Do Safety Nets Promote Technology Adoption? Panel Data Evidence from Rural Ethiopia*

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Abstract

We use panel data from rural Ethiopia to investigate if participation in a safety net program enhances fertilizer adoption. Using a difference-in-difference estimator and inverse propensity score weighting we find that participation in Ethiopia's food-for-work program increased fertilizer adoption. Results also indicate that the likelihood of adopting and the intensity of fertilizer usage increased with livestock holdings for food-for-work-participant households providing some evidence that the intervention helped asset-rich farm households more than asset-poor households. We find no significant effects of free distribution on fertilizer adoption or intensification. Our results are consistent with the hypothesis that safety nets can be viewed as mechanisms that allow households to take on more risk to pursue higher profits. The paper highlights important policy implications related to the inter-related dynamics of safety nets and extension services that aim at promoting productivity enhancing modern agricultural technologies.

Keywords: Safety Net, Fertilizer Use, Inverse Propensity Score Weighting,

JEL: O12, O33, Q12, Q16

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1 Introduction

There is a rich literature investigating why poor households are unlikely to adopt risky technology (Feder et al., 1985). The availability of inputs, uncertainty about profitability, and credit constraints are a few of the explanations that have received the most attention in academic research and in government resources. However, few studies have (empirically) looked at the role of insurance (the ability to smooth consumption) in explaining technology usage.[1] The adoption of income-improving technologies is hindered by farmers ability and willingness to access credit. On the supply side, asymmetric information and imperfect enforcement lead to credit rationing. On the demand side, imperfections in the insurance market, the inability of households to protect against downside shocks, lead to risk rationing where farmers are less willing to take on risk and voluntarily withdraw from the credit market (Binswanger and Sillers, 1983; Boucher et al., 2008; Dercon and Christiaensen, 2011). In short, poor households that are ill-equipped to handle negative shocks may engage in less risky, less profitable activities.[2] In this paper we add to the literature on technology adoption by investigating the role safety nets play in the likelihood of a household taking on more risk. Identifying mechanisms which allow households to overcome the initial uncertainty of adopting a potentially profitable technology has important policy implications for poverty alleviation.

Dercon and Christiaensen (2011) investigate the ability of households to take on risky production technologies in the presence of credit and insurance constraints. A key prediction of their model is that households will adopt fewer risky inputs when they face higher ex-post downside consumption risk. Using data from rural Ethiopia, we investigate this further and explore the role that food aid can play in the adoption and usage of fertilizer (a risky production technology). Food aid programs are implemented to protect against ex-post downside consumption risk and in essence can mitigate the adverse effects of shocks allowing households to engage in higher return, higher risk activities.[3]

At the same time, food aid may provide a disincentive to households to invest in productive assets and may lead households to become dependent upon aid. Previous studies investigated the impact of safety nets on household behavior and outcome. Andersson et al. (2011) investigate the impact of the Ethiopian Productive Safety Net Program on rural households livestock wealth and tree

[1] Gine and Yang (2009) is one of the few studies which empirically explores the relationship between insurance and technology adoption. The authors implement a randomized field experiment where they offer credit to purchase a new crop technology. The treatment group was also required to purchase a weather insurance policy. The authors found that take-up was lower among farmers offered insurance with the loan.

[2] These environments can lead to risk-induced poverty traps where households which are able to insure their consumption against income shocks engage in more profitable activities and escape poverty, while others are stuck with low-return, low risk activities trapping themselves into poverty.

[3] Other papers have explored the demand for formal insurance among farmers (Gine and Yang, 2009; Gine et al., 2008). Unlike these papers we explore an insurance mechanism, in the form of food aid, which does not require a direct cost to purchase.

planting. They find a positive impact of the program on the number of trees planted by households but no impact in livestock holdings. Gilligan and Hoddinott (2007) on the other hand study the impact of the emergency food aid programs after the 2002 drought in rural Ethiopia which we consider in the current paper. These authors find that participation in food-for-work increased growth in total consumption and food consumption for participating households while free food distribution helped to raise growth in food consumption but impacted negatively on food security. In this paper, we add to the literature on the effects that food aid has on household behavior by exploring how it affects fertilizer usage, a critical productivity-enhancing modern agricultural input. Understanding how the intervention affected technology adoption is important to have a full picture of its impact on the different aspects of household outcomes.

Identifying the effect of food aid on household behavior is hindered due to selection into the program; assignment of treatment is not random. We address the non-random assignment of aid allocations by using inverse-propensity score weighting and a difference-in-difference estimator by exploiting the expansion in Ethiopia's food aid program during the 2002 drought. We find that participation in Ethiopia's food-for-work program increased fertilizer adoption. We also find that the likelihood of adopting and the intensity of fertilizer usage increased with livestock holdings for food-for-work-participant households providing some evidence that the intervention helped asset-rich farm households more than asset-poor households. We find no significant effects of free distribution on fertilizer adoption or intensification. Our results are consistent with the hypothesis that safety nets can be viewed as mechanisms that allow households to take on more risk to pursue higher profits. The paper highlights important policy implications related to the inter-related dynamics of safety nets and extension services that aim at promoting productivity enhancing modern agricultural technologies.

The rest of the paper is structured as follows. Section 2 presents a theoretical discussion of the role food aid can play in technology adoption. Section 3 discusses the data. Section 4 presents the identification strategy and the econometric model. Section 5 presents mean treatment effects and section 6 concludes.

2 Theory

The decision to adopt productivity-improving technologies depends on households' willingness and ability to access credit. Credit market imperfections make it difficult for farmers in developing countries to access credit in order to adopt profit-maximizing production technologies. The combination of information asymmetries and limited liability make providing credit riskier for the lender

than the innate cause of uncertainty. Adverse selection, moral hazard, and imperfect enforcement are examples of factors which restrict poor households access to credit.

At the same time, imperfections in the insurance markets may affect farmers' willingness to demand credit (Binswanger and Sillers, 1983; Boucher et al., 2008; Dercon and Christiaensen, 2011). Dercon and Christiaensen (2011) provide a model that links imperfections in insurance markets to inefficiencies in production choices. The authors show that imperfections in credit markets and uninsured consumption risk results in less than optimal usage of modern inputs.

Boucher et al. (2008) show that households may voluntarily withdraw from the credit market due to high-collateral contracts that provide lower expected utility than engaging in a risk-free, subsistence activity. In other words, households will choose to adopt less risky technologies to avoid permanent damage to their welfare. Their model also suggests that conditional on having access to credit markets, households will under-utilize modern inputs.[4]

The theoretical literature suggests that mechanisms that protect against downside shocks should encourage risk-averse farmers to demand more credit to adopt more profit-maximizing production technologies. Evidence on the effects of insurance on the adoption of improved technologies have produced conflicting results. Dercon and Christiaensen (2011) found that downside consumption risk leads to lower fertilizer usage in rural Ethiopia, while Gine and Yang (2009) found that farmers which were offered an insured loan to purchase high-yielding hybrid maize and groundnut seeds for planting in Malawi had lower take-up rates than farmers that were offered an uninsured loan.

Food aid is a form of insurance that protects against downside risk and in principle could encourage adoption among risk-averse farmers. There are no direct financial costs incurred by the farmer which in turn do not drive up the costs of an insured loan as in the case of Gine and Yang (2009) and its purpose is to protect against downside consumption shocks. While the theoretical literature suggests the potential for food aid to increase the adoption of improved techniques, there may exist disincentive effects from the availability of food aid that would discourage farmers from investing in more profitable technologies.[5] At the same time, labour requirements with the food-for-work program may crowd out labour input in other productive activities.

Below we empirically test the effects that food aid has on the adoption of fertilizer. The Ethiopian Government has demonstrated its commitment to agricultural development by introducing policies

[4]Boucher et al. (2008)'s model can be modified so that the risk to the lender is not determined by the effort allocated by the borrower but by the knowledge the farmers possess about the new technology. Extension services remove some of this risk so that lenders offer lower collateralized loans.

[5]Little (2008) fail to find evidence that households become dependent on food aid and Andersson et al.(2011) find that food aid actually encourages investment in tree planting.

that promote the use of modern inputs in order to intensify crop yields. The relatively large number of smallholder farms that have adopted chemical fertilizer gives us a large enough sample to investigate the adoption of a particular modern input. At the same time, examining the adoption of a single technology allows us to avoid the difficulty in controlling for differences across technologies in potential profitability and risk.[6]

3 Context and Data

To investigate the role of the Ethiopian Government's food aid safety net program on fertilizer usage, we exploit the Government's response to the 2002 drought. The 2002 drought decreased cereal production by over 25 percent and left over 12.3 million Ethiopians in need of food aid assistance. The government responded to the drought by expanding its food aid program which primarily consists of food-for-work and free distribution.[7] Using longitudinal data from the Ethiopian Rural Household Survey (ERHS), we are able to observe household behavior before and after the drought which will allow us to identify the effect of food aid on fertilizer adoption and intensification. In essence, we are able to identify the effect of the Ethiopian Government's response to the 2002 drought.[8]

The ERHS was conducted in 15 Peasant Associations across rural Ethiopia.[9] The survey was administered by the International Food Policy Research Institute (IFPRI) in collaboration with the department of economics at Addis Ababa University (AAU) and the Center for the Study of African Economies (CSAE) at Oxford University. The ERHS interviewed 1,477 households seven times between 1994 and 2009. We make use of the 1999 and the 2004 rounds of the ERHS. We restrict our analysis to 6 of the 15 Peasant Associations as these are the only Peasant Associations where a nontrivial share of households report using fertilizer and receiving food aid. This leaves us with a sample size of 456 households.

Our measure of aid comes from self-reported measures of aid received from the government or a non-Government Organization. The 2004 round of the ERHS asked specific questions about the 2002 drought, the impact the drought had on crop output, and how responsive the government was

[6] Holden et al. (2001); Pender and Gebremedhin (2008); Freeman and Omiti (2003); Adesina (1996); Waithaka et al. (2007); Alem et al. (2010); Doss (1999); Dercon and Christiaensen (2011) discuss the determinants for fertilizer adoption.

[7] Refer to Gilligan and Hoddinott (2007) for a more detailed description of the 2002 drought and farmers' perception concerning the severity of the drought and the efficacy of the food aid program.

[8] This is important, as we are not investigating changes in food aid receipt over time. The treatment we are exploiting is the one time response to the 2002 drought.

[9] The Peasant Associations are the lowest administrative unit in Ethiopia and consists of several villages. Throughout the paper we will refer to a village and a Peasant Association interchangeably.

to the drought. The treatment is a dummy variable equal to 1 if the household reports receiving aid between September 2002 and March 2004 and 0 otherwise.[10]

The expansion in the food aid program primarily resulted in covering more areas as opposed to covering more households within historically aid recipient villages. Table 1 reports the share of households receiving aid for the 15 Peasant Associations sampled in the ERHS by survey year. Between 1995 and 1999, no more than 7 of the 15 villages had received some form of assistance in the form of food aid in a given year. In 2004, 9 of the 15 villages had received some form of food aid. This is the same number of villages which received aid during the 1994 drought. While there exists variation in the share of households receiving food aid within a village over time, the table does not suggests that the expansion resulted in more coverage within villages.[11]

There may exist concern that aid recipient households are fundamentally different from non-recipient households. Prior research on food aid targeting in Ethiopia has shown that there exists substantial errors of inclusion and exclusion in household targeting (Sharp, 1997; Clay et al., 1999; Jayne et al., 2002; Broussard et al., 2012). Table 1 depicts the variation in the share of households covered within a village over time. Of the villages used in the analysis only 16 percent of households never received food aid between 1994 and 2004 and only 5 percent of households received food aid in every round their village received aid. Most households cycle on and off food aid (conditional on the village receiving aid) and between free distribution and food-for-work.[12]

The survey also asked detailed questions about inputs used for crop agriculture. Households were asked about the type and amount of fertilizer used during the previous main season. Households that report using chemical fertilizer make up our sample of fertilizer adopting households. Table 2 provides a summary of the share of households using fertilizer and the intensity of fertilizer usage for the 15 Peasant Associations by survey year. In 2004, 45 percent of the households surveyed in the ERHS used fertilizer, an increase from the 43 percent of households which used fertilizer in 1994. Many households switched in and out of using fertilizer over the 10 year period. Intensity rates in 2004 were approximately 94 kilograms of fertilizer per hectare, a decrease from its 1994 level. Most households in the survey reported that the main constraint to using modern inputs, including fertilizer, were due to costs, with few households reporting availability of modern inputs as a constraint (Dercon and Christiaensen, 2011). The table shows that between 1999 and 2004 for many of the villages used in the analysis the share of households using fertilizer and the quantity

[10] We use the same measure of aid receipts used by Gilligan and Hoddinott (2007).

[11] The existing literature does not suggest that within village targeting improved following the 2002 drought relative to other years. Broussard et al. (2012) use the first 6 rounds of the ERHS to investigate food aid targeting, they found no evidence that the (household) targeting strategy used in 2004 differed from the earlier rounds.

[12] The cycling between free distribution and food-for-work is primarily due to the fact that villages rarely received both programs in a given period.

of fertilizer used per hectare declined.

The six Peasant Associations used in the analysis are Haresaw, Dinki, Adele Keke, Korodegaga, Aze Deboa, and Gara Godo. Between 12 and 86 percent of the households in these villages reported using fertilizer in 2004 and between 36 and 94 percent of the households reported receiving food aid in response to the 2002 drought.

4 Identification Strategy and Econometric Model

To identify the effect of the Ethiopian Government's response to the drought on fertilizer adoption we adopt a methodology similar to Gilligan and Hoddinott (2007). We compare adoption behavior of aid recipient households to non-aid receiving households. The ERHS contains a rich set of variables used by village representatives to select aid recipient households and due to the errors of inclusion and exclusion of aid recipients reported by earlier studies (Sharp, 1997; Clay et al., 1999; Jayne et al., 2002; Broussard et al., 2012) we believe that households which did not receive aid are suitable controls for aid recipient households. By comparing fertilizer usage of treatment and control groups before and after the 2002 drought, we are able to capture the effect of the Ethiopian Government's food aid safety net program. The naive difference-in-differences estimator is estimated from the following regression:

$$\Delta Y_i = \beta_0 + \beta_1 D_i + \epsilon_i \tag{1}$$

where D is an indicator variable equal to 1 if the household received food aid and 0 otherwise.

Comparing simple differences between recipient and non-recipient households could lead to biased estimates of the true impact of food aid due to the fact that food aid was not randomly assigned and pretreatment characteristics determined selection into each food aid program.[13] To account for the non-randomness of food aid allocations we employ inverse-propensity score weighting (Hirano et al., 2003; DiNardo, 2002).

Let X be a vector of observable control variables which determine selection into the food aid program. Regressing ΔY on D and X would allow us to identify the effect of treatment on the outcome variable. This is the unconfoundedness assumption or the selection-on-observables as-

[13]The difference-in-differences estimator controls for selection bias due to unobservable time-invariant characteristics.

sumption, which states that treatment is independent of potential outcomes conditional on the observed covariates. This means that, conditional on covariates, treated and non-treated households would, on average, be expected to experience the same changes in outcomes following the drought in the absence of treatment.

Rosenbaum and Rubin (1983) show that conditioning on the propensity score, where the propensity score is $Pr(D = 1|x)$, also achieves identification. Instead, we employ inverse-propensity score weighting which Hirano et al. (2003) has shown to produce an efficient estimate of the average treatment effect. Inverse-propensity score weighting constructs two counterfactual means and takes their difference to obtain the average treatment effect (DiNardo, 2002). The treatment mean and the control mean for the population is obtained by a weighted mean of outcomes in the treated and control group, respectively. This approach reweights the data to balance the distribution of covariates across treated and untreated households.[14]

Denoting the estimated propensity score for person i as $\hat{p}_i$, the estimated inverse-propensity score weight for person i is:

$$\hat{w}_i = \frac{D_i}{\hat{p}_i} + \frac{1 - D_i}{1 - \hat{p}_i} \tag{2}$$

and the estimated average treatment effect is:

$$A\hat{T}E = \frac{1}{N_T} \sum_{i \in T} \hat{w}_i y_i - \frac{1}{N_C} \sum_{i \in C} \hat{w}_i y_i \tag{3}$$

where N_T is the number of treated observations and N_C is the number of control observations.

The decision to adopt fertilizer depends on a households ability to access credit. Livestock holdings are an important source of collateral in rural Ethiopia given the the many restrictions in the land market. We allow the impact of receiving aid to vary with the household's value of livestock holdings. The difference-in-difference reweighting estimator is obtained via the following regression:

$$\Delta Y_i = \beta_0 + \beta_1 D_i + \beta_2 x_i + \beta_3 D_i (x_i - \bar{x}) + \epsilon_i \tag{4}$$

where ΔY_i is the outcome of interest for individual i, D is the indicator for treatment, x_i is the household's value of livestock holdings in 1999, and $\bar{x}$ is the mean value of livestock holdings for the sample so that $(x_i - \bar{x})$ is the demeaned value of livestock holdings. We weight the regression

[14] This approach has been used recently in the economics literature to estimate the average treatment effects of economic development programs (Busso and Kline, 2008) and welfare reforms (Bitler et al., 2006), just to name a few. Refer to DiNardo (2002) and Hirano et al. (2003) for a discussion of the use of propensity score reweighting to estimate the average treatment effect.

by the inverse propensity score described above. β_1 identifies the average treatment effect, while $\beta_1 + \beta_3$ identifies how the treatment effect varies with different values of livestock holdings. If β_3 is significantly different from zero, there is evidence of heterogeneity of treatment effects by asset holdings. We estimate standard errors for the impact estimates by a bootstrap using 1000 replications of the sample.

Due to the different selection criteria and work requirements across the two forms of food aid programs, we estimate separate treatment effects for participation in FFW and in FD. The controls we include for the participation regressions include 1999 household characteristics, variables which capture the households social networks, political connections, and death and illness shocks.[15]

5 Results

We use a logit model in order to estimate the propensity scores for both the FFW and FD programs. We adopted many of the same set of control variables which are believed to be associated with the probability of participating in each food aid program as used by Gilligan and Hoddinott (2007).[16] The control variables selected are believed to be associated with the probability of participating in each food aid program.

For the FD program, the control variables used to estimate the propensity scores include changes in monthly log real consumption per adult equivalent between previous rounds of the ERHS survey; pre-drought (1999) land area owned and its square; pre-drought household demographics variables (number of male household members between the ages of 15 and 64, the number of female household members between the ages of 15 and 64, the number household members younger than 15 years of age, the number of household members older than 64 years of age, the household's dependency ratio, whether the household is headed by a female, and the log age of the household head); whether the household head's primary job was farming; whether the household head had any formal education; the household head's highest grade completed in school; whether the household reported experiencing a drought between 1999-2002; whether the household experienced a death or serious illness shock between 1999-2002; whether all household members were too weak, sick, young or old to work; measures of the household's political and social connections in the village (whether the parents of the household head were important in the village, whether a parent of

[15] Refer to Gilligan and Hoddinott (2007) for a more detailed description of the variables used.

[16] The differences in the variables used to estimate the propensity score are due to ensuring that the "balancing property" is met. Because we use only a subset of the villages used in Gilligan and Hoddinott (2007) we had to exclude some of the conditioning variables used in their paper in order to insure that the treatment sample and the sample of comparison observations had similar mean propensity scores and observables at various levels of the propensity scores.

the respondent holds a local official position (interacted with regional dummies), number of iddirs the household belonged to prior to the drought, and the number of people that would help the household in time of need); and an indicator for whether the household met any targeting criteria for FD in its village.

The control variables used to estimate the propensity scores for the FFW sample include many of the same variables used for the FD sample. Additional variables include whether the households social network has grown since five years prior to 1999, and an indicator variable for if the household head was born in the village. We also include an indicator variable for whether the household met any targeting criteria for FFW in its village. We exclude the variables for if the household head's primary job is farming and the household head's highest completed grade. The logits also include village fixed effects.

Table 3 provides means of the variables used to identify the selection into the two food aid programs. Column 1 provides the means for the full sample of households in the 6 villages used in the analysis (509 households). Because propensities near zero or one violate the condition required for reweighting that the probability of treatment be bounded away from zero and one, the remaining columns provide the means and difference in means for the samples used in the analysis in which the estimated propensities are never near zero or one. The FFW sample consists of 172 control households and 284 treated households. The FD sample consists of 178 control households and 264 treated households. Columns 2 and 3 provides the means for the treated and control samples for the FD sample. Column 4 provides the differences in means between the treated and control samples. FD recipient households tend to have more land, have fewer male household members, the household head is slightly more educated and they had more household members that were too sick to work relative to the non-FD recipient households.

Columns 6 and 7 provide the means for the treated and control samples for the FFW sample. Column 8 provides the differences in means between the treated and control samples for the FFW samples. FFW recipient households also tend to have more land, have a lower dependency ratio, the household head tends to be younger and slightly more educated, they had more household members that were too sick to work, their parents tend to be important in the village, they are more likely to have had a household member died, and they are less likely to have been born in the village relative to the non-FFW recipient households.

Columns 5 and 9 of table 3 reports estimated differences between the treated and control samples after adjusting using inverse propensity score weighting. The differences in means between the aid recipient households and non-aid recipient households are no longer statistically different.

Table 4 presents the results from the naive difference-in-difference analysis without covariate adjustment. The outcome variables are the change in fertilizer usage and the change in the quantity of fertilizer used in kilograms per hectare. The change in the outcome variables are between 1999 and 2004 (two years before and after the drought). Bootstrapped standard errors are presented in parentheses. Panel A presents the results for food-for-work and panel B presents the results for free distribution. The naive estimator shows that food aid recipient households had lower adoption rates and use less fertilizer per hectare than non-aid recipient households, however these results are not significantly different from zero.

Table 5 presents the average treatment effect from the reweighted difference-in-difference analysis. Row 1 provides the mean outcome for participants, row 2 provides the mean outcome for non-participants, and row 3 provides the average treatment effect. Bootstrapped standard errors are presented in parentheses. Columns 1 and 2 presents the results for fertilizer usage to see if food aid encouraged households to take on more risk. Reweighting the difference-in-difference estimator for covariate imbalance changes the sign of the point estimates for the food-for-work sample but does not change the sign or the magnitude of the point estimates for the free distribution sample.

For the FFW sample, the estimated mean effect on fertilizer intensity is positive but insignificant while the estimated mean effect on fertilizer adoption is statistically significant at the 10 percent level. Because fertilizer usage decreased between 1999 and 2004, the results show that FFW participant households decreased their fertilizer usage by 11.4 percentage points less than non-participant households. Column 3 of the table investigates if the positive effect on fertilizer usage translates into increases in the change in the real value of crop output per adult equivalent. Our results show a positive average effect of aid on the change in the value of crop output but the point estimate is not statistically significant from zero.

The differences between the naive and the reweighted difference-in-difference estimates demonstrate the selection bias associated with the targeting of the food aid programs. The differences suggests that food-for-work was targeted towards households that were less likely to adopt fertilizer and to those households that use less fertilizer per hectare in the absence of treatment. The similarities between the naive and reweighted difference-in-difference estimates for the free distribution sample support results from earlier papers which suggest insufficient targeting of free distribution (Clay et al., 1999; Jayne et al., 2002; Dercon and Krishnan, 2003; Broussard et al., 2012). The negative effects we find from the FD regressions suggests that the disincentive effects may exceed the positive effects. However, we do not obtain significant effects on any of the negative coefficients.

To investigate whether the increase in fertilizer adoption is attributable to the protection against downside consumption risk, column 4 and 5 present the average treatment effect on the change in

monthly log real total consumption per adult equivalent (column 4) and the change in monthly log real food consumption per adult equivalent (column 5). Although we find a positive effect of FFW on total consumption and food consumption, the results are not significantly different from zero. However, we do find that FD increased food consumption. This casts doubts on the hypothesis that households are taking on more risk due to the insurance that food aid provides against downside consumption shocks.

Finally, column 6 of table 5 investigates if food aid protects households from selling their assets, measured by the change in the real value of livestock in thousands of Ethiopian Birr. The results show that food aid had no effect on livestock holdings. The results for consumption and livestock in columns 4-6 allow us to compare our findings from inverse propensity score weighting with results from Gilligan and Hoddinott (2007) who use propensity score matching. For FFW, our point estimates for the average treatment effect on consumption and livestock are very similar to the point estimates obtained in table 3 of their paper, although our villages are a subset of villages used by them. In fact, Our findings are consistent with the findings from Gilligan and Hoddinott (2007) when they exclude Shumsha village, which is also excluded from our sample. For free distribution, our results are virtually identical in magnitude and significance to the results obtained in table 4 reported by the authors.

5.1 Heterogeneous Impacts of Food Aid Participation by Livestock Holdings

Households with more assets will have easier access to credit and therefore will have the ability to invest in income-improving technologies such as fertilizer. The results presented so far provided the average treatment effect of receiving food aid, however, the effect of receiving food aid on fertilizer adoption and intensification may vary with the household's holdings of assets that can be use for collateral. In Ethiopia, land can not be used as collateral so livestock plays an important role for many rural Ethiopians. We investigate if the effect of aid on fertilizer use varies with livestock holdings.

The final row of table 5 presents the coefficient on the interaction term of aid participation with demeaned livestock holdings in 1999. For FFW, the interaction term is positive and significant at the ten percent level for both fertilizer adoption and fertilizer intensification; FFW increases the likelihood of adopting fertilizer and increases the amount of fertilizer used per hectare the higher the household's livestock holdings. These findings suggests that the government's response to the 2002 drought helped asset-rich farm households more than asset-poor households. We still fail to find any evidence that the increases in fertilizer translate into increases in the value of crop output.

We do not find heterogenous effects of participation in free distribution on fertilizer usage or on the value of crop output.

5.2 Falsification Test

There may still be concerns that the significant differences reported in table 5 for FFW households are driven by unobservable characteristics. If unobservable characteristics determine selection into the food aid program and these characteristics are correlated with future fertilizer usage behavior, then the reported estimates will be biased. To test this concern we preform the following falsification test. We test the effect that the government's response to the 2002 drought had on fertilizer behavior in 1999, before food aid was administered (in response to the 2002 drought). If unobserved characteristics are driving our results, we would observe differences between the treated and control groups in years prior to the actual treatment.

Table 6 presents results from this falsification test. The dependent variable is the change in fertilizer usage between 1997 and 1999. The treatment is whether the household received food aid in 2002. The coefficients are negative and not significantly different from zero suggesting that our results are not driven by unobserved characteristics of the households.

6 Conclusion

The paper investigated the role safety nets, in the form of food aid, play in fertilizer adoption. Using a difference-in-difference estimator along with inverse propensity score weighting we were able to identify the average treatment effect of food aid on fertilizer usage. We find that households that participated in the food-for-work program following the 2002 drought were more likely to adopt fertilizer 18 months later. We also found that the likelihood of adopting fertilizer and the intensity of fertilizer usage increased with livestock holdings for food-for-work participant households. We found no significant effects of free distribution on fertilizer usage.

While we did find positive effects of food-for-work on consumption, we were unable to reject the hypothesis that the coefficients were equal to zero. This casts doubts on the hypothesis that households are taking on more risk due to the insurance that food aid provides against downside consumption shocks. However, our results are consistent with the hypothesis that safety nets can be viewed as a mechanism that allows households to take on more risk to pursue higher profits. The fact that food-for-work had statistically significant effects on fertilizer usage while free distribution did

not, suggests that the work requirement from the food-for-work program discourages disincentives to engage in productive activities.

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Table 1: Share of Households Receiving Aid by Survey Year

	Survey Year					
Village	1994a	1994b	1995	1997	1999	2004
Haresaw	0.64	0.00	0.50	0.15	0.65	0.64
Geblen	0.77	0.88	0.00	0.67	0.64	0.72
Dinki	0.00	0.83	0.00	0.00	0.27	0.67
Yetemen	0.00	0.00	0.00	0.00	0.00	0.00
Shumsha	0.96	0.90	0.60	0.56	0.55	0.70
Sirbana Godeti	0.00	0.00	0.00	0.00	0.00	0.00
Adele Keke	0.25	0.00	0.00	0.57	0.00	0.47
Korodegaga	0.17	1.00	0.17	0.00	0.00	0.94
Trirufe Ketchema	0.00	0.00	0.14	0.00	0.00	0.00
Imdibir	0.00	0.64	0.63	0.17	0.00	0.00
Aze Deboa	0.00	0.00	0.00	0.00	0.00	0.76
Adado	0.00	0.00	0.00	0.00	0.00	0.00
Gara Godo	0.00	0.87	0.11	0.00	0.00	0.36
Doma	0.00	0.97	0.73	0.00	0.48	0.22
D.B. -Milki	0.00	0.00	0.00	0.00	0.39	0.00
Total	0.20	0.39	0.18	0.14	0.21	0.36

Notes:

Table 2: Fertilizer Usage By Village By Round

	Share of Households Using Fertilizer						Application Rate Per Hectare (KG)					
Village	1994a	1994b	1995	1997	1999	2004	1994a	1994b	1995	1997	1999	2004
Haresaw	0.00	0.07	0.01	0.15	0.30	0.12	0.00	47.50	25.00	36.83	28.87	29.50
Geblen	0.03	0.03	0.02	0.19	0.08	0.02	37.50	27.50	5.00	23.33	35.90	14.00
Dinki	0.02	0.07	0.04	0.14	0.25	0.18	10.13	30.83	33.42	32.79	46.21	39.21
Yetemen	0.69	0.74	0.73	0.61	0.73	0.66	160.43	180.64	190.74	186.88	157.45	138.46
Shumsha	0.00	0.01	0.00	0.09	0.13	0.10	0.00	1.00	0.00	44.00	35.56	9.50
Sirbana Godeti	0.77	0.81	0.85	0.82	0.84	0.79	241.80	294.68	271.28	250.48	100.56	203.79
Adele Keke	0.37	0.49	0.08	0.43	0.46	0.44	101.42	96.36	57.50	86.63	34.72	67.25
Korodegaga	0.28	0.46	0.63	0.84	0.61	0.52	53.00	49.96	58.16	82.67	53.21	51.00
Trirufe Ketchema	0.77	0.86	0.81	0.91	0.84	0.89	90.62	107.29	93.16	151.87	91.61	79.99
Imdibir	0.00	0.00	0.00	0.02	0.00	0.05	0.00	0.00	0.00	4.00	0.00	25.00
Aze Deboa	0.91	0.95	0.83	0.78	0.87	0.57	39.81	47.00	32.53	39.10	32.77	52.71
Adado	0.00	0.00	0.00	0.02	0.02	0.04	0.00	0.00	0.00	37.00	37.50	18.67
Gara Godo	0.75	0.92	0.63	0.96	0.89	0.86	38.19	33.93	22.59	58.70	74.59	44.41
Doma	0.00	0.08	0.06	0.10	0.38	0.00	0.00	12.00	21.25	90.00	43.84	0.00
D.B. -Milki	0.77	0.75	0.60	0.79	0.74	0.86	130.85	122.68	115.88	95.89	91.71	122.82
Total	0.37	0.43	0.36	0.48	0.49	0.45	112.02	114.50	111.81	109.08	74.61	93.84

Notes: Application rates per hectare are for households which report using fertilizer.

Table 3: Characteristics of Sampled Households

		Free Distribution Sample				Food-For-Work Sample			
	Full Sample	Levels		Differences		Levels		Differences	
Selection Variables	(ERHS)	Treated	Control	Unadjusted	Adjusted	Treated	Control	Unadjusted	Adjusted
Difference in ln real consumption, 1997-1999	-0.120	-0.099	-0.127	0.028	-0.027	-0.073	-0.203	0.130	0.025
Difference in ln real consumption, 1995-1997	0.393	0.392	0.344	0.048	0.017	0.416	0.357	0.059	-0.027
Difference in ln real consumption, 1994-1995	-0.196	-0.184	-0.175	-0.010	-0.025	-0.211	-0.184	-0.027	0.042
Land area owned (hectares)	1.269	1.417	1.089	0.328***	-0.053	1.562	0.855	0.707***	0.121
Land area owned squared	3.115	3.764	2.316	1.448	-0.287	4.513	1.089	3.424**	0.811
Number of Adult Men	1.503	1.496	1.522	-0.026**	0.046	1.609	1.390	0.220	0.024
Number of Children	2.717	2.591	3.051	-0.460	-0.064	2.810	2.663	0.147	0.024
Number of Elderly Adults	0.230	0.197	0.225	-0.028	0.010	0.222	0.233	-0.011	-0.005
Number of Adult Female	1.597	1.614	1.607	0.007	-0.080	1.683	1.535	0.148	0.017
Dependency ratio	1.243	1.179	1.309	-0.130	-0.032	1.198	1.262	-0.064*	-0.013
Ln of household head age	3.824	3.823	3.806	0.017	-0.008	3.806	3.857	-0.051**	-0.023
Household head has any formal education	0.189	0.212	0.180	0.032**	0.001	0.225	0.140	0.086***	0.024
Household head is female	0.287	0.269	0.303	-0.034	0.005	0.254	0.302	-0.049	0.027
Households experienced drought	0.833	0.845	0.820	0.024	-0.004	0.852	0.797	0.056	0.000
Male household member had serious illness	0.092	0.080	0.096	-0.016	0.003	0.088	0.105	-0.017	-0.011
Female household member had serious illness	0.086	0.091	0.090	0.001	0.007	0.088	0.105	-0.017	0.002
Household members weak/sick/young/old	0.061	0.080	0.039	0.040*	0.007	0.025	0.087	-0.063***	0.009
Parent important in PA social life	0.676	0.678	0.685	-0.007	0.007	0.729	0.634	0.095**	0.003
Number of iddir household belonged to	0.874	0.871	0.798	0.073	-0.054	0.915	0.837	0.078	0.003
Number of people that will help in time of need	7.189	6.977	7.152	-0.174	0.097	7.701	6.826	0.875	-0.143
Network size has grown in last 5 years						0.285	0.349	-0.064	0.017
If household head primary job is farmer	0.768	0.792	0.764	0.028	0.003				
Household member died	0.222	0.227	0.208	0.019	-0.011	0.500	0.413	0.087*	0.002
Household head born in this PA						0.697	0.802	-0.105***	-0.015
Household heads highest completed grade	1.048	1.087	0.933	0.155	0.035				
Household met at least one targeting criterion	0.448	0.405	0.455	-0.050	0.003	0.859	0.750	0.109***	0.012
Parent holds official position in Kebele									
Tigray region	0.018	0.011	0.022	-0.011	-0.001				
Amhara region	0.016	0.015	0.022	-0.007	0.001				
Oromia region	0.055	0.061	0.039	0.021	0.006				
SNNPR region	0.069	0.080	0.039	0.040*	-0.013				

Notes:

Table 4: Naive Estimates of the Impact of Food Aid

Panel A: Food For Work		
	Outcome Variables	
	Adoption	Intensity
Difference in average outcomes, ATE	-0.011	-0.154
	(0.052)	(0.211)
Panel B: Free Distribution		
	Outcome Variables	
	Adoption	Intensity
Difference in average outcomes, ATE	-0.043	-0.240
	(0.053)	(0.225)

Significance levels : $*$: 10% $**$: 5% $***$: 1%

Notes: The FFW sample consists of 172 control households and 284 treated households. The FD sample consists of 178 control households and 264 treated households. Bootstrapped standard errors in parentheses using 1000 replications of the sample. Fertilizer adoption is an indicator for if the household used fertilizer in 2004 relative to 1999, where -1 = stop using fertilizer, 0 = no change in usage, 1 = started using fertilizer. Fertilizer intensification is the change in the quantity of fertilizer used in kilograms per hectare, 1999-2004. The crop income variable is the change in the real value of crop output per adult equivalent, 1999-2004.

Table 5: Estimates of the Impact of Food Aid

Panel A: Food For Work						
	Outcome Variables					
	Fertilizer			Consumption		
	Adoption	Intensity	Crop Income	Total	Food	Livestock
Average outcome, FFW participants	-0.123	-0.548	0.469	0.179	0.339	0.739
Average outcome, non-participants	-0.243	-0.774	0.359	0.062	0.209	0.741
Difference in average outcomes, ATE	0.114*	0.208	0.107	0.117	0.129	-0.002
	(0.069)	(0.233)	(0.200)	(0.129)	(0.157)	(0.197)
Impact by Livestock Holdings, 1999:						
Interaction Term	0.110*	0.297*	-0.057			
	(0.0651)	(0.177)	(0.165)			
Panel B: Free Distribution						
	Outcome Variables					
	Fertilizer			Consumption		
	Adoption	Intensity	Crop Income	Total	Food	Livestock
Average outcome, FD participants	-0.129	-0.584	0.612	0.161	0.363	0.744
Average outcome, non-participants	-0.076	-0.369	0.437	0.033	0.095	0.558
Difference in average outcomes, ATE	-0.056	-0.216	0.177	0.128	0.268**	0.186
	(0.060)	(0.223)	(0.199)	(0.101)	(0.120)	(0.229)
Impact by Livestock Holdings, 1999:						
Interaction Term	0.016	-0.041	-0.114			
	(0.056)	(0.189)	(0.104)			

Significance levels : $*$: 10% $**$: 5% $***$: 1%

Notes: The FFW sample consists of 172 control households and 284 treated households. The FD sample consists of 178 control households and 264 treated households. Bootstrapped standard errors in parentheses using 1000 replications of the sample. Fertilizer adoption is an indicator for if the household used fertilizer in 2004 relative to 1999, where -1 = stop using fertilizer, 0 = no change in usage, 1 = started using fertilizer. Fertilizer intensification is the change in the quantity of fertilizer used in kilograms per hectare, 1999-2004. The crop income variable is the change in the real value of crop output per adult equivalent, 1999-2004. Outcome variables for consumption are change in monthly log real total (food) consumption per adult equivalent, 1999-2004. The livestock variable is the change in the real value of livestock in thousands of Ethiopian Birr, 1999-2004.

Table 6: Falsification Test

Panel A: Food For Work			
	Outcome Variables		
	Fertilizer		
	Adoption	Intensity	Crop Income
Difference in average outcomes, ATE	-0.074	-0.230	-0.193
	(0.048)	(0.212)	(0.169)
Panel B: Free Distribution			
	Outcome Variables		
	Fertilizer		
	Adoption	Intensity	Crop Income
Difference in average outcomes, ATE	0.055	0.286	0.026
	(0.053)	(0.208)	(0.183)

Notes: The FFW sample consists of 172 control households and 284 treated households. The FD sample consists of 178 control households and 264 treated households. Bootstrapped standard errors in parentheses using 1000 replications of the sample. Fertilizer adoption is an indicator for if the household used fertilizer in 1999 relative to 1997, where -1 = stop using fertilizer, 0 = no change in usage, 1 = started using fertilizer. Fertilizer intensification is the change in the quantity of fertilizer used in kilograms per hectare, 1997-1999. The crop income variable is the change in the real value of crop output per adult equivalent, 1997-1999.

Leveling with Friends: Social Networks and Indian Farmers' Demand for Agricultural Custom Hire Services*

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ABSTRACT

Technology-driven gains in productivity and profitability can dramatically improve the quality of life for the rural poor in developing countries. Extension services responsible for the dissemination of agricultural technologies typically rely on the assumption that farmers learn from early adopters in their social networks. In this paper we investigate network effects on farmers' exposure to, and demand for, an agricultural technology—laser land leveling—in eastern Uttar Pradesh, India. Research on network effects is made notoriously difficult by the reflection problem: it is not usually possible to determine if farmers adopt technologies because others in their networks use them, or because they share characteristics with adopters in their networks and thus make similar decisions. To circumvent this problem we randomly select farmers from a pool of would-be adopters, as determined by an experimental auction, to actually adopt the technology. We employ a second auction one year later to elicit willingness to pay from all sample farmers. We find that farmers' exposure to laser land leveling is positively affected by having a first generation adopter in their social network, and that farmers with an adopting farmer in their network are willing to pay nearly 25 percent more for the technology than are comparable farmers without an adopting farmer in their network.

JEL Codes: O13, O14, Q16

Keywords: Social Learning, Network Effects, Technology Adoption, India

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INTRODUCTION

Technological innovation in agriculture can make agriculture more productive and profitable to the rural poor in developing countries, improving their day-to-day quality of life and food security. One particular class of innovations—resource-conserving technologies—are designed not only to increase productivity and reduce production costs, but also to alleviate negative environmental externalities and use water and soil resources more sustainably. Understanding how farmers obtain information about new agricultural technologies, and understanding how that information spreads, is therefore crucial to many development objectives. Farmers have multiple sources of agricultural information at their disposal, some of which are more valued than others. Farmers often rely on their social networks as their most trusted and reliable source of information regarding the suitability, profitability, and use of new technologies. This is a key principle underlying agricultural extension strategies: where farmers are geographically or socially dispersed, and where public resources for technology promotion are scarce, farmers' social networks can be used to widely disseminate new technologies. Such strategies typically depend on reaching out to "progressive" or "model" farmers to demonstrate the technology and incite adoption, in hope that other farmers will follow (Anderson and Feder, 2004). In some instances, this dissemination process can be accelerated through direct interventions such as subsidies or coupons for early adopters because the information externality generated by these adopters tends to increase adoption in subsequent periods, even if the technology is no longer subsidized (Kremer and Miguel, 2007). Other strategies may use social mobilization—bringing farmers together in cooperatives, self-help groups, or community organizations—to similarly leverage these network effects. Empirical evidence of farmer-to-farmer technology spillovers, however, is lacking.

One reason for the lack of empirical studies of network effects is that they are difficult to identify because of the reflection problem (Manski, 1993). The reflection problem occurs because under most circumstances it is not possible to determine if two farmers use similar technologies because one learns from or mimics the other or because the farmers are merely similar or face similar conditions and constraints. Many observational studies on social networks have implemented creative and highly convincing strategies to identify network effects, often taking advantage of panel data (Bandiera and Rasul, 2006, Conley and Udry, 2010, Foster and Rosenzweig, 1995, Maertens, 2012, McNiven and Gilligan, 2012, Munshi, 2004, Munshi and

Myaux, 2006). Recently, a handful of empirical studies on network effects— including, but not limited to, agriculture—have used randomized interventions, or field experiments, to identify network effects (Babcock and Hartman, 2010, Cai, 2011, Duflo, et al., 2006, Duflo and Saez, 2003, Kremer and Miguel, 2007, Oster and Thornton, 2009). The benefit of using such interventions to identify network effects has been highlighted by experimental studies with results contrary to the findings of most observational studies, and even contrary to results obtained using the same data as if it were observational (Duflo, et al., 2007, Duflo, et al., 2006, Kremer and Miguel, 2007).

In this paper we present findings on network effects on agricultural technology dissemination from a field experiment that randomly assigns custom hire of a new agricultural technology to farmers in three districts of eastern Uttar Pradesh (EUP), India. The technology in question is laser land leveling (LLL), a resource conserving technology, which we describe below. Because LLL equipment is expensive and requires some skill to operate, most Indian farmers – and all smallholders – are likely to access LLL though rental arrangements known as custom hire services. This study uses a pair experimental auctions held one year apart to measure farmer demand for LLL and to infer learning. These auctions were binding: if a farmer bid enough for LLL services on their land they could expect to pay real money out of pocket and receive real LLL custom hire services. After the first auction, we held a lottery to determine who would actually purchase services. Using this randomization, we are able to test for the effect of having an adopting farmer in a farmer's social network on exposure to and demand for the technology, conditional on the number of would-be adopters in his[1] network. We find that farmers with early adopters in their network are more likely to be exposed to LLL and ultimately exhibit substantially higher WTP for LLL in the second auction.

Laser land leveling in India

In flood-irrigated rice-wheat systems of the Indo-Gangetic Plains, 10-25 percent of irrigation water is lost because of poor management and uneven fields. Uneven fields can also lead to inefficient use of fertilizers and chemicals, increased biotic and abiotic stress, and low yields (Jat, et al., 2006). Laser land leveling is a process of precisely smoothing the land surface using a

[1] We use masculine pronouns throughout for ease of composition. In our sample, 84 percent of study farmers were

laser-guided drag scraper attached to a tractor that removes earth from peaks in the field and places it in troughs. Leveling with laser guidance can reduce undulations to a height of 1 to 2 cm compared to traditional leveling methods that achieve reductions to only 4 to 5 cm (Jat, et al., 2006). This degree of added leveling precision is available to plots as small as 0.2 acre (below which maneuvering the leveling equipment effectively become difficult).

The immediate benefit of LLL is a reduction in water use. This is particularly important in the Indo-Gangetic Plains, where groundwater is being extracted at increasingly unsustainable rates, and where farmers still rely on flood irrigation, which requires them to irrigate until the highest point of the field is submerged. Although Indian farmers do not pay unit charges for the water they use, most farmers must pump irrigation water and therefore incur savings in the form of diesel fuel costs from such a reduction in water usage. LLL has also been shown to improve crop establishment and growth, thereby improving the efficiency of chemical and fertilizer use while decreasing the damage caused by biotic and abiotic stress, ultimately leading to production cost reductions and increases in output and yields (Jat, et al., 2006).

In India, LLL was initially introduced in western Uttar Pradesh in 2001. Since then, the technology has achieved widespread acceptance in some areas of the Indo-Gangetic Plains (IGP)—notably in the agriculturally progressive Indian states of Haryana and Punjab. Since 2001, the number of laser land levelers has risen to 925 and the acreage under LLL has grown to 200,000 hectares in 2008. Agronomic trials in rice-wheat systems in this region have found that LLL results in 10-30 percent irrigation savings, 3-6 percent increases in effective farming area, 6-7 percent increases in nitrogen use efficiency, and 3-19 percent increases in yield (Jat, et al., 2006, Jat, et al., 2009). In on-farm trials, net annual farmer revenues rose from $200-300 per hectare (Jat, et al., 2009). LLL could also have public benefits in the form of reduced groundwater depletion and lower nutrient and chemical runoff. Jat et al. (2006) estimate that extended use of LLL to 2 million hectares of rice-wheat land in the IGP could save 1.5 million hectare-meters of irrigation water and 200 million liters of diesel, increase crop production by $500 million, and reduce greenhouse gas emissions by 0.5 million metric tons over three years.

In contrast to these more agriculturally developed regions of India, LLL is new to the more heterogeneous and poorer region that is the focus of this study. Farmers in this region have smaller plots, and their production practices are less input intensive. Private LLL service providers have yet to extend their services networks to this quite different region, in part because

the business models they have developed in the western IGP may not be viable in the EUP. In a companion paper, we use the auction data we describe below to simulate novel business models to deliver LLL in the heterogeneous EUP (Lybbert et al. 2012). In this paper, we exploit the lack of familiarity with LLL in the region to study how farmers learn about this new technology. While LLL has been introduced very sparsely into EUP via small-scale demonstrations, our sampling design ensures that the farmers in our sample have little or no exposure to these demonstrations.

STUDY SETTING AND DATA COLLECTION

Uttar Pradesh state (UP) covers 243,000 km^2 and is home to 200 million residents, a remarkable population density even by Indian standards. The population of UP is highly agrarian and relatively poor; 70 percent of the population lives in poverty according to a recent Multidimensional Poverty Index (Alkire and Santos, 2010), and EUP is relatively poor compared to the rest of the state.

The main crops grown in the area are rice and wheat, followed by mustard, sugarcane, pulses, maize, and other crops. Farmers cultivate rice during the summer *kharif* season when the monsoon provides much of the water needed for irrigation.[2] Farmers cultivate wheat in the winter *rabi* season when the crop depends more on irrigation. Unlike areas in the western IGP where canals are a significant source of irrigation water, EUP depends primarily on groundwater that is extracted primarily by diesel, rather than electric, pumps. Because LLL is completely new to EUP and there is no market or price information for the technology, this is a promising study area in which to gauge demand using an experimental auction. EUP is also an ideal location to test network effects on learning because information on the technology is essentially non-existent outside of the intervention.

For this study we selected three districts—Maharajganj, Gorakhpur, and Deoria—to represent heterogeneity across farm size and productivity in rice and wheat cropping systems. In each district, we randomly selected four villages from among those with a population greater than 48 households (twice the desired sample size per village to allow for the presence non-farming households, or households without a plot large enough to laser level) and less than 400 households (to avoid lengthy and incomplete village censuses that would have made household

[2] During the *kharif* season most irrigation water is used for flooding the rice fields.

selection difficult). For each district, a population of 400 households per village is greater than the 90th percentile of all villages. We ensured that each sample village was not in the proximity of any of the few LLL demonstrations being conducted in EUP. Following consultations with individuals involved in agricultural research, extension services, and farm equipment sales and custom hiring, we were able to pinpoint locations where LLL demonstrations and related demonstrations of resource-conserving technologies in EUP had been held.[3] Villages within a ten-kilometer radius of any LLL demonstrations were excluded from the sample, as were any villages where related promotions of resource-conserving technologies had been conducted. In the final sample only six farmers reported ever hearing of LLL, two farmers reported ever seeing LLL machinery, and one farmer reported ever using LLL, or knowing the market price of LLL hire.[4]

For each of these twelve villages, we randomly chose a paired village that met the same population criteria, was within a five-kilometer radius, and was not within a 10-kilometer proximity to any previously selected village pair. Villages were selected in pairs to assess the spatial reach of social networks both within villages and across villages.[5] Within each village, we randomly selected approximately 20 farmers from those cultivating plots of at least 0.2 acres (the minimum sized plot for LLL) to be included in the study.[6] The resulting sample totaled 478 farmers.

In each village the study unfolded as follows. First, the enumeration team conducted a scripted information session to introduce the sampled farmers to LLL. This session lasted approximately one hour, and included a talk by a lead member of the enumeration team; a video showing a laser land leveler operating on a field, an interview with the service provider, and an interview with the farmer receiving the service; and a question-and-answer session with a progressive farmer from EUP who received LLL services as part of a demonstration.[7] At the

[3] Only three sources of LLL demonstrations were identified in EUP: sites selected by the Cereal Systems Initiative for South Asia (CSISA), of which this study is a part; the Krishi Vigyan Kendra (KVK) center in Kushinagar, a unit of the Indian Council for Agricultural Research that is responsible for technology promotion among farmers; and one private service provider who borrowed a CSISA LLL unit, provided custom hire services, and worked in partnership with the project.

[4] We believe that the single instance of a farmer reporting to have used LLL is an instance of misreporting or enumerator error.

[5] In preliminary analysis we find very few farmers discuss agriculture with farmers in the paired village.

[6] The intended sample size for each villages was 24, with an additional 12 replacement farmers pre-selected in case of absenteeism or lack of a big enough plot among the original 24 farmers.

[7] The lead enumerator was one of three, and the progressive farmer was the same for all information sessions.

conclusion of the information sessions, the team gave pictorial brochures about LLL to the farmers that contained the range of possible bids they could make in the experimental auction. At the information session, the team also photographed all sample farmers and compiled a farmer photo directory for each village to be used later to help identify network links.

Naturally, farmers at each information session inquired about the per hour cost of LLL services. Because the information session was designed as precursor to an experimental auction (explained in further detail below), the enumeration team answered questions in a consistent manner and in a way designed to prevent participants from anchoring on a specific price when it came time for auction bidding. Specifically, the enumeration team explained that in recent years in different states where LLL services were being provided, the price had ranged from Rs. 400 to Rs. 800.[8]

Next, the team conducted baseline surveys with sample farmers to collect information on farm and household characteristics. The baseline survey included a social networks module that used the photo directories to help farmers identify their network contacts. For the social networks survey, enumerators asked farmers about their connections with all study farmers in their village. Each farmer was shown a composite picture containing photos of the other sampled farmers in their village. They were asked to identify themselves in the picture and then answer a series of yes or no questions about their relationships with the other farmers in the picture, e.g.: are any of these farmers their friends? Are any in their family? With which of these farmers do they discuss agriculture? Farmers were also asked to identify the progressive farmers in the photo. The same exercise was then conducted using a composite picture of photos of sample farmers in the paired village.

With our social networks elicitation module in mind, it is useful to provide a broader description of relevant methodological dimensions to social network analysis. Prior social network studies have used a variety of definitions of social networks. In some cases, farmers' social networks have been defined as the entire village (Besley and Case, 1994, Foster and Rosenzweig, 1995, Munshi, 2004). While using the village as the relevant social network certainly captures many if not all of a farmer's contacts, it also captures many that are not in the farmer's network (Babcock and Hartman, 2010, Maertens and Barrett, 2012). Although farmers in these village settings may know everyone else in their village, the degree to which they share

[8] We see no evidence of anchoring to 400 in the auction results (Figure 1).

agricultural information, or even know what techniques other farmers use, is questionable.[9] In some cases it is possible to use observable variables from existing survey data, such as caste, gender, age, wealth, literacy, or religion to refine what farmers' social networks are likely to be (Munshi and Myaux, 2006).

Many recent network studies have been able to elicit network links directly. In some cases survey respondents are asked about their social networks in an open-ended manner, i.e., allowing the respondent to list any farmers they know, trust, communicate with, or exchange information with (Bandiera and Rasul, 2006, Cai, 2011, Duflo, et al., 2006, Kremer and Miguel, 2007). The advantage of this approach is that it helps define the social network in a more complete manner by allowing farmers to list contacts who might be outside the sample. A disadvantage is that the analyst may not have information about the farmers' network contacts, requiring them to either expand the sample (Duflo, Kremer, and Robinson 2006) or gather information about network contacts from the original sample farmer (Bandiera and Rasul 2006), which may be prone to error.

There are many ways in which a network connection can be defined. A connection can be unidirectional (A claims B as a friend) or bi-directional (A claims B as a friend and B claims A as a friend). Connections can be defined as one-dimensional and dichotomous (A and B are friends or they are not) or multi-dimensional and continuous (a social distance measure is composed from different measures of social connectivity, for example, level of trust, duration, and proximity). One-dimensional measures used in the literature include friend or family (Bandiera and Rasul, 2006, Kremer and Miguel, 2007), information contact or information neighbor (Cai, 2011, Conley and Udry, 2010, Duflo, et al., 2006, McNiven and Gilligan, 2012), and geographic neighbor (Duflo, et al., 2006). Because our study centers on the adoption of an agricultural technology we use agricultural contacts to define social networks.

Several days after the information session and baseline survey, the enumeration team gathered all of the sample farmers in a given village to conduct an experimental auction to elicit their demand for LLL. We used a modified (discretized) Becker-deGroote-Marchak style auction (Becker, et al., 1964), in which farmers were asked, in secrecy and plot by plot, a series of yes/no questions of the type, "would you pay Rs. X per hour to have this plot laser-leveled?" for

[9] Conley and Udry (2010) find that Ghanaian farmers counted 29 percent of their village as agricultural contacts. Bandiera and Rasul (2006) find that farmers in Northern Mozambique count less than 5% of sunflower adopters in their village as friends or family.

increasing values of X. Possible values were Rs. 0, 250, 300, 350, 400, 450, 500, 550, 600, 700, and 800 per hour. When a farmer said he would not pay Rs. X, the facilitating enumerator would move to the next plot. The maximum value at which the farmer agreed he would pay for LLL services is the maximum WTP for that plot, and the maximum WTP for all of a farmer's plots is considered his overall maximum WTP for LLL custom hire services.

Just before the final price was drawn, the lead enumerator informed all participants that because of capacity constraints, we would likely not be able to provide LLL services to all auction winners. Consequently, we would use a random and public lottery immediately following the auction to determine who would actually pay for and receive LLL custom hire services. To ensure that the majority of farmers would enter the lottery, in each village Rs. 250 was drawn as the purchase price.[10] Around two-thirds of all farmers won the auction and entered the lottery. We stratified farmers with WTP≥Rs.250 by their maximum WTP before randomly selecting half of the farmers in each strata in order to ensure demand variation among those actually receiving LLL services. This auction-lottery mechanism divided the sample into three groups of roughly equal size: auction losers, auction winners/lottery losers, and lottery winners from across different demand strata.

The lottery winners were required to pay for and receive LLL services at the draw price at a mutually agreed upon date during the months that immediately followed the auction and the lottery. The timing of the auction and lottery were such that the LLL custom hire services would be provided to lottery winners during the 100-day fallow season between *rabi* and *kharif,* which is effectively the only time farmers have to receive such services. Service provision during this time was carefully monitored to ensure that farmers had no other access to LLL services, e.g., through side-selling by the service provider or by other projects operating in EUP.

The lottery winners who received LLL custom hire services became our "first-generation adopters" in so far as they adopted the technology following the first auction and lottery. After the first generation adopters received LLL custom hire services the enumeration team conducted regular surveys throughout the *kharif* (summer) rice season and the *rabi* (winter) wheat season. In addition to gathering data on input use, including labor, we asked farmers about their exposure

[10] Although the price was pre-selected by the enumeration team to be Rs.250, this price was unknown and effectively random to participants. In one village Rs. 300 was selected and in another village Rs. 350 was selected, before it became clear a lower price was needed to bring enough farmers into the lottery. Subsequently Rs. 250 was selected in all other village. This difference should not change auction results in either year.

to LLL through other sample farmers using the photo directory: With whom have you discussed agriculture with since the auction? With who have you discussed LLL specifically? Whose fields did you see the LLL equipment operate on? Whose fields have you visited?[11]

In spring 2012 we collected demand data using a second auction identical in structure to the first, but without a lottery so all farmers who bid high enough would receive LLL custom hire services. For the purposes of this study, using WTP data from an experimental auction as opposed to binary adoption data has several advantages. First, it allows us to measure network effects on demand in the money metric. Second, it allows us to capture changes in demand that do not push a farmer across an adoption threshold, i.e., changes in demand for farmers who would not adopt (at some price) before *or* after one year of exposure, and for farmers who would adopt before *and* after one year of exposure (at some price). We provide examples of these advantages with our results.

RESULTS

The auction/lottery mechanism resulted in the following trifurcation of participants: auction losers, auction winners/lottery losers, and lottery winners. Because of self-selection, we expect auction losers (non-adopters) to systematically differ from auction winners (would-be adopters), and this is indeed the case. Auction winners have 20 percent more years of schooling, 60 percent larger landholdings, and are generally wealthier (as measured by a factor analytic wealth index). Because auction winners are split into lottery winners and losers at random, there is no systematic difference between the two groups that would influence both technology adoption and outcomes (table 1).

In our analysis of social network effects, we assume a given farmer is 'treated' with an LLL social network treatment if he has at least one adopter (lottery winner) in his social network. The probability of having a lottery winner in the farmer's network is dependent on the number of would-be adopters in his network (auction winners), which could be correlated to unobservable characteristics of the farmer himself that also influence adoption. While this implies that we face a version of the reflection problem, we have a means of controlling for this problem by including

[11] The exposure data used for this version of the paper are from near the end of the *kharif* rice season, before the beginning of the wheat season.

the number of would-be adopters in the farmer's network, which we observe in this study by design. The econometric model is therefore:

$$y_i = \alpha + \beta_1 \cdot LW_i + \beta_2 \cdot AW_i + \beta_3 \cdot TN_i + X_i{}'\beta_X + \varepsilon_i. \quad (1)$$

The dependent variable y_i is exposure to, or demand for, LLL. LW_i is an indicator variable for farmer i having and first generation adopter in his network, AW_i is the number of would-be adopters in i's network, and TN_i is the total number of farmers in i's network, which can be added to improve precision. X_i is a vector of farmer characteristics. The coefficient β_1 is the network effect.

While LW_i can technically be either continuous (e.g., intensity of adoption, number of adopters) or binary (e.g., presence of an adopter), we treat it as binary because on average farmers in our sample have 0.24 first generation adopters in their network of agricultural contacts and only 4 percent of farmers have more than one first generation adopter in their network. We do this mainly to facilitate interpretation, but also because of the strong likelihood of decreasing marginal effects of additional in-network adopters. While the existence of decreasing marginal effects is ultimately an empirical question, it is one we cannot answer with our data; the continuous variable for number of adopting network contacts and the dichotomous variable for having at least one are 92 percent correlated, so we are unable to use both as explanatory variables as others have (see Bandiera and Rasul, 2006 McNiven and Gilligan, 2012).

Exposure to LLL

We estimate equation (1) using the following binary exposure outcomes: if a farmer has seen the LLL operate, if a farmer has discussed LLL with an adopter, and if a farmer has visited the fields of an adopting farmer. In certain specifications we control for other variables that might affect exposure to LLL: education, age gender, caste, if the farmer is progressive,[12] and total landholdings. We use a linear probability model so coefficients are easily interpretable.[13]

First, we examine if and how farmers gain exposure to LLL through their network contacts. Our results in table 2 indicate that a farmer with an adopter in his social network is not more likely to see a leveler operate than a farmer without an adopter in his network. This is

[12] During the social networks survey all farmers were asked to identify progressive farmers on the village photo directory. If a farmer was selected as progressive by at least one other village farmer, he is considered progressive. The notion of a progressive farmer as conveyed by enumerators was easily understood by the farmers.
[13] The results are robust to logit and probit specifications.

reasonable; the arrival of the leveler and its operation in the village was a very public event and two-thirds of all sample farmers saw the leveler operate. However, a farmer with an adopter in his social network was approximately 20 percent more likely to have a conversation about LLL with an LLL adopter and also approximately 20 percent more likely to visit the field of an adopting farmer. Considering the mean percent probability of exposure to LLL via conversation was 50 percent and the mean percent probability of exposure through visiting a laser-leveled field is 33 percent, network effects on exposure are substantial.

Demand for LLL

Next we turn to the more important question of whether these network effects influence demand for the technology. Auction results show that mean WTP increased from the first auction to the second. This was expected, as many farmers initially said they would only adopt LLL once they saw it with their own eyes. Mean WTP for LLL in the baseline (2011) auction was Rs. 204 per hour and, among those with WTP>0, Rs. 322 per hour. In the follow-up (2012) auction mean WTP was Rs. 310 and Rs. 382 per hour, respectively. These differences in means are both significant at the 0.01 confidence level using a t-test. Figure 1 presents histograms of bids across the two auctions. It is worth noting that in 2012 there was no clustering around Rs. 250, the price drawn in the 2011 auction. This suggests that farmers were consistently bidding their individual WTP rather than anchoring around some price expectation based on the prior year's draw price. Importantly, this also suggests that the auction was well understood by the participants.

The dependent variable of interest in our main specification of equation (2) is WTP in the second auction, after the farmers had one year of exposure to the technology through others'—and in some cases their own—use. Again we treat having at least one adopting farmer in one's social network as a dichotomous variable. We test the model first using no control variables, and then add control variables to increase precision. We also offer results using as an alternative measure of demand the difference in WTP between the two years. Because laser land leveling lasts for several years, the service has characteristics of a durable good, namely that a farmer who just had a plot leveled is unlikely to have it leveled the following year, even at a low price.

Therefore, if a farmer had all of their plots leveled after the first auction and had no plots to bid on in the second year they were omitted from analysis.[14]

We find that farmers with at least one adopting farmer in their network were willing to pay an additional Rs. 75 per hour more for LLL custom service hire than farmers without an adopting farmer in their network, and this difference is significant at the 0.05 confidence level. This amounts to 24 percent of average WTP in the second auction (table 3, columns 1 and 2). Estimation of equation (1) using the difference in WTP between the first and second auction as the dependent variable shows that farmers with an adopter in their social network had an increase in WTP of around Rs. 65 per hour more than farmers that did not. This is over half of the mean difference in WTP between years, Rs. 112 per hour (table 3, columns 3 and 4).

An advantage of using WTP data from an experimental auction rather than observed adoption data (typically binary)[15] is that we can see changes in demand that might not push a farmer over some adoption threshold. To illustrate this point we construct a set of dichotomous adoption variables for WTP ≥ P at various prices: Rs. 250, 300, 350, 400, 500, and 600. Using a linear probability model we find that at a price of Rs 250 per hour, having at least one adopter the farmer's network increases his probability of adoption by 11 percent. At a price of Rs. 350 we find the impact to be 20 percent. However, at a price of Rs. 300 per hour we do not find a significant network effect, and the point estimate is only 6.5 percent. In areas of the IGP where LLL markets exist, the price in recent years has been between Rs. 500-600 per hour. At these prices we also do not detect significant network effects, and the point estimates are on the order of 7-9 percent. Network effects on constructed adoption variables can be found in table 4. We found even less evidence of network effects using a continuous network variable in this model (results not shown). In a static situation, the market price of a technology is ultimately the relevant price for analyzing network effects on technology adoption. However, if network effects increase demand over several seasons, or if the market price of a

[14] Farmers chose the plots they wanted leveled most for the 2011 auction. If these plots were leveled after the auction and lottery, the farmer was left with plots he presumably had less desire to have leveled in 2012. This could downwardly bias estimates of WTP in 2012 for these farmers. When we include only farmers who had no plots leveled in 2011 we find the same sized network effects.

[15] Adoption data can also be continuous, i.e. amount of land, or duration, i.e. time until adoption.

technology stands to decrease as costs decrease or the market thickens, then detecting network effects on demand below the market price may be important.

To check that there is no spurious relationship between having an adopter in one's social network (conditional on the number of would be adopters) and WTP for LLL custom hire we perform a simple zero test by regressing WTP from the first auction on network variables. We find no impact of having at least one adopter in the farmer's network, indicating that the network effects we find on demand in the second auction, or on the difference in WTP between auctions, are not spurious (table 5 columns 1 and 2). In the absence of a lottery, all would-be farmers would adopt the technology. If we regress WTP from the first auction on the number of would-be adopters in the farmer's social network we find a marginally significant positive network effect, even when controlling for observable farmer characteristics, which is clearly spurious (table 5, columns 3 and 4). The difference in the zero test results that use the lottery, and those that do not, underscores the benefit of randomly assigning the technology to first generation adopters in order to estimate network effects.

CONCLUDING REMARKS AND NEXT STEPS

In developing countries, reaching many small and often isolated farmers directly with agricultural extension to introduce new technologies is prohibitively costly. Extension therefore operates under the assumption that technology disseminated to a small set of farmers— typically progressive farmers— will result in other farmers learning about the benefits of the technology and eventually adopting. Rigorous estimation of network effects, however, is lacking. In this study we use a set of experimental auctions coupled with a randomized technology intervention to asses if having first generation adopters of a new resource conserving technology— laser land leveling— in a farmer's network increases his exposure to, and demand for, the technology.

We find that farmers with at least one early adopter in their network are more likely to talk with an adopting farmer specifically about the technology and are more likely to visit a leveled field. We also find that network effects increase demand for the technology; farmers with at least one first generation adopter in their network are willing to pay 24 percent more for laser land leveling in a second auction than comparable farmers without a first generation adopter in their network. This large increase in demand bodes well for strategies that use one-time coupons or subsidies to incite early adoption with the goal of bolstering future demand. This study

highlights the benefits of using an experimental auction to measure demand rather than using dichotomous adoption data. Because our auction data is continuous (or at least trends towards continuous), we can detect network effects below the cost of the technology, which would typically not be possible.

Moving forward, we plan on using data on farmers' impressions of laser land leveling collected intermittently during the year between auctions to examine what farmers are learning about the technology, and if these impressions line up with the impressions and actual experiences of first generation adopters. We hope that this will shed some light on whether farmers are learning about characteristics of the technology and its benefits, or if they are engaging in mimicry. We also will examine heterogeneous network effects. Because we have a great deal of information on our sample farmers, we can test to see if farmers learn from similar farmers (as determined by wealth, education, caste, etc.), and if network effects are really stronger when a progressive farmer uses the technology first. Finally, we will aim to determine if new networks form because of the introduction of the technology. We initially solicited networks before the introduction of laser land leveling, but our periodic data on exposure will allow us to see if farmers began talking to first generation adopters that were not in their networks initially.

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Figure 1. Frequency of bids for LLL custom hire in 2011 and 2012 auctions

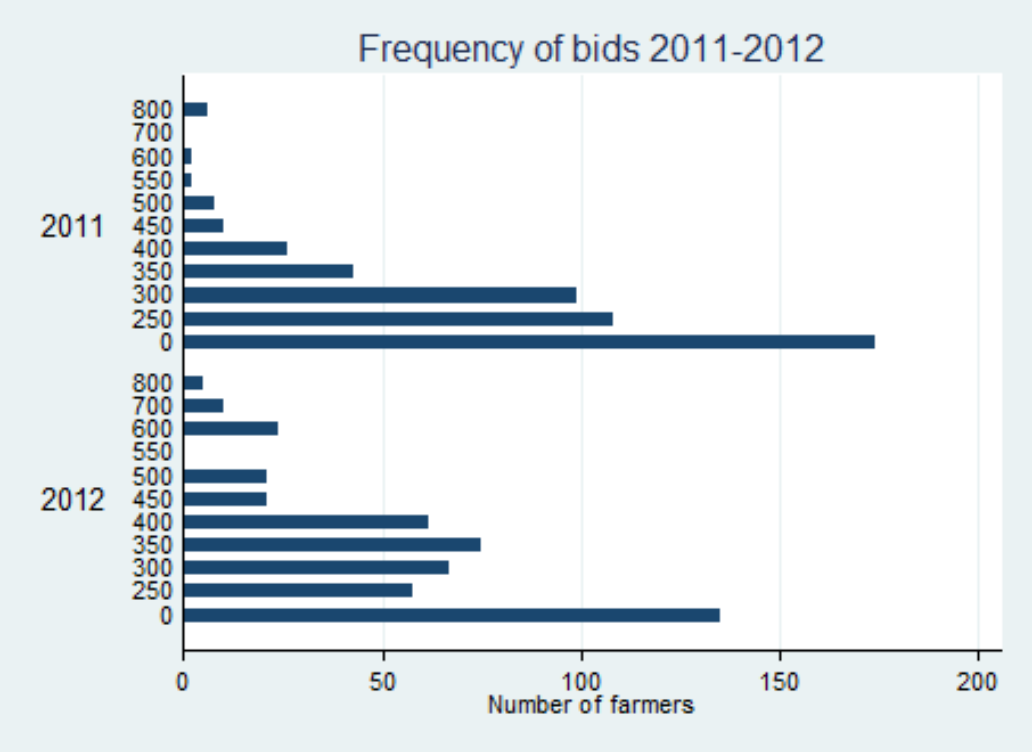

Table 1. Demographic differences between auction winners and losers (left two columns) and lottery winners and losers (right two columns)

	Auction		Lottery (Auction winners only)	
	Losers	Winners	Losers	Winners
Age (years)	48.01	48.74	48.63	48.84
	(1.10)	(0.94)	(1.35)	(1.32)
Education (years)	5.69	6.93	6.87	7.00
	(0.38)	(0.33)**	(0.46)	(0.48)
Total land (acres)	1.41	2.29	2.23	2.35
	(0.27)	(0.23)***	(0.34)	(0.31)
Wealth index	-0.162	0.106	0.098	0.113
	(0.045)	(0.068)***	(0.088)	(0.105)
N	192	286	142	144

Notes: Standard errors in parentheses, ***, **, * denote p<0.01, p<0.05, and p<0.1 for significance of t-test for differences between auction winners and losers and between lottery winners and losers. Wealth index consists of credit access, livestock, Diwali or Eid El Khabir spending, house condition, and public works (MNREGA) participation.

Table 2. Network effects on famers' exposure to LLL through interactions with adopters

	See leveler operate		Talk about LLL with adopter		Visit leveled field	
At least one adopter in network	0.001	0.024	0.212***	0.203**	0.195**	0.188**
	(0.077)	(0.076)	(0.079)	(0.080)	(0.077)	(0.078)
Number of would be adopters in network	0.104*	0.078	-0.098*	-0.104*	-0.062	-0.058
	(0.054)	(0.055)	(0.056)	(0.056)	(0.051)	(0.052)
Total network size	-0.045	-0.045	0.026	0.023	0.014	0.008
	(0.029)	(0.029)	(0.029)	(0.029)	(0.028)	(0.029)
Years of education of household head		0.623***	0.493***	0.347***	0.312***	0.137
		(0.092)	(0.029)	(0.095)	(0.028)	(0.085)
Age of household head		0.001		-0.002		0.006
		(0.005)		(0.006)		(0.005)
Farmer is male		-0.003*		0.001		0.002
		(0.002)		(0.002)		(0.002)
Farmer is in general caste		0.112		0.091		0.038
		(0.070)		(0.071)		(0.064)
Farmer identified as progressive		0.078		0.057		0.040
		(0.059)		(0.064)		(0.061)
Total landholdings (acres)		0.096**		0.006		-0.015
		(0.049)		(0.052)		(0.049)
Constant	0.620***	-0.007		0.003		-0.003
	(0.028)	(0.005)		(0.006)		(0.005)
Observations	475	475	475	475	475	475
R-squared	0.01	0.043	0.016	0.027	0.016	0.029

All dependent variables are dichotomous. Estimates are from a linear probability model. Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table 3. Network effects on demand for LLL

	(WTP 2012)	(WTP 2012)	(ΔWTP)	(ΔWTP)
At least one adopter in network	73.438**	76.396**	63.352*	67.924*
	(32.053)	(33.837)	(37.405)	(38.172)
Number of would be adopters in network	-1.375	-6.069	-36.692	-39.721
	(21.830)	(21.958)	(26.308)	(26.354)
Total network size	-0.481	-1.664	0.465	5.115
	(11.213)	(11.121)	(13.803)	(13.277)
Years of education of household head		1.474		-3.011
		(2.048)		(2.455)
Age of household head		-0.200		-1.105
		(0.620)		(0.722)
Farmer is male		7.080		6.560
		(28.676)		(31.099)
Farmer is in general caste		10.651		-13.281
		(23.361)		(25.693)
Farmer identified as progressive		40.605**		56.804**
		(19.892)		(24.197)
Total landholdings (acres)		-0.175		-5.282**
		(3.011)		(2.194)
Constant	296.99***	277.86***	116.37***	174.817***
	(11.127)	(35.668)	(13.293)	(39.853)
Observations	419	419	419	419
R-squared	0.024	0.042	0.008	0.035

Estimates are from OLS model. Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table 4. Network effects on constructed dichotomous adoption variables

	Rs. 250	Rs. 300	Rs. 350	Rs. 400	Rs. 500	Rs. 600
At least one adopter in network	0.109*	0.065	0.203**	0.058	0.091	0.074
	(0.058)	(0.070)	(0.082)	(0.086)	(0.068)	(0.058)
Number of would be adopters in network	-0.001	0.070	0.032	0.094	-0.041	-0.064*
	(0.046)	(0.054)	(0.061)	(0.058)	(0.042)	(0.036)
Total network size	-0.004	-0.004	-0.027	-0.026	0.027	0.040*
	(0.021)	(0.027)	(0.031)	(0.029)	(0.025)	(0.024)
Constant	0.795***	0.638***	0.491***	0.314***	0.125***	0.077***
	(0.024)	(0.029)	(0.031)	(0.029)	(0.021)	(0.018)
Observations	422	422	422	422	422	422
R-squared	0.011	0.022	0.028	0.021	0.011	0.016

Estimates are from a linear probability model. Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table 5. Zero test for spurious network effects on demand for LLL

	WTP 2011 (1)	WTP 2011 (2)	WTP 2011 (3)	WTP 2011 (4)
At least one adopter in network	11.315	9.681		
	(29.261)	(28.319)		
Number of would be adopters in network	33.338	33.648	37.834‡	36.278‡
	(22.014)	(21.178)	(23.823)	(23.365)
Total network size	0.388	-6.859	11.577	4.328
	(11.228)	(11.201)	(8.04)	(8.36)
Years of education of household head		4.502**		4.195**
		(1.898)		(1.882)
Age of household head		0.883		0.742
		(0.549)		(0.547)
Farmer is male		1.079		6.043
		(25.441)		(25.265)
Farmer is in general caste		22.942		24.860
		(21.326)		(21.430)
Farmer identified as progressive		-15.757		-14.669
		(18.770)		(18.816)
Total landholdings (acres)		5.208***		5.237***
		(1.769)		(1.750)
Constant	180.965***	103.899***	180.951***	107.956***
	(10.541)	(31.865)	(10.525)	(31.860)
Observations	422	422	422	422
R-squared	0.026	0.076	0.021	0.071

Estimates are from OLS model. Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1, ‡ p<0.15.

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

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Abstract

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

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1 Introduction

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

I provide new experimental evidence about the role of agricultural information on marketing outcomes. I conducted a field experiment in the central highlands of Peru where I randomly allocated price information among agricultural households in 58 villages. Roughly half of these villages were assigned to a treatment group, while the others remained as controls. Within villages in the treatment group, I randomly provided cell phones to around 120 households.

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

information more digestable — rather than providing a massive number of SMS — farmers only received information for the crops they harvested.

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

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

This paper presents four main contributions to the literature relating price information and farmers' agricultural market performance. First, I am able to isolate the short-run effect on farmers' marketing strategy by appropriately phasing the timeline of the intervention. Second, as opposed to some previous work that has focused on Information and Communication Technologies in general, the nature of the intervention allows me to disentangle the sole effect of market price information (stripped from any other potential benefit). Third, in contrast to previous papers who restrict their attention on households that had previous access to a certain technology (e.g. previous cell phone ownership, radio, etc.), this

intervention encompassed the provision of such technology. This allows me to explore to what extent selection bias may have led previous results, since households with previous access to technology tend to be wealthier and more educated. Fourth, this paper improves the contents provided to farmers, which can provide guidance as to what type and level of detail would boost the impact of future price information policies. The price information was very detailed, household-specific and provided in a digested way.

Preliminary results suggest that price information has a large and sizeable impact: farmers who directly received the information experienced 13%-17% increases in their sales prices. This result is robust to different specifications and variations in the sample. This effect was mostly driven by increases in prices for relatively more perishable products (for which information is more valuable). Consistent with higher prices, farmers with information also experienced increases in sales volumes (at the expense of reduced quantities for self-consumption). I find no differential effects by previous ownership of a cell phone. This suggests that those less familiar with this technology can also benefit from a price dissemination policy. All in all, I do not find any evidence to support the presence of spillover effects: there are no apparent price benefits to farmers who did not receive the information directly but were in villages where someone else did. Even when village-level spillovers might be somewhat broad areas for information exchange, this result is consistent when I refine potential areas for social interaction (e.g. geographic distance, crop restrictions, etc.). This work will be extended to analyze if price information also had impacts on farmers' marketing channels (i.e. whether they improve their bargaining position against a middleman, sell to a different middleman or directly sell their products in markets).

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

2 Related Literature

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

A secong group of papers have analyzed the impact of other types of Information and Communication Technologies (ICTs) in rural contexts. Svensson & Yanagizawa (2009) study the impact of the Market Information System (MIS) in Uganda, which disseminated agricultural prices through radio stations. Exploiting cross-sectional data, the authors compare households with and without radios in districts that were and were not covered by MIS.

They find that access to information increased farm-gate prices for maize by 10%-15%. Goyal (2010) investigates the impact of internet kiosks installed by a large processor in Madhya Pradesh, India; which provided soybean price information. She finds that this led to an increase of 1-3% in the prices received by farmers. It also increased the farmers' land allocated to soybeans by 19%, suggesting a substitution away from other crops.

All these papers rely on quasi-experimental data, where the variation comes from the introduction of different technologies. This literature has been contested by a third group of papers that set up interventions based on randomized controlled trials (RCT). First, Futch & McIntosh (2009) envisaged an experimental evaluation of a village phone program in Rwanda. In their pipeline design, villages were randomly assigned different times of phone installation. Unfortunately, after the baseline, actual phone installation considerably deviated from the original design. The analysis of this (non-random) data suggests that the project had no significant effect on agricultural prices[1]. They posit that — given that the diversion from the original design created a bias in favor of the control group — these estimates are probably upwardly biased and, thus, cannot explain the lack of impact. They argue this negligible impact might be explained by the presence of a very similar program that was previously set in place in most of their area of study. Second, Fafchamps & Minten (2012) conducted a field experiment providing one-year free subscriptions to an SMS-based agricultural information service (provided by Reuters Market Light) in Maharashtra, India. The subscriptions were randomly allocated among farmers who already had cell phones in this region. The service included price information in different markets, as well as weather forecasts and crop advisory. They find that such service did not lead to increases in agricultural prices for those who received it. Third, Mitra et al. (2012) study the impact of price information on potato farmers in West Bengal, India. They test the efficacy of two alternative strategies for market price dissemination: a private one where a group of ran-

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

domly selected farmers received SMSs with this information and a public one where prices where posted in public notive boards in some villages. The authors find that neither of these strategies improved farmers' market performance.

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

The design of my RCT also allows me to investigate if there are any spillover effects of information[2]. The idea of this potential effect has been present for a while. Though applied to a consumer problem, in the early sixties, Stigler had already noted that: "Information is pooled when two buyers compare prices: if each buyer canvasses s sellers, by combining they effectively canvas $2s$ sellers, duplications aside... in fact, pooling can be looked upon as a cheaper form of search" Stigler (1961, p. 219). While others have also laid out the

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

same idea[3], no empirical evidence to support it has been provided so far. To examine this possibility, I exploit the fact that the treatment was randomized at the village level in the first stage. In particular, I investigate the marketing outcomes of households who did not receive any price SMS, but lived in villages where others did. The idea is that those in this group might have been exposed indirectly to the price information, even when they did not receive such information directly.

3 The Intervention

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

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

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

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

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

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

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

tantly, I gathered retrospective data about their previous (2008/2009) agricultural cycle: production, sales volume, prices, and marketing decisions. The questionnaire also asked them which products they had already planted for the 2009/2010 season.

The baseline survey included 876 households in the 58 villages where the intervention took place. Rather than randomly allocating the cell phones among the full roster of households, the villages were assigned either to a treatment or a control groups in a first stage (Figure 3) . This initial assignment of treatment by cluster has two advantages. First, it minimized the risk of contamination of the control group - if treatment and control households were in the same village, this would increase the possibility of beneficiaries passing price information along to control households. Second, this provides a framework to investigate the existence of spillover effects in the treatment villages. Thus, the 58 villages were were randomly assigned to a treatment (28) and a control (30) group.

There were 457 households in the treatment villages, from which 119 were randomly selected to receive a cell phone. These cell phones were handed out even when the household already had one. The devices were distributed in early April, during the early harvest. For four months (mid-April to mid-August), a team of undergraduate students collected price information of 17 different products (by quality): peas, lima beans, barley, four types of corn, two types of *olluco* (a popular Andean tuber), and eight types of potato. The information was gathered in three permanent markets (Huancayo, Jauja and Tarma) and three weekly *ferias* (Chupaca, Huayucachi and Zapallanga). The calendar of price distribution is presented in Table 1. Once the information was collected, it was compared with the list of products that households planted for the 2009/2010 season according to the baseline information. During the same morning, only the information of the relevant products for each participant was sent through SMS to the number of the cell phone the intervention provided. An example of a text message with price information is presented in Figure 4a. The text message included the date, market, product, quality and price quote.

I tried to ensure that participants understood the information they were being sent. Along

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

The participants were informed of an important service restriction: during the first few months (until late August), their mobiles would only receive calls and text messages from a number authorized by the project. Through this restriction, I can rule out any other potential uses of the mobile phone that could drive the results (i.e. communication with input providers, collusion with other producers, coordination with traders, etc.). In this way, the treatment does not encompass the full advantages of a mobile phone, but only being able to receive price information in different markets. Participants were also required to answer periodic calls to check if there were any problems with the devices, whether the price SMS were being delivered appropriately, and whether they had any problems reading the information. All in all, besides being able to receive periodic check-up calls, these devices did not have any capabilities beyond those of a pager during the intervention period. However, after August, full capabilities of the cell phones would be restored and they would operate as regular pre-paid phones. Participants were told they would be able to keep the devices without any further obligation. These phones were distributed to all selected households, even to those who already owned one. No one who was offered a cell phone declined to participate in the project.

In September 2010, a follow-up survey was conducted. The questionnaire included informa-

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

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

4 Theoretical Model

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

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

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

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

4.1 Symmetric Information

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

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

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

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

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

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

In this case, it is straightforward to see that the farmer will push the trader to his reservation utility in both market price scnearios[6], so constraints (1b) and (1c) bind with equality. When

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

the self-consumption constraints (1d) and (1e) are not binding, the farmer offers:

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

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

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

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

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

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

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

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

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

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

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

4.2 Asymmetric Information

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

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

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

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

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

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

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

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

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

and the following Incentive Compatibility (IC) constraints:

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

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

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

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

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

First, assume that the crop has a low degree of perishability and poses no limits on self-consumption levels, i.e. neither (5b) nor (5c) is binding. Denote $\left\{(s_H^{AI}, Y_H^{AI});\ (s_L^{AI}, Y_L^{AI})\right\}$ as the optimal contract offered by the farmer in this situation and $r_H^{AI} = \frac{Y_H^{AI}}{s_H^{AI}}$ and $r_L^{AI} = \frac{Y_L^{AI}}{s_L^{AI}}$ as the implicit per-unit farm gate prices when market prices are high and low, respectively. Solving and comparing with (2a) and (2b) for high market prices yield:

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

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

Analogously, for low prices:

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

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

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

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

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

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

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

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

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

4.3 Discussion of the Model and Predictions

In this section, I present a model where a farmer negotiates with a trader a contract to sell his agricultural output. Such contract establishes the quantity and payment (and, implicitly, a per unit farm gate price) for their transaction. For simplicity, the model assumes that there are only two possible states of the nature: either market prices are low (p_L) or high (p_H).

The farmer offers the trader two options of quantities (s) and payments (Y): (s_H, Y_H) and (s_L, Y_L). If the trader reports that market prices are high, then the combination (s_H, Y_H) is enforced. Analogously, (s_L, Y_L) is enforced if he reports low market prices. The per unit farm-gate prices for each option are determined implicitly by $r = \frac{Y}{s}$. The farmer keeps the remaining production he has not sold for self-consumption $\bar{Q} - s_H$ or $\bar{Q} - s_L$ in each state.

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

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

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

5 Empirical Strategy and Preliminary Results

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

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

* *Info* takes a value of 1 if the household is in a treated village and received the price SMS. It takes a value of zero otherwise.
* *Spill* takes a value of 1 if the household is in a treated village but did not receive the price SMS (i.e. excludes *Info=1*) . It takes a value of zero otherwise.
* The remaining households (i.e. those with *Info=0* and *Spill=0*) are those in control villages.

5.1 Baseline Comparisons

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

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

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

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

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

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

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

5.2 The Effect of Information on Agricultural Prices

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

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

where $Log(P_{ict})$ is the logarithm of the price of household i for crop c in period t. The variable t takes a value of zero for for the 2008/2009 season (before the treatment) and a value of one for the 2009/2010 season (after the treatment). $Info_i$ and $Spill_i$ are the (time-invariant) treatment statuses for each household. D_{ict} is an indicator dummy of whether household i harvested crop c in period t. Additionally, the error term has two components. The first one (ε_i) accounts for the fact that the errors within the same household are not independent from one another. The second one (μ_{ict}) is a purely idiosyncratic error

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

can varies across households, crops and time. In particular, $\mu_{ict} \sim N(0, \sigma_u^2) = 0$ and $\varepsilon_i \sim N(0, \sigma_\varepsilon^2)$. For consistency, this specification requires ε_i to be uncorrelated with other explanatory variables. This is a plausible assumption in this setting because of the random assignment of the treatment[10]. Additionally, standard errors are clustered at the village level to allow for any covariate shocks.

In this framework, β_1 captures the changes (in percentage points) of prices experienced by those who received the information compared to those in the control villages, within each crop. β_2 provides an analogous estimate for those who may have benefited from information spillovers, relative to the control group. The results of this estimation are reported in Table (5). They suggest that there were sizeable impacts for those who benefited directly from the information: prices at which they were able to sell their production increased by 14% (with crop controls) to 15% (with crop and quality controls). The results show little evidence of spillover effects at treated villages: the estimates for β_2 are close to zero and statistically insignificant.

I also run some alternative specifications. First, I should point out that estimations in Table (5) include all households who report sales either in the baseline (2008/2009 season) or the follow-up (2009/2010 season) survey. This might introduce some bias, but the direction is not clear. For example, it might be the case that households in the control group who did not sell in the follow-up were those more likely to get lower prices. This would induce an upward bias in my estimates. However, treated households who were encouraged to sell due to the treatment (and did not sell before) might get lower prices than those who sold in both rounds (e.g. one can think of these as less experienced or less engaged in market activities). This would, in turn, suggest a downward bias in the estimates. To account for this possibility, I estimate equation (11) with a sample of households that sold in both

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

periods. The results are reported in the first two columns of Table 6. The coefficients remain relatively stable and, if anything, they increase slightly (17% with crop controls and 18% with crop and quality controls). The estimates for the spillover groups remain small and indistinguishable from zero.

Additionally, note that households in both rounds do not necessarily sell the same products and, even if they do sell the same products, they might be of a different quality. However, on one hand, note that shifts in crop types are uncorrelated with the treatment by construction: the information was provided after the sowing season, so these decisions should not have been affected by the intervention. On the other hand, product quality is mostly determined by agroclimatic conditions and should not be correlated with the treatment either. With the same seed and farming process - depending on factors like rain, temperature anomalies and crop diseases - a farmer can obtain either first, second or third quality harvests. Thus, estimates in Table (11) still provide the average effect of the intervention.

Nevertheless, it is somewhat more intuitive to compare how a specific household trades exactly the same product (type and quality) before and after the information treatment. In this spirit, I construct a restricted (though considerably smaller) sample with households that sold the same items (same product with same quality) in both periods. In this line, I estimate the following regression on my restricted sample through Ordinary Least Squares[11]:

$$Log(P_{ic,t=1}) = \gamma P_{ic,t=0} + \beta_1 Info_i + \beta_2 Spill_i + \epsilon_{ict} \tag{12}$$

Columns 3-5 of Table 6 present the results for regression (12). The results remain relatively stable with coefficients between 0.13 and 0.18 for $\beta_1$and close to zero for β_2. Therefore, even within this smaller sample with a much more narrowly restricted comparison, the results remain relatively stable.

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

5.3 Effects on Sales

Next, I estimate the impact of price information on production, probability and volume of sales. I follow the procedure described in Equation (11). The estimates are reported in Table (7). As expected, because of the timing of the intervention, the effect on production volumes is not significant (Columns 1-2). Also, I do not find a significant effect on the probability of sales as reported in Columns 3-4. However, conditional on selling, I do find a positive impact on the volume of these transactions, as reported in Column 5-6. This is consistent with the model presented in section 4, where farmers do not need to restrict in the negotiation with traders. In this sense, it seems that households previously allocating larger volumes to self-consumption or agricultural by-products are encouraged to sell more of their production in view of the higher profitability they can obtain.

I do not find any clear evidence of spillover effects among households in villages where others received the price information: while the effect in the regression without quality dummies (Column 5) is not significant, there is some effect when we control for quality (Column 6).

5.4 Heterogeneous Treatment Effects

5.4.1 Differences by Perishability of Crop

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

This is consistent with previous work that finds that the impact of price information should be more valuable for farmers who sell more perishable crops (e.g. Muto & Yamano 2009, Aker & Fafchamps 2011). While there might be a set of other factors in play (e.g. market struc-

ture, context-specific features, etc.), it is possible that differences between perishable and non-perishable crops might also explain why Mitra et al. (2012)do not find any impact of their price transmission intervention with farmers growing potato (a relatively less perishable crop) in India.

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

$$\begin{aligned} Log(Y_{ict}) =& \alpha Info_i + \theta Spill_i + \gamma t + \beta_1 Info_i t + \beta_2 Spill_i t + \\ & \delta_1 Perish_c + \delta_2 Perish_c Info_i + \delta_3 Perish_c Spill_i + \\ & \delta_4 Perish_c t + \delta_5 Perish_c Info_i t + \delta_6 Perish_c Spill_i t + \varepsilon_i + \mu_{ict} \end{aligned} \tag{13}$$

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

Results for equation (13) are reported in Table (8) and support the predictions of the theoretical model. In the regression for prices (in Columns 1 and 2), I find that the effect of the treatment is driven by a large and significant effect on perishable products (δ_5). In fact, the coefficient for other crops is small (β_1) and not distinguishable from zero, while δ_5 is large and statistically significant. However, the regression for sold quantities (in Columns 3 and 4) shows the opposite: overall increases in sold quantities are mainly driven by relatively

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

non-perishable products. While the effect for non-perishable products β_1 is large, the overall impact for non-perishable products ($\beta_1 + \delta_5$) is not significant.

5.4.2 Differences by (Previous) Cell Phone Ownership

As explained previously, one of the differences with previous papers exploiting RCTs in this area is that I do not restrict the treatment to those who already had a cell phone. In fact, cell phones were distributed regardless of previous ownership, and actually about half of my sample already had one prior to the intervention. This allows me to estimate the effects for both groups. I split my sample in two groups: those who already had a phone and those who did not, and estimate Equation (11) for both. The estimate on the former sample is roughly the one I would have obtained had my treatment been randomized only among those with mobile service. Indeed, because of variations in the intervention and the information provided, it is not strictly comparable to those in Fafchamps & Minten's (2012) study. However, they do provide an idea of what would have happened had my intervention (with the variations in the treatment) been restricted like theirs.

These results are presented in Table 9. The coefficients are similar in both groups, providing evidence that this selection is not driving my results. Thus, I posit that the divergence in these results should be due to difference in the particular contexts of the RCTs, the relevance of information provided or how the information was displayed. However, I cannot distinguish among these competing hypotheses.

5.4.3 Additional Regressions for Spillover Effects

The results in the previous sections do not support the presence of strong spillover effects. One possibility is that villages are somewhat broad areas for information exchange: if the nearest neighbor with information is still too far away, there might be no possibility for communication. In this spirit, I provide some estimates that restrict the spillover effect through geographic distances. I collected the GPS position of each household in the baseline

that allow me to control for this. For each household living in a treated village but did not receive the market price information, I estimate the distance to each household that directly received the information through an SMS. Within treated villages, I can determine the distance of each household to its nearest neighbor with information within treated villages. With this information, I create quintiles of distance to the nearest source of information. Denote D_q as dummy variables for each of these quintiles, where $q = 1$ is the group with closest neighbors that directly received the information and $q = 5$ is the most distant.

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

$$Log(P_{ict}) = \alpha Info_i + \sum_{q=1}^{5} \theta_q (D_q Spill_i) + \gamma t + \delta Info_i t + \sum_{q=1}^{5} \gamma_q (D_q Spill_i t) + \varepsilon_i + \mu_{ict} \quad (14)$$

I present these results in Table 10. If the geographic distance were to play an important role in price transmission, then we would expect $\gamma_1 > \gamma_2 > ... > \gamma_5$. However, the estimated coefficients do not suggest this pattern. All the coefficients are still small and not statistically different from zero.

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

$$Log(P_{ict}) = \alpha Info_i + \delta_0 Spill_i + \delta_1 Spill_i Match_{ict} + \gamma t + \delta Info_i t + \theta_0 Spill_i t + \theta_1 Spill_i Match_{ict} t + \varepsilon_i + \mu_{ict} \tag{15}$$

If the previous results — where there was no evidence of spillover effects for the transmission of prices — were driven by product differences, we would expect $\theta_0 = 0$ and $\theta_1 > 0$. However, the results in Table 11 show that both coefficients are small and not statistically significant. This additional piece of evidence seems to confirm the absence of spillover effects and the idea that farmers do not share the market information they receive privately with others.

6 Conclusion

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

I compare these results with the ones under symmetric information. If both parties have access to market price information, there is no need for the farmer to exercise these truth-telling mechanisms. Aa a consequence, both farm-gate prices and sales volumes should increase. It also predicts that, if there are self-consumption limits to perishable products (that cannot be fully consumed within the household before they rot), access to information would have a differential impact on this type of crops. First, increases in sales volumes shold

not be as large as with non-perishable products because farmers are not able to considerably restrict their sales volumes. Second, because even with asymmetric information they were not able to use sales restrictions as truth-telling mechanisms, perishable products would experience larger price increases than their counterparts.

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

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

I find that households with access to information are able to get better prices for their crops: their sales prices increase by 13%-17% relative to those of their counterparts. The result is robust to different samples and specifications. My results also suggest that improved prices lead to increases in volume of sales, though there is no significant impact on their probability of sales.

I also analyze if there are any information spillover effects. Direct beneficiaries might have shared the information they received with their neighbors and lead to indirect gains by others. To test for this possibility, I examine the marketing outcomes of households who

did not receive the information but lived in villages where others did. All in all, I do not find any significant impact on marketing outcomes among households in this group.

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

Figure 1: Location of the Intervention

Figure 2: Agricultural Season and Timeline of the Intervention

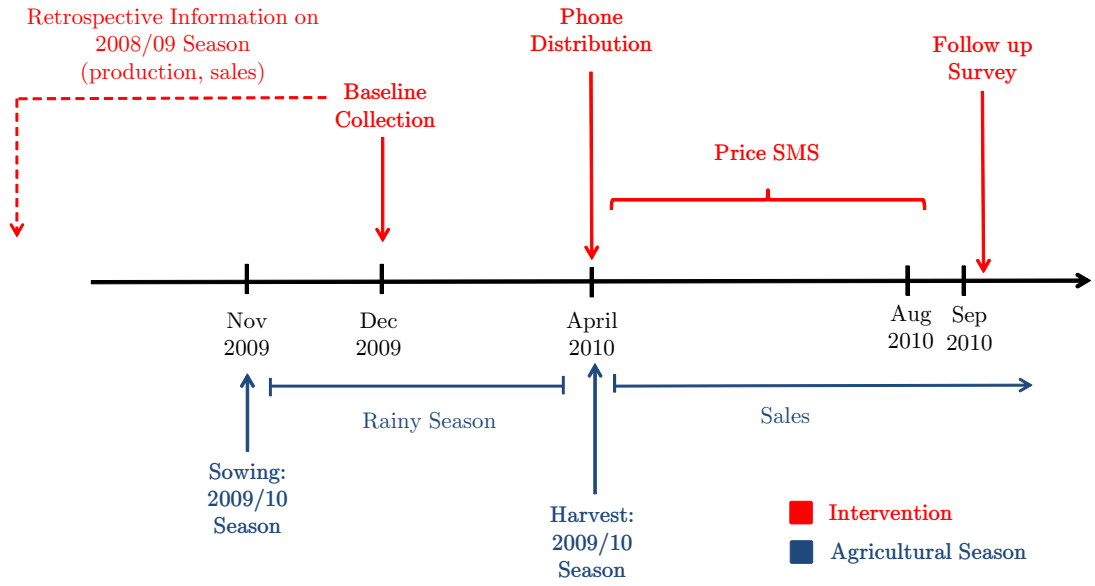


Figure 3: Location of Markets, Treatment and Control Villages

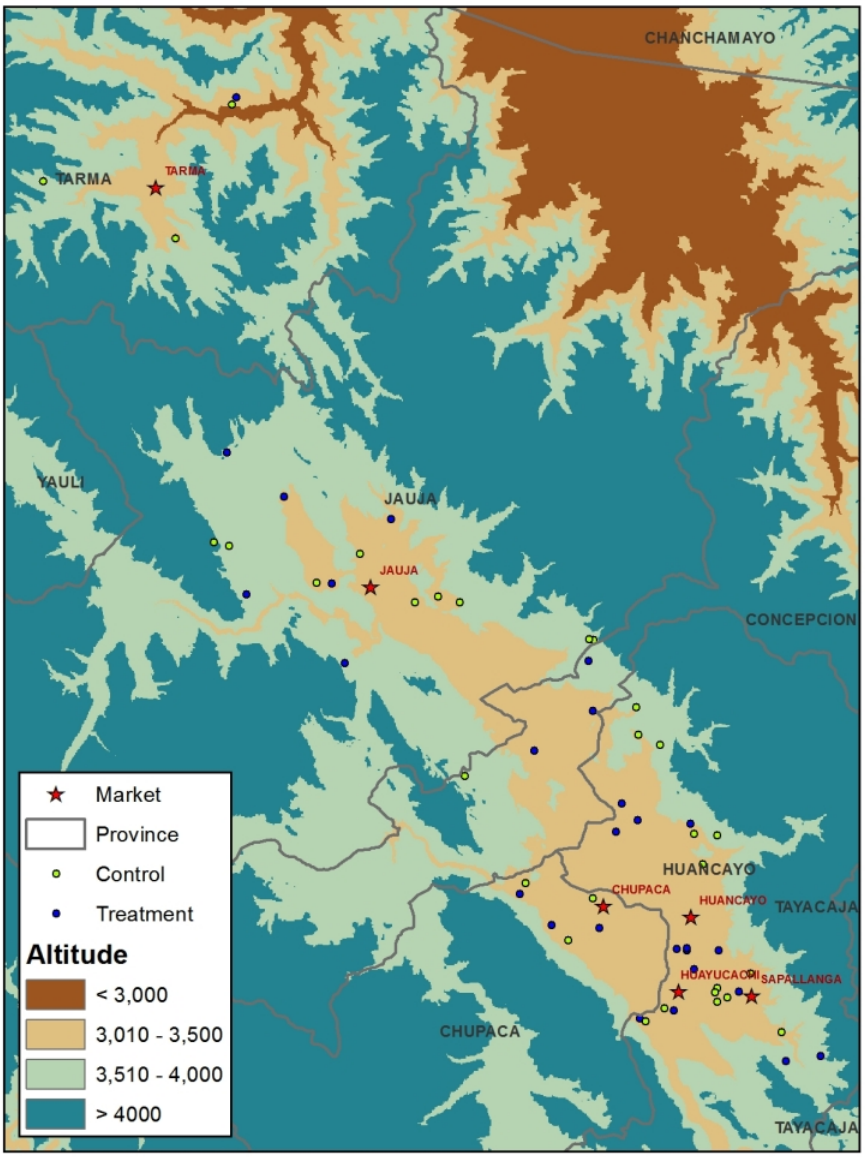
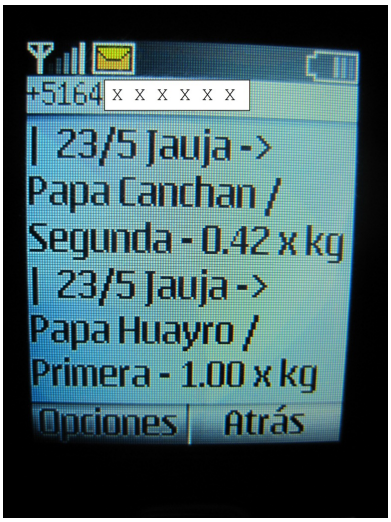


Table 1: Calendar of Price Distribution by Permanent Markets and Ferias

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

Figure 4: Cell Phone and Price Distribution

(a) Example of Price SMS



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

Table 2: Household Characteristics in Baseline

	Treat	Spill	Control	Diff[1]	
	(T)	(S)	(C)	(T)-(C)	(S)-(C)
HH Head Characteristics					
Age	50.48	51.17	49.92	0.56	1.25
	(13.11)	(15.62)	(14.58)	(1.53)	(1.09)
Head is male [2]	0.87	0.80	0.84	0.02	-0.04
	(0.34)	(0.40)	(0.36)	(0.04)	(0.03)
Years of education	7.40	7.00	7.47	-0.07	-0.48
	(3.90)	(4.21)	(3.99)	(0.42)	(0.30)
No education [2]	0.03	0.06	0.04	-0.01	0.02
	(0.18)	(0.24)	(0.20)	(0.02)	(0.02)
Primary [2]	0.44	0.46	0.45	-0.01	0.02
	(0.50)	(0.50)	(0.50)	(0.05)	(0.04)
Secondary [2]	0.44	0.38	0.41	0.03	-0.03
	(0.50)	(0.49)	(0.49)	(0.05)	(0.04)
Technical [2]	0.06	0.06	0.05	0.00	0.01
	(0.24)	(0.24)	(0.23)	(0.03)	(0.02)
College [2]	0.03	0.04	0.05	-0.01	-0.01
	(0.18)	(0.19)	(0.21)	(0.02)	(0.01)
Any member has Cell Phone[2]	0.47	0.51	0.51	-0.04	-0.00
	(0.50)	(0.50)	(0.50)	(0.05)	(0.04)
Log PC HH Exp	4.69	4.64	4.70	-0.00	-0.06
	(0.48)	(0.56)	(0.48)	(0.05)	(0.04)
Log Land	8.36	8.10	8.26	0.10	-0.16
	(1.35)	(1.51)	(1.47)	(0.15)	(0.11)
Has land with irrigation[3]	0.28	0.30	0.27	0.01	0.03
	(0.45)	(0.46)	(0.44)	(0.05)	(0.03)
N	119	338	419		

[1] For the first three columns, the means and standard deviations of each variable in the information, spillover and control groups are reported. In the last two columns, the differences were calculated using the following regression: $Y_i = \alpha_1 Info_i + \alpha_2 Spill_i + \mu_i$. Regression standard errors are reported in parentheses.

[2] In the case of discrete variables the linear regression was replaced for a probit model.

[3] The variable takes a value of one if the household has at least one plot with irrigation.

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

Table 3: Crop Compostion in Baseline

	Treat (T)	Spill (S)	Control (C)	Difference[1] (T)-(C)	(S)-(C)
Peas	0.15	0.11	0.15	0.00	-0.05
	(0.36)	(0.31)	(0.23)	(0.10)	(0.07)
Barley (common)	0.29	0.22	0.22	0.07	0.01
	(0.45)	(0.42)	(0.41)	(0.08)	(0.08)
Lima Beans	0.11	0.08	0.06	0.05	0.02
	(0.31)	(0.28)	(0.37)	(0.06)	(0.07)
Corn - White	0.41	0.40	0.36	0.05	0.04
	(0.49)	(0.49)	(0.44)	(0.13)	(0.13)
Corn - Cusqueado	0.03	0.03	0.03	0.01	0.01
	(0.18)	(0.18)	(0.16)	(0.03)	(0.02)
Corn - Cusqueno	0.04	0.02	0.01	0.03	0.01
	(0.20)	(0.14)	(0.10)	(0.03)	(0.01)
Corn - San Jeronimo	0.03	0.04	0.02	0.01	0.02
	(0.18)	(0.19)	(0.14)	(0.03)	(0.02)
Olluco - Yellow	0.07	0.05	0.09	-0.03	-0.04
	(0.25)	(0.23)	(0.29)	(0.03)	(0.04)
Olluco - Dotted	0.03	0.01	0.05	-0.02	-0.03
	(0.16)	(0.11)	(0.21)	(0.01)	(0.02)*
Potato - Yellow	0.03	0.02	0.02	0.01	0.00
	(0.16)	(0.14)	(0.13)	(0.02)	(0.02)
Potato - Andean	0.02	0.04	0.03	-0.01	0.01
	(0.13)	(0.20)	(0.17)	(0.02)	(0.03)
Potato - Canchan	0.07	0.03	0.06	0.01	-0.03
	(0.25)	(0.17)	(0.24)	(0.03)	(0.02)*
Potato - Huayro	0.02	0.03	0.02	0.00	0.01
	(0.13)	(0.17)	(0.14)	(0.02)	(0.02)
Potato - Perricholi	0.24	0.21	0.22	0.03	-0.01
	(0.43)	(0.41)	(0.47)	(0.13)	(0.13)
Potato - Peruanita	0.05	0.05	0.01	0.04	0.03
	(0.22)	(0.21)	(0.11)	(0.05)	(0.04)
Potato - Unica	0.01	0.00	0.03	-0.02	-0.03
	(0.09)	(0.05)	(0.18)	(0.01)	(0.01)*
Potato - Yungay	0.39	0.44	0.44	-0.05	0.01
	(0.49)	(0.50)	(0.50)	(0.09)	(0.09)
N	119	338	419		

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

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

Table 4: Agricultural Production and Sales Comparison in Baseline

	Log Production		Prob Sales[1]		Log Sales		Log Price	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Info_i$	0.05	0.07	0.02	0.02	0.08	0.06	-0.01	-0.01
	(0.11)	(0.12)	(0.04)	(0.05)	(0.15)	(0.16)	(0.04)	(0.04)
$Spill_i$	-0.05	0.03	-0.01	0.01	0.03	0.02	0.01	0.01
	(0.08)	(0.09)	(0.03)	(0.04)	(0.12)	(0.12)	(0.03)	(0.03)
Constant	5.63	5.81			6.11	5.97	-0.43	0.42
	(0.29)***	(0.26)***			(0.33)***	(0.34)***	(0.12)***	(0.12)***
Product Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quality Dummies	No	Yes	No	Yes	No	Yes	No	Yes
Observations	2503	2139	2503	2139	1080	1078	1080	1078
Households	876	751	876	751	532	532	532	532

[1] Marginal effects calculated from a Probit Model.

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

Table 5: DID Estimation for Prices

	(1)	(2)
$Info_i$	-0.01	-0.02
	(0.08)	(0.07)
$Spill_i$	0.03	0.02
	(0.07)	(0.07)
t	0.10	0.12
	(0.05)*	(0.06)**
$Info_i$ x t	0.14	0.15
	(0.07)*	(0.08)*
$Spill_i$ x t	-0.00	0.00
	(0.06)	(0.07)
Constant	0.21	0.25
	(0.17)	(0.14)*
Product Dummies	Yes	Yes
Quality Dummies	No	Yes
Observations	2116	2014
Households	612	612

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

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

Table 6: Additional Price Regressions with Restricted Samples

Sample	HH in both Rounds[1]		HH with same prod and qual in both rounds		
Dep Variable	P_{it}		$P_{i,t=1}$		
	(1)	(2)	(3)	(4)	(5)
$Info_i$	-0.03	-0.02	0.18	0.13	0.13
	(0.08)	(0.07)	(0.06)***	(0.07)*	(0.08)*
$Spill_i$	0.04	0.03	0.02	0.02	0.03
	(0.07)	(0.07)	(0.06)	(0.07)	(0.07)
t	0.08	0.09			
	(0.06)	(0.06)			
$Info_i$ x t	0.17	0.18			
	(0.08)**	(0.09)**			
$Spill_i$x t	0.02	0.02			
	(0.07)	(0.08)			
$P_{i,t=0}$			0.55	0.33	0.13
			(0.08)***	(0.08)***	(0.07)*
Constant	-0.06	0.07	-0.17	0.16	0.32
	(0.07)	(0.05)	(0.05)***	(0.06)**	(0.07)***
Product Dummies	Yes	Yes	No	Yes	Yes
Quality Dummies	No	Yes	No	No	Yes
Observations	1610	1608	263	263	263
Households	333	333	176	176	176

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

Table 7: Difference-in-Differences Estimations for Production and Sales

	Log Production		Prob Sale[1]		Log Sales	
	(1)	(2)	(3)	(4)	(5)	(6)
$Info_i$	0.14	0.16	0.02	0.01	-0.00	-0.05
	(0.21)	(0.21)	(0.06)	(0.07)	(0.20)	(0.22)
$Spill_i$	-0.08	-0.01	-0.04	-0.02	-0.00	-0.03
	(0.17)	(0.18)	(0.04)	(0.06)	(0.19)	(0.21)
t	-0.47	-0.34	-0.10	-0.01	-0.45	-0.42
	(0.12)***	(0.12)***	(0.03)***	(0.05)	(0.12)***	(0.11)***
$Info_i$ x t	0.17	0.11	0.08	0.08	0.30	0.32
	(0.18)	(0.17)	(0.06)	(0.07)	(0.16)*	(0.17)*
$Spill_i$ x t	0.19	0.13	0.09	0.08	0.24	0.26
	(0.15)	(0.15)	(0.06)	(0.07)	(0.15)	(0.15)*
Constant	4.76	5.41	0.47	0.90	5.74	5.92
	(0.28)***	(0.30)***	(0.07)***	(0.12)***	(0.27)***	(0.32)***
N	4752	3917	4759	3757	2116	2014
Households	829	775	829	775	612	612

[1] Linear Probability Model.

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

Table 8: Effects by Product Perishability

	Log Price		Log Sales	
	(1)	(2)	(3)	(4)
$Info_i$	0.22	0.19	-0.24	-0.32
	(0.19)	(0.18)	(0.29)	(0.31)
$Spill_i$	0.15	0.12	-0.14	-0.19
	(0.17)	(0.17)	(0.30)	(0.30)
t	0.14	0.17	-0.50***	-0.47***
	(0.08)*	(0.09)*	(0.15)	(0.14)
Perishable [1]	0.27	0.26	-0.61***	-0.64***
	(0.04)***	(0.05)***	(0.14)	(0.13)
$Info_i$ x t	0.07	0.07	0.43**	0.45**
	(0.10)	(0.11)	(0.20)	(0.21)
$Spill_i$ x t	-0.03	-0.04	0.28	0.29
	(0.09)	(0.10)	(0.18)	(0.18)
$Info_i$ x Perishable	-0.14	-0.09	-0.04	0.30
	(0.10)	(0.12)	(0.23)	(0.20)
$Spill_i$x Perishable	0.09	0.12	0.05	0.16
	(0.10)	(0.10)	(0.23)	(0.21)
t x Perishable	-0.12	-0.17	0.11	0.08
	(0.06)**	(0.09)*	(0.41)	(0.42)
$Info_i$x Perishable x t	0.32	0.30	-0.10	-0.32
	(0.15)**	(0.15)*	(0.57)	(0.55)
$Spill_i$ x Perishable x t	0.22	0.23	-0.32	-0.36
	(0.20)	(0.18)	(0.54)	(0.52)
Constant	-0.66	-0.53	6.51***	6.76***
	(0.14)***	(0.14)***	(0.23)	(0.24)
Product Dummies	Yes	Yes	Yes	Yes
Quality Dummies	No	Yes	No	No
Observations	2116	2014	2116	2014
Households	612	612	612	612

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

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

Table 9: Effect by (Previous) Cell Phone Ownership[1]

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

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

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

Table 10: Effects by Distance to Nearest Neighbor with Information[1]

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

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

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

Table 11: Effects by Crop Match to Households with Direct Information[1]

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

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

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

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