.310
Interaction with treatment					
Dummy young head of hh	Coeff	0.023	-0.056	0.078	-0.019
	t-value	0.370	-0.940	**1.850**	-0.440
Total crop area cultivated	Coeff	-0.003	0.001	0.001	-0.001
	t-value	**-1.730**	0.470	0.310	-0.630

(a) Matching based on village triplet code dummies
(b) Village triplet code dummies included but not reported
(c) Mean value substracted from those control variables interacted with treatment
t-values based on standard errors clustered by village triplet code;
t-values in bold significant at the 10% level or better.

Table 15: Crop varieties grown and cultivation practices

For whole sample		Change of crop variety since last year	If yes, reason is profita-bility	If profita-bility, because of RML	Change in cultivation practices last year	If change, because of RML
Number of observations		895	240	156	911	203
Nearest neighbor matching (a)						
ATT	Coeff	0.029	0.020	0.155	-0.027	0.211
	z-value	1.100	0.460	**2.830**	-1.110	**3.990**
Regression results (b)						
1. IV-LATE						
Treatment	Coeff	0.043	0.006	0.200	-0.045	0.410
	t-value	0.970	0.090	**2.060**	-1.240	**5.170**
Intercept	Coeff	0.525	0.374	-0.033	0.476	-0.016
	t-value	**59.350**	**30.950**	**-2.060**	**65.780**	-0.950
2. Heterogeneous effects (c)						
Treatment	Coeff	0.021	-0.003	0.140	-0.025	0.199
	t-value	0.910	-0.080	**2.220**	-1.270	**3.580**
Intercept	Coeff	0.518	0.408	-0.112	0.432	-0.051
	t-value	**17.210**	**9.280**	**-5.680**	**14.940**	-1.270
Dummy young head of hh	Coeff	-0.001	-0.079	0.103	0.042	0.036
	t-value	-0.030	-1.280	**2.350**	0.890	0.850
Total crop area cultivated	Coeff	0.001	0.002	0.001	0.001	0.000
	t-value	0.770	1.640	0.980	1.310	-0.030
Interaction with treatment						
Dummy young head of hh	Coeff	-0.091	0.053	-0.001	-0.009	0.033
	t-value	**-1.760**	0.680	-0.010	-0.150	0.220
Total crop area cultivated	Coeff	-0.002	0.000	0.000	-0.001	0.001
	t-value	-1.240	-0.120	0.100	-0.910	0.210

(a) Matching based on village triplet code dummies
(b) Village triplet code dummies included but not reported
(c) Mean value substracted from those control variables interacted with treatment
t-values based on standard errors clustered by village triplet code;
t-values in bold significant at the 10% level or better.