PRICE INFORMATION, INTER-VILLAGE NETWORKS, AND "BARGAINING SPILLOVERS": EXPERIMENTAL EVIDENCE FROM GHANA*

Nicole Hildebrandt[1], Yaw Nyarko[2], Giorgia Romagnoli[3], and Emilia Soldani[4]

[1]Boston Consulting Group
[2]New York University and CTED
[3]University of Amsterdam
[4]Goethe University, Frankfurt am Main

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Abstract

Through a randomized experiment and detailed data on communications among farmers, we identify the impact of text-messages-based commodity price information on rural farmers' revenues. The intervention affected prices received by farmers in two ways: (1) a long-lasting increase (9%) for treated farmers, and (2) substantial indirect benefits for certain control group farmers, which cannot be explained by classical informational spillovers. We discuss a novel mechanism of bargaining spillovers which can explain such positive externalities, even in the absence of information sharing between the treatment and control groups. Our results highlight the importance of accounting for longer-run spillovers and the potential of ICT interventions in emerging markets.

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Keywords: Price information, Agriculture, Bargaining, ICTs, Networks, Externalities.

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1 Introduction

The rapid increase in mobile phone coverage and ownership in developing countries is making it easier to provide farmers with accurate, (near) real-time information on prices to help them make optimal marketing decisions. Can such market information help farmers get higher prices for their production? And, what are the indirect impacts of information provision on traders, on farmers who do not have access to price information, and on market outcomes as a whole? Given the growing interest in ICT-related informational interventions by policymakers, foundations, and governments around the world, these questions are fundamental. We answer them through a two-year randomized evaluation of an SMS-based market information system (MIS) in Ghana. We document positive impacts of price alerts on the prices received by treated farmers for certain crops, and sizeable indirect effects on the control group.

For the study we partner with the local agricultural information service provider Esoko, recruit 1,000 smallholder commercial farmers in the northern part of the Volta region, and collect detailed information on their sales, production and marketing behavior for two years. About half of the farmer communities in the sample are randomly assigned to the treatment group, and receive weekly price alerts for local and urban markets, for their main commercial crops. The other half receive no alerts, and serves as our control group.

The study makes three key contributions to the literature on ICT-related information interventions in developing countries, and to the broader methodological literature on randomized evaluations. First, it shows that the price alerts have large positive effects on the prices received by farmers, and that these effects are sustained over time. Second, by comparing price trends across different crops, which are grown by the same farmers and characterized by different marketing environment, we shed light on the mechanisms which can facilitate or hinder the impact of price alerts. In our setting, the positive price impact on yam prices (about +9%) is driven by improvements in farmers' bargaining outcomes with traders, rather than changes in other aspects of farmers' marketing behavior.[1] Third, thanks to the collection of very detailed information on inter-village

[1] Previous economic evaluations of similar interventions have come to mixed conclusions on the benefits of these services for farmers. Randomized control trial (RCT) evaluations of MIS in Columbia (Camacho and Conover, 2011) and India (Fafchamps and Minten, 2012; Mitra et al., 2014) have failed to find measurable impacts on producer prices. In contrast, Svensson and Yanagizawa (2009) and Nakasone (2013) find that MIS in Uganda and Peru, respectively, increase producer prices by 13%-15%. Courtois and Subervie (2014) use propensity score matching methods to evaluate the impact of the same MIS we study here, and find impacts ranging from 7%-10%, albeit for a set of crops

communication and marketing networks, we identify large positive spillover effects on control group farmers. The spillover effects begin to appear several months after the start of the intervention and increase over time. By Year 2, they are comparable in magnitude to the direct benefit for the treatment group. As a result, standard estimates of the treatment effect based on the assumption of no spillovers (e.g. on the so-called SUTVA assumption), would lead to the erroneous conclusion that the price alert service had no long-run benefit to treated group farmers.

Our analysis of the spillover effects of price alerts contributes to the field experimental literature looking at the indirect effects of interventions.[2] Particularly in the realm of agriculture, other studies have focused primarily on the possibility of spillovers occurring within a village and being driven by information sharing or control units gaining indirect access to the treatment. In general, the size of such spillovers is positively correlated with the density of treatment in any given geographical area.[3] In contrast to previous studies, we extend our focus to spillovers *across* villages. We show that their size is correlated with the density of treatment in the relevant marketing and social network, which we empirically capture with the average degree of connection to treated communities. Furthermore, the spillovers are not directly driven by information sharing from treated to control farmers. To understand the nature of the spillovers, it is important to notice that, in the absence of price alerts, the interaction of farmers and traders is characterized by asymmetric information: while traders presumably have real time information on the urban prices of each crop, most farmers do not. Our intervention provided treated farmers with such information, leading them to ask traders for higher prices. We speculate that, when faced with a sufficiently high share of informed farmers, traders switch to less aggressive bargaining, leading to higher prices also for farmers who are not informed.

for which we find no effect. Goyal (2010) studies a related intervention involving information kiosks in district markets in Andhra Pradesh, and finds that the kiosks increased producer prices by about 1%-3%. There is also a literature looking more broadly at the impact of mobile phone coverage on agricultural outcomes in the developing world; see Jensen (2007), Aker (2008), Muto and Yamano (2009), Jensen (2010), and Aker and Fafchamps (2014). Based on our analysis, we can speculate that differences in the perishability of the harvested crop and in the prevalence of bargaining in the markets considered may help rationalize the heterogeneity of results proposed in the literature. In our context, the prevalence of bargaining is linked to the nature of the relationship between farmers and buyers.

[2]This literature covers a range of interventions in areas such as disease control (Miguel and Kremer, 2004), labor markets (Crépon et al., 2013), and elections (Asunka et al. (2014) and Giné and Mansuri (2011)).

[3]Notable exceptions are Burke (2014) and Svensson and Yanagizawa-Drott (2012), who consider also general equilibrium effects. Specifically, Burke (2014) considers the general equilibrium effects of a loan intervention for maize farmers in Kenya, while Svensson and Yanagizawa-Drott (2012) estimate the partial and general equilibrium effects of a national MIS in Uganda, in a non-experimental setting. In their study, the changes in marketing behaviors of informed farmers lead to a decrease in the urban market prices, resulting in lower farm-gate prices and therefore negative spillover effects on uninformed farmers. The differences between their findings and ours are likely due to the characteristics ot the respective marketing environments and to the smaller scale of our intervention.

To formalize our intuition of the mechanism driving spillovers, we build a model of bargaining in the presence of asymmetric information. The first key assumption in the model is that traders cannot distinguish between treated farmers, who are informed about urban prices, and control farmers, who are not informed. This allows for "bargaining spillovers" to occur in our model, as traders adjust their optimal bargaining strategy and offer higher prices to farmers they believe are likely to be informed, irrespective of their true informational status. The second key assumption is that traders' beliefs on the probability that a certain farmer community is informed is increasing in the community's own connections to treated communities in the area. Given this assumption, the model can explain our empirical finding that the bargaining spillovers are higher for farmers with strong connections to treated communities. Our data offer reasonable empirical support for both assumptions, as well as for an additional prediction of the model related to the timing of sales for treated farmers. As we discuss in the paper, these predictions would be very hard to reconcile with a scenario where spillovers are driven exclusively by control farmers getting urban price information from the treatment group. In this sense, they support our hypothesis that traders' reactions to the intervention—and thus, bargaining spillovers—played an important role in our setting.

Our results illustrate that indirect impacts can be substantial, and if ignored can lead to serious misinterpretations of the effects of the programs being studied. Our results are also significant for policymakers and foundations engaged in the ongoing debate about the benefits of ICT-based interventions in the developing world. Compared to many other interventions in the agricultural marketing space, our estimated treatment effects—on the order of 9% increase in prices—are fairly substantial.[4] For the median yam farmer selling 1,200 tubers in a year, our estimated treatment effect translates into an additional 170 GHS (US$114) in annual revenues[5]. Considering that profit margins for farmers are believed to be low, the impact could be considerably larger in terms of an increase in farm profits. While we do not collect data on production costs or on profits, the literature suggests profit margins of around 50%: if costs are unaffected, the 9% increase in prices would hence result in a 18% increase in profits.[6] As these figures don't take into account the additional benefits on control farmers, they can be interpreted as a lower bound of total effects.

[4]See footnote 1 for a list of related studies and the corresponding estimated impacts.

[5]The conversion is based on the exchange rate at the time of data collection.

[6]Several recent papers looking at the profitability of yam farming in Nigeria suggest that profit margins may be about 50% (see Izekor and Olumese, 2010; Sanusi and Salimonu, 2006).

Another way to consider the magnitude of the intervention, to get a measure comparable in spirit to the return on investment (ROI), is to compare the cost of the service with the estimated benefits to farmers. If we ignore the indirect benefit on farmers in the control group, the average impact on the annual revenues of treated farmers is about 170 GHS[7]. The cost of the service amounts to about 78 GHS per farmer, inclusive of subscription (18 GHS) and training (60 GHS). The return per farmer is therefore about 200% of the total costs and it is likely to grow over time because the largest component of costs is training, which is only needed once.[8] The estimated return to costs would be even higher if the indirect benefits on control farmers were accounted for.

The remainder of this paper is structured as follows. Section 2 provides an overview of agricultural marketing in Ghana and outlines our experimental design. Section 3 describes the data we collected during the study and presents relevant descriptive statistics. Section 4 details our empirical methodology and Section 5 presents the resulting estimates, with a focus on identifying the driving mechanisms and the role of spillovers (Section 5.1). Section 6 formalizes our narrative in a model of bargaining with asymmetric information and discusses how such model fits our data. Section 7 concludes.

2 Background and Experimental Design

As in other parts of sub-Saharan Africa, a majority of farmers in Ghana are smallholder farmers who heavily rely on traders (middlemen) to market their production. Traders travel around the country purchasing agricultural output from farmers, transport it to urban markets and sell it there, often at a significantly higher price.[9] Transactions between farmers and traders usually take place at the farm gate, local community, or local market. They are conducted informally and involve some amount of bargaining between the parties, which vary across crops[10].

Because traders travel extensively, they tend to have detailed knowledge on market prices and trends, significantly more so than farmers. Farmers often complain that they are being cheated

[7] All costs are denominated in real August 2011 cedis.

[8] Although expensive, we view training as essential to wide scale take-up and usage of the service. See footnote 50 for details on the computation of the costs and returns.

[9] Middlemen should not be confused with small-scale local traders, who are mainly women and aggregate crops to re-sell to the middlemen. We focus on middlemen as they are the dominant actors in the market and farmers identify them as the source of cheating in negotiations.

[10] Formal contractual relationships between farmers and traders—e.g. pre-harvest contracts where buyers pre-pay for crops in advance of harvest—are rare in Ghana (Quartey et al., 2012).

by traders, who would lie about urban prices (which farmers are unable to verify) in order to buy at lower prices. Given the information asymmetry between farmers and traders, providing farmers with better price information may help them secure higher prices, but the ultimate impact depends on the existing market conditions. In general, informational interventions could lead to higher revenues or prices through an array of alternative mechanisms. Treated farmers could use the price information to: (i) inform their decision of where to sell, (ii) when to sell, (iii) what crop to grow and in what quantities and/or (iv) improve their bargaining with middlemen. Based on our discussions with farmers using the service in other parts of the country, and on the features of the marketing environment we highlight below, our prior belief was that the main mechanism at play would be bargaining. More specifically, (i) the choice of where to sell is limited because transport to distant markets is costly and entails taking on substantial risk and possessing knowledge of how to arrange transport. Similarly, (ii) significant changes in timing of sales seem unlikely because liquidity constraints force most farmers to sell during the harvest season rather than postponing to the dry season, in which prices are higher. Finally (iii) the two-year duration of our study is conceivably too short for farmers to adjust the type of crop produced, given the empirical evidence on slow adoption of new technology in agriculture (Conley and Udry, 2010). Indeed, as detailed in Section 5 and Appendix A, we find little if any evidence of price alerts leading to changes in place or timing of sale or shifts in the crop grown or land cultivated.

The extent to which bargaining can improve farmers' outcome depends on the market structure. More precisely, a necessary condition for price information to benefit farmers is that traders are not already operating in a perfectly competitive market setting (Jensen, 2010). In our study area, traders are best described as operating under oligopoly: barriers to entry are high, since trading requires access to capital and a network of farmers and transporters.

Table I summarizes the relevant market conditions by crop, based on data we collected at the beginning of our study, before implementing the treatment.

[INSERT TABLE I HERE]

As reported in Table I, the typical farmer in our study area sells on average to 3-5 different traders per agricultural season, depending on the crop, confirming that monopoly is not the best representation of the market between traders and farmers. On average, farmers have long-standing relationship with less than half of these traders for yams, groundnut and processed cassava. This is an important feature, as the prevalence of long-standing relationships with traders, especially if they involve the provision of credit or agricultural inputs, could hinder the effectiveness of informational interventions (Molony, 2008).[11]

Table I also reveals that yam marketing is special along several dimensions. First, yam is the only crop for which bargaining is a universal feature of crop marketing. For products such as maize and gari (a form of processed cassava), prices are fairly homogeneous among sellers in the local market, and farmers often report paying the prevailing "market price" for their production. For yam, no such reference price exists, and the farmer's ability to successfully negotiate with the trader is reported to be a crucial determinant of the final price. As we discuss below, this difference is fundamental in mediating the impact of the intervention on different crops.

Not surprisingly, the variability of prices across farmers is larger for crops with prevalent bargaining. The data in Table I refer to the agricultural year prior to our intervention: price variation is highest for yam (for which bargaining is universal), and lowest for maize and processed forms of cassava such as gari (for which bargaining is much less prevalent).[12]

Middlemen are also significantly more active in the purchase of yam than other crops. In fact, yam trading mostly takes place the day before the actual market day, in a separate area of the marketplace. Finally, yam is the only crop that is sold in urban markets by a non-negligible proportion of farmers: selling yam directly in the urban market may therefore be a valuable outside option when turning down a trader's low offer. Note that heterogeneity in the characteristics of farmers across crops cannot explain the differences in the market environment, since most farmers in the area grow more than one crop.

Based on the characteristics discussed above, yam appears to be the crop where the bargaining channel is most likely to translate price information into higher revenues. Therefore, if the impact of the intervention is indeed mediated by bargaining, we should expect to observe larger treatment effects for this crop.

[11] Molony (2008) provides evidence that Tanzanian farmers are unable to exploit mobile phone-based information services in their negotiations with traders for fear of breaking long-term relationships with middlemen who also supply them with credit. Farmers in our study did not express concerns of this type.

[12] Lower price dispersion, for farmers selling in different markets, may indicate higher market integration, which would limit the room for informational interventions to improve farmers outcomes. A full analysis of the spatial integration of markets for different crops in Ghana is out of the scope of this paper, but Cudjoe et al. (2008) suggest that markets in Ghana are well-integrated for grains (maize and rice), but not for tubers (yam and cassava). If urban market prices co-moved with the local prices, for which farmers obviously possess better information, our intervention would lead to little change in farmers' information set, and thus little change in outcomes.

2.1 Experimental Design

We conducted our experiment in the northern part of the Volta region, an area that lies in central-eastern Ghana, approximately 300km from Accra. Within the study area, we sampled 100 communities located within four contiguous districts.[13] From each community, we sampled 10 farmers to be included in the study among those who sell at least some portion of their crop (i.e. we excluded subsistence-only farmers). Nine farmers declined to be part of the study, leaving our final sample at 991 farmers.

Our randomization strategy is designed to (1) minimize the risk of information spillovers while also (2) ensuring balance between treatment and control groups.[14] To minimize spillovers, we group highly-connected communities together into what we call a "community cluster," and then randomize at the community cluster level. To measure connections across communities pairs, we collect detailed data on inter-village marketing and communication network. In particular, for any pair of farmer communities, we define a *market overlap index* and a *marketing communication index*. The former measures how many farmers from the two villages sell in the same markets, ro to the same traders. The latter measures how many farmers from one community talk to farmers in the other one about agricultural marketing, and how often. We then use principal component analysis to extract a scalar "connectedness index" based on the indexes of market overlap and marketing communication and on geographic proximity. To form community clusters, we select a cut-off value for the *connectedness* index and cluster together the pairs of communities with scores higher than the cut-off. This process results into the formation of 90 community clusters, out of 100 communities.[15] We then proceed to randomly assign community clusters to the treatment and control groups. To ensure balance, the randomization is stratified on two characteristics: the district (Nkwanta North, Nkwanta South, Krachi East, Krachi West), and the most commonly-grown crop (yam, or not yam). Within each strata, we randomly assign half of the community

[13]We chose this area for two reasons. First, the area is "virgin territory" in the sense that the MIS we study was not previously present in this area, and there are few NGOs operating there. Second, the area is fairly self-contained geographically: the Togo border lies to the east, and the Volta Lake lies to the west. The four districts we included in the study are Krachi East, Krachi West, Nkwanta North, and Nkwanta South. Ghana consists of 10 administrative regions, which are further subdivided into districts. There are approximately 216 districts in the entire country, and 25 districts within the Volta region.

[14]A well-known trade-off exists between these two goals: minimizing spillovers requires that treatment and control groups be sufficiently far apart geographically, while balance requires that treatment and control groups be similar to each other, and similarity usually calls for geographical proximity (Duflo et al., 2007).

[15]For more details on this procedure, see Appendix B.

clusters to the treatment group, and half to the control group. The treatment group includes 45 clusters (corresponding to 49 communities), the control group 45 clusters (51 communities).

2.2 Details about the treatment

Farmers in the treatment group are given a free subscription to a Market Information Service (MIS) operated by the privately-held company Esoko. The MIS provides weekly price alerts to subscribers via SMS (plain text message). Since most markets in the country are weekly, this should provide farmers with the most up-to-date price information available. The weekly price alerts started in October 2011 and cover the farmer's two main commercial crops, for four local markets in the study region and four of the main urban markets in the country.[16] To ensure that farmers can process and use the information, the treatment also include in-depth training.

Farmers in the control group were not provided with trainings or price alerts, but were surveyed with the same frequency as farmers in the treatment group.

3 Data

Over the course of the study, we gather extensive data on farmers and their marketing behaviors for yams but also for the other crops, and this enables us to understand in great detail the impact of the intervention. From August 2011 through June 2013 we gathered monthly transactional data for all farmers in the study, providing information about every sale transaction conducted by the farmer for his/her two main commercial crops (quantity and variety sold, total revenue, price per unit, place of sale, and type of buyer).[17] We supplement this transactional information with three annual surveys covering a wide range of topics, including socio-demographic traits, sources of information

[16] Esoko relies on a network of "market enumerators" to collect these market prices. Esoko trains enumerators to ensure that prices are collected in a consistent manner across markets, and holds twice-yearly refresher trainings to reinforce the enumeration methodology. In addition, the company quality reviews all prices before they are sent out and occasionally employs "mystery shoppers" to validate the information sent in by enumerators. Esoko operates its MIS in 10 countries across the African continent. The four urban markets covered by the price alerts in our study are Accra-Agbogbloshie, Accra-Ashaiman, Tema, and Koforidua. The four local markets are Nkwanta, Kpassa, Borae, and Dambai. Prior to the start of our experiment, Esoko did not monitor prices at these local markets, due to the fact that it had virtually no MIS subscribers in this area. As part of the study, we commissioned Esoko to begin gathering these market prices. Price alerts were in English, one of Ghana's official languages. Prices were sent in local unit measures, e.g. 100 tubers of yam, 1 long bag of maize, since the use of standard international units of measure is not common in Ghana.

[17] Most farmers in Ghana grow a variety of crops for consumption and sale, rather than focusing exclusively on a single crop. This is also true in our sample.

about marketing and prices, and general marketing behaviors. The annual surveys were conducted at baseline (in July-August 2011, prior to the start of the intervention), midline (in July-August 2012, about nine months after the start of the intervention) and endline (in June-August 2013, about 1.5 years after the start of the intervention).

The richness of our data allows us to provide new empirical evidence on the impact of MIS along two dimensions. First, using the monthly data we are able to compare short- (i.e. within the first year) and longer-run (i.e. second year) effects and look at the *dynamics* of the treatment over time. Second, the detailed information in the annual data allows us to test competing hypothesis for the mechanisms that could be driving our results.

Descriptive Statistics and Balance

Table II reports the summary statistics of our sample at baseline, by treatment status, and the tests of balance between the treatment and control groups. Overall, the variables are well balanced between treatment farmers and control farmers.[18]

[INSERT TABLE II HERE]

Farmers in our study are 41 years old on average, are predominantly male, and rely on farming as the main source of household income. The sample is not highly educated: while 42% have completed junior high school, nearly 50% have no formal education. Median income earned from the farmer's two main commercial crops amounted to GHS 1,400 (US$898) in the agricultural season ending in June 2011.[19] The main crops grown by farmers in the sample are yam, cassava, maize, and groundnut. Yam is by far the most commonly grown crop, with over 60% of farmers reporting it as one of their two main commercial crops. Farmers' knowledge of urban market prices is very poor: only about 30% of farmers believe that they are well informed about urban market prices at the time of the baseline survey. Farmers are more informed about local market prices, consistently with the fact that most farmers actively sell in local markets.

Because our main analysis focuses on yams, we report in Table III the same statistics for the subsample of 628 farmers who grow and market yams during the period of our study.

[18] To test for joint orthogonality, we follow (see McKenzie, 2015) and consider the Wald χ^2 test of joint significance of the coefficients in a probit model for treatment, where all of the variables in Table II are used as independent variables. The Wald χ^2 for the entire sample is 15.44 and the associated p-value is 0.493: we therefore cannot reject the null hypothesis of joint orthogonality.

[19] Dollar figures are calculated using the average GHS-USD exchange rate for 2011 from Oanda.com.

[INSERT TABLE III HERE]

As yam is one of the main crops in the area, it is not surprising that the characteristics of this subsample do not significantly differ from those of the full sample, and that at baseline yam farmers in the treatment and control groups are statistically identical along most dimensions.[20]

Only a few differences can be noted between the average yam farmer and the average generic farmer in our study: yam farmers in the study are more likely to be men and slightly less likely to have completed junior high school or more, they own fewer assets and are more likely to sell at urban markets. At baseline, yam farmers in the treatment group are less informed about urban prices and have lower expertise with text messages than yam farmers in the control group. We are not worried about differences in text message expertise, as this is specifically targeted during the training.

4 Methodology

Our goal is to quantify the impact of price alerts on the yam price received by farmers in our study. Our focus is on yam, as this is the only crop for which the marketing environment suggests that price alerts could indeed have an impact (see discussion of Table I in Section 2). Nonetheless, the data for the other crops serves as a valuable benchmark at several steps of the study. The analysis of the effects of our intervention can be organized in four steps. First, we estimate the shorter and longer run (average) intention to treat effects on the price received by yam farmers. In this phase, as customary with impact evaluations based on RCT, we rely on the SUTVA assumption. In our setting, this amounts to assuming that the price alerts had no impact on the prices received by farmers in the control group.[21] Second, we empirically investigate the mechanisms through which price alerts may affect the outcomes: quantities sold, timing of harvest and sale, place of sale, bargaining between farmers and wholesale traders. Third, we test for the presence of spillovers. The third phase relies on the assumption that the spillovers to the control group should be increasing over time and positively correlated with the degree of connection to the treated communities. The increase over time does not need to be monotonic. Fourth, we propose a simple method to account

[20] To test the hypothesis of joint orthogonality, we compute the Wald χ^2 for the subsample of yam farmers. the resulting score is 18.68, with p-value 0.2287: the null hypothesis of joint orthogonality cannot be rejected.

[21] SUTVA stands for Stable Unit Treatment Value Assumption (SUTVA). The SUTVA requires that "the potential outcomes for each individual i are unrelated to the treatment status of other individuals" (Angrist et al., 1996).

for spillovers and derive a "de-biased" estimate of the effects of the price alerts on prices received by farmers in the control and treatment groups. For this step, we rely on stronger assumptions on the structure of spillovers.

In the first stage, if there are no spillovers, the Intent-to-Treat (ITT) effect of price alerts on the prices received by farmers, in each period $s \in \{0, 1, 2\}$ (pre-treatment, shorter-run, and longer-run), is identified by the coefficients κ_s in

$$p_{ijt} = \sum_{s=0}^{2} \{\lambda_s Y_s + \kappa_s (T_j * Y_s)\} + X'_{ij}\psi + \omega_k + \omega_t + e_{ijt} \qquad (1)$$

where p_{ijt} is the producer price outcome for farmer i living in community cluster j selling in month t, T_j is a treatment status indicator, Y_s is an indicator for period s, ω_k are randomization strata fixed effects, ω_t are time period fixed effects, and X_{ij} is a set of additional covariates (gender, asset index, and the community's distance to the closest district market).[22] The coefficients λ_s capture the average control group price in period s. The inclusion of randomization strata fixed effects and time period fixed effects implies that the ITT effect is identified from within-period, within-strata variation between treatment and control groups. We estimate Equation 1 both in levels and logarithms.

In order to further explore the mechanisms, and the role of bargaining in particular, we report estimates for Equation 1 also for the other crops. In additional analysis reported in Appendix A we adapt Equation 1 to estimate the intention to treat effects on quantities sold and on place and timing of sales. This amount to substituting such outcomes for prices p_{ijt} in Equation 1. In order to test the bargaining channel more explicitly, we further estimate the following multivariate regression equation

$$p_{ijt}^{ASK} = \alpha AccraPrice + \beta T \cdot AccraPrice + w_k + w_t + e_{ijt}, \qquad (2)$$

where p_{ijt}^{ASK} is the first price request made by yam farmer i from community j in month t and $AccraPrice$ is the average yam price in Accra in the same month (i.e., the content of the price alerts received by farmers in the control group). As above, strata (w_k) and year (w_t) fixed effects are

[22] See Bruhn and McKenzie (2009) for a discussion of why it is appropriate to include strata fixed effects in a setting comparable to ours.

included. We estimate this equation using solely data from the extensive annual surveys conducted at midline and endline (i.e. all months after August 2011), as in these surveys we have details about who started the negotiation and what were the initial and ending price offers/requests.[23] The coefficients α and β capture the correlation between farmers' initial asking prices and the average Accra prices in the corresponding month, respectively for the control and treatment groups. As treated farmers are receiving the price alerts, we might expect their price offers to be more closely related to these: in this case, we would expect $\beta > \alpha$. However, the correlations captured by α and β may potentially reflect the seasonality of prices. To avoid this, we slightly modify Equation 2 and estimate:

$$p_{ijt}^{ASK} = \alpha AccraPriceShock + \beta T \cdot AccraPriceShock + w_k + w_t + e_{ijt}, \tag{3}$$

where $AccraPriceShock$ captures the novel information contained in the price alerts, on top of what an experienced farmer could guess based on seasonality patterns and past price trends. To construct such measure of price shocks, we regress the Accra price series on a monthly time trend (linear) and monthly fixed effects, and use the results of this regression to estimate a "predicted" Accra price. The deviation between the observed monthly average price in Accra and this "predicted" price is $AccraPriceShock$.

The estimates based on Equations 1, 2 and 3 are presented in Section 5. They offer pretty clear evidence of the short-run impact of the price alerts on the treated farmers, and the mechanisms which could lead to such effects. In order to better understand the longer run dynamics, we conduct an in-depth analysis of the spillovers accruing to the control group: the method is outlined in the next section, and the results are presented in Section 5.1.

4.1 Analysis of Spillovers

In our analysis of the longer-run effects of price alerts, we first of all ask whether the price alerts led also to any spillover effects on the control group farmers. Our approach in this phase is guided by the assumption that, for control farmers, the size of the indirect benefits should be correlated with

[23]Specifically, in the annual surveys we asked farmers to recall the details of an important transaction from the previous agricultural season, including the month and place of sale, quantity sold, whether bargaining took place, the farmer's initial price offer, the price he expected to receive, and the final sale price. Yam farmers almost always make the first price offer in negotiations with traders.

the intensity of their connections to treatment group farmers. Furthermore, we expect spillovers to increase over time. Recall that our index of connectedness (c_{jk}) or "network ties" capture both geographical proximity and the amount of communication and interaction across any given pair of communities (details discussed in discussed in Section 2.1 and Appendix B). We now use this index to compute a measure of average network ties to the treatment group. For each community, we define its average connection index to the treated group ($C2T$ in short) as the average of its connections to each community in the treatment group.[24] Communities with weaker connections to (other) treated communities have a C2T measure that is closer to zero, while those with stronger network connections to treated communities have a C2T measure that is closer to one. In order to look for evidence of indirect benefits for the control group, we examine the relationship between the connectedness-to-treatment-group index (C2T) and prices over time, for farmers in the control group. After calculating the C2T measure for all villages (treatment and control), we run the following regression on the monthly sales data:[25]

$$\ln p_{ijt} = \sum_{s=0}^{2} \{\delta_s Y_s + \alpha_s (T_j * Y_s) + \beta_s (C_j * \mathrm{C2T}_j * Y_s) + \gamma_s (T_j * \mathrm{C2T}_j * Y_s)\} + X_{ij}' \psi + \omega_k + \omega_t + e_{ijt} \quad (4)$$

The outcome of interest, $\ln p_{ijt}$, is the natural logarithm of the price received by farmer i, in community j, in month t. We want to estimate the impact of C2T over time, for each type of farmer (treatment and control), so we interact C2T with indicator variables for treatment status (T_j for treatment and C_j for control) and a set of time indicators Y_s, $s \in \{0, 1, 2\}$ (pre-treatment, Year 1, and Year 2). As before, the regression includes strata fixed effects (ω_k) and time period fixed effects (ω_t), as well as other covariates (X_{ij}). The main coefficients of interest are β_s and γ_s, which respectively capture the impact of C2T at each time period for farmers in the control and treatment groups. Using a difference-in-difference approach, we focus on the evolution of their

[24] Focusing on any given community j, let $\{c_{jk}\}_{k \neq j}$ be the array containing the connection indexes between community j and all other communities k in the study. Community $j's$ average-connection-to-the-treatment group index (which we abbreviate as $\mathrm{C2T}_j$) is then defined as the simple arithmetic mean of all connection scores c_{jk}, such that community k is in the treatment group (should community j be treated, we do not consider its connection to itself, of course). This index is then rescaled to lie between zero and one.

[25] The literature typically takes one of two approaches to measuring spillover effects. The first approach varies treatment density, either within the randomization unit (community) or across broader geographic areas in the study area. The second approach looks at pre-existing network ties between control units and treatment units. Given our small study area, we choose to follow the second approach.

difference ($\beta_s - \gamma_s$) over time.[26] The double difference approach allows us to isolate the change in the impact of connections to the treatment group ($C2T$) for the control groups (captured by β_s), net of the change observed in the same time frame for treated farmers (captured by γ_s).

After establishing the presence of positive price spillovers for the control group farmers, we try to learn more about the mechanism which best explains them. We therefore focus on the evolution over time of the difference in quality of price information held, between treated and control farmers. Should the positive spillovers be due to a treatment contamination, where treated farmers are systematically sharing the price information with farmers in the control group, we should expect the differences in quality of information held to fade over time. We test this by estimating the following equation:

$$\text{guess error}_{ij} = \kappa T_j + X'_{ij}\psi + \omega_k + e_{ij}, \tag{5}$$

where guess error$_{ij}$ is a measure of how well informed farmer i, from community j, is about yam prices in Accra. In the endline survey, in Year 2, we asked all yam farmers to give us their best estimate of current (i.e. at the time of the survey) prices for yam in Accra: guess error$_{ijt}$ is the difference between farmer i's guess and the actual price, in absolute value. Farmer's characteristics (X'_{ij}) and strata fixed effects (ω_k) are included. Should farmers in the control group gain indirect access to price information, we expect coefficient κ to be zero. For robustness, we also re-estimate Equation 5 in logarithmic form.

In addition, we investigate the association between the guessing error and a farmer's connection to the treated communities through the following equation:

$$\ln \text{guess error}_{ij} = \kappa T_j + \beta_s(C_j * \text{C2T}_j) + \gamma_s(T_j * \text{C2T}_j) + X'_{ij}\psi + \omega_k + e_{ij}, \tag{6}$$

For information sharing to fully explain the spillovers, $C2T$'s effect on the guessing error made by

[26] The reason we rely on difference in difference, rather than simply considering the evolution of the effect of $C2T$ on control farmers over time, is that $C2T$ is not an exogenous variable. For example, C2T may be associated with village attributes that are unrelated to our intervention but positively affect market prices (better access to markets and traders, or better access to information from non-Esoko sources). If this is the case, an increase in the estimated β_s over time might not reflect the effect of increasing positive spillovers associated with the intervention, but rather other spurious effects. By using the change in α_s over time as a benchmark, we mitigate the risk of capturing such spurious mechanisms.

control farmers (captured by β_s in Equation 6) should exhibit an opposite dynamic to its effect on prices received by control farmers (β_s in Equation 4).

Finally, in order to get consistent estimates of the average impact of price alerts in the presence of spillovers, we make two additional assumptions: (1) that the relationship between network ties to treated communities and the size of the accruing spillovers is linear; and (2) that there is no spillover on those control group communities having no network ties (as we measure them) to the treated ones. Given these assumptions, the de-biased treatment effect can be recovered comparing the estimates of Equations (1) and (4), which we repeat here for convenience:

$$\begin{aligned} p_{ijt} &= \sum_{s=0}^{2} \{\lambda_s Y_s + \kappa_s (T_j * Y_s)\} + X'_{ij}\psi + \omega_k + \omega_t + e_{ijt} \\ \ln p_{ijt} &= \sum_{s=0}^{2} \{\delta_s Y_s + \alpha_s (T_j * Y_s) + \beta_s (C_j * \mathrm{C2T}_j * Y_s) + \gamma_s (T_j * \mathrm{C2T}_j * Y_s)\} + X'_{ij}\psi + \omega_k + \omega_t + e_{ijt} \end{aligned}$$

The two equations are similar, except the latter includes the interaction of the treatment status dummies (C_j and T_j, for control and treatment groups) with the network ties index $C2T_j$. In this equation, δ_s captures $E[p_{ijt}|T_j = 0, C2T_j = 0]$ in period s, the mean prices for a hypothetical pure control farmer with no network ties ($C2T_j$ index equal to zero) to the treated communities. Given our two assumptions, such farmer would be unaffected by spillover: in other words, δ_s is a measure of mean prices for the hypothetical pure control group unaffected by spillovers. To de-bias κ_s, we need to net out the average spillover effect on the control group, which is equal to the difference between the observed average price in the control group (λ_s) in equation (**??**) and the estimated pure control average (δ_s) in equation (4). The unbiased treatment effect is then equal to $\kappa_s + (\lambda_s - \delta_s)$, i.e. the biased treatment effect adjusted for the impact of spillovers on the control group.[27]

[27] We can also use this approach to estimate the average spillover effect on the treatment group. This is equal to the de-biased treatment effect minus α_s, which is the treatment effect for a farmer with a C2T score of zero (which, by our assumption, implies that the farmer is completely unaffected by spillover effects associated with the intervention). Our estimation of the average spillovers and de-biased treatment effect is similar in spirit to the techniques developed by Baird et al. (2014) and Miguel and Kremer (2004).

5 Results

In this section, we present the estimates for all four phases of the analysis detailed in Section 4.[28] Columns (1) and (2) in Table IV present results from the estimation of Equation (1) using the monthly sales data for yam, without and with additional farmer's characteristics. The top panel shows results using price levels (yam prices per 100 tubers, denominated in real August 2011 Ghana Cedis), and the bottom panel shows results using log prices. In interpreting these results, it should be kept in mind that if any violation of SUTVA occurs because of spillovers, our estimates would be a biased assessment of the treatment impact. Furthermore, the bias would likely be larger in the longer-run, because it probably takes some time for spillovers to occur.

[INSERT TABLE IV HERE]

The estimated pre-intervention coefficient (κ_0) is mostly small in size and never statistically significant: this confirms balance at baseline. For the shorter-run, we estimate a positive and statistically significant treatment effect of about 8.73 Ghana Cedis per 100 tubers, or, from the log specification, a 5.0% increase in prices (coefficient κ_1). In contrast, the longer-run impact is not statistically significant (coefficient κ_2). This may indicate that the intervention only led to temporary price increases, or that the longer run estimates are biased by the presence of spillovers: we come back to this point in detail in Section 5.1.

For comparison, we also report the estimates for the other crops grown by farmers in our study, in aggregate and separately (Columns 3-7). It should be noted that farmers in the area grow several crops: the differences across columns hence reflect differences in impact across crops, rather than across farmers. In general, we estimate a null impact on all crops, with the exception of raw cassava, which shows a positive impact, but only in the log-prices specification. As raw cassava is often sold to local processors, the nature of bargaining for this good might be substantially different.[29] The

[28]In mid-2012, we discovered that the surveyors in the Nkwanta North district were falsifying some of the data in the monthly surveys. Rather than go back and have the work redone in retrospect, we decided to simply discard the suspect data. Thus, the monthly data relied on in this paper does not contain information for Nkwanta North from August 2011 through June 2012. Given our stratified sampling approach, the omission of this data should not distort any of results, although it does reduce sample size which may lead to greater imprecision in some of our estimates. Results using the annual data (where we did not have to drop any data) are comparable, and are available upon request.

[29]Yam and cassava differ in many dimensions. First, while yam is mostly sold unprocessed, cassava is often sold in processed form. In our sample, at baseline, nearly 30% reported selling in raw form only, 53% reported selling in processed forms only, and 18% reported selling in both forms. The option to convert raw cassava to processed foodstuffs changes the outside option available to farmers in their negotiations with buyers. Also, as mentioned above, buyers of raw cassava in our study area are generally *not* middlemen, but local processors that buy tubers to

broader takeaway from these results is that the impact of the intervention appears to be highly dependent on the marketing environment of the crops. The intervention had the biggest effect on the crop with the largest price dispersion and with the greatest prevalence of farmer-trader bargaining (yam). This could be because the bargaining channel was the only viable mechanism through which the intervention could operate, or because yam markets are less well-integrated than those of other crops in our study. Either way, in terms of external validity, the results highlight the importance of understanding the local market context in order to ascertain whether price alert systems can be used to improve farmers' price outcomes.[30]

Because of these considerations, in the reminder of the paper we mostly focus our attention on yam, and leverage data on the other crops to support our main findings. Since yam is by far the most prevalent crop in the area, we still have plenty of power for our tests.

How does the intervention lead farmers to obtain higher prices from traders? As detailed in the Appendix, we do not find any evidence of an impact of price alerts on quantities sold, size of land cultivated or timing of sales (Tables A1, A2 and A3 and Figure A2 in Appendix A). In particular, even though they receive higher prices for yams but not for other crops, treated farmers do not shift more land cultivated from other crops to yam. It is possible that low access to savings, credit and farming technologies might prevent farmers to implement such changes, at least within the limited time horizon covered in our study. This is also consistent with anecdotal evidence gathered through our conversations with farmers in the study.[31] The one change we observe (Table A4, Appendix A), is that some farmers in treated communities switch from selling at home to selling at the farmgate:

make gari and dough. More than 90% of all cassava sales recorded in our monthly surveys are to local processors, and less than 10% are to middlemen traders. The bargaining dynamics between farmers and local cassava processors are arguably different from those with large-scale itinerant traders: as a result, price information might prove less empowering. Indeed, our estimates suggest an increase of around 9% in prices in the first year of treatment for raw cassava, but this only statistically significant when estimated using log-prices. We interpret this fact as suggesting that the estimated impact or cassava is highly sensitivity to outliers or non-linearities.

[30]Nakasone (2013), Aker and Fafchamps (2014), and Jensen (2010) emphasize that price information is likely to have a larger effect on more perishable commodities, since perishability limits the ability of market actors to use storage strategies to respond to supply and demand shocks. This increases the volatility of aggregate prices, and limits farmers' ability to hold back sales to traders at low prices. While yams are storable, they are more perishable than processed maize or gari, or even raw cassava (which can be left in the ground for extended periods of time, and then uprooted at the time of sale). Thus, our results are broadly in line with the existing literature showing larger impacts of price information on more perishable commodities.

[31]The fact that market frictions prevent profitable innovation is well established in the micro-development literature. In the context of Ghana, evidence exists that technology adoption is costly and slow and that financial constraints affect production choices (Karlan et al. (2012)). In additional analysis, not included, we look for evidence of spillover effects on the total amount of yams and other crops harvested by farmers in control communities, and find very small and not significant changes. These additional results are available from the authors on request.

this finding is consistent with the idea that price information gives farmers the "confidence" to sell at the farm-gate (Fafchamps and Minten, 2012).[32] Price information seems to be leveraged to ask middlemen for fairer prices, rather than to inform changes in production or marketing behavior: in the first harvest season since the beginning of treatment, 66% of farmers explicitly declare using price information (gathered from a variety of sources including, for the treatment group, the price alerts) to bargain with traders.

A closer look at the bargaining dynamics between farmers and traders offers support to the hypothesis that bargaining is key in converting price information into higher prices and revenues. Panel A of Table V reports the estimates based on Equation 2.

[INSERT TABLE V HERE]

The coefficient α corresponds to the correlation between control group farmers' initial asking prices and average Accra prices in the corresponding month. In the first column, this correlation is estimated by including only the randomization strata fixed effects and the year-treatment group fixed effects, while in the second column we additionally control for quantity of yam sold (linear and quadratic) and place of sale (home/farm gate/local market/urban market). Similarly, the coefficient β captures the analogous correlation, for farmers in the treatment group. The results indicate a positive and statistically significant relationship between Accra prices and asking prices for both treatment and control groups. The positive sign for the interaction term β between Accra yam prices and the treatment dummy suggests a stronger relationship for treatment farmers, but this result is not statistically significant. The results of Panel A suggest that even control group farmers have some idea of price trends in the urban market, but this should not come as a surprise, given their long experience and the presence of seasonality in price time series.

As discussed in Section 4.1, a more appropriate test is to look at how farmers' initial price offers relate to price deviations from what an uninformed, but reasonably experienced, farmer might expect based on past experience. To this end, Panel B reports estimates based on Equation 3 , which give the correlation between farmers' initial asking prices and the shocks to Accra price. The

[32] Nonetheless, before the introduction of price alerts, we find no statistically significant evidence that sales at the farmgate may be associated with higher prices. It should be noted that the intervention could also plausibly affect farmers' marketing decisions along the quality dimension (e.g. selling smaller or less fresh tubers when prices are lower). To ensure consistency, our analysis focuses on the "best grade" of yam. This is however a relative and not particularly precise measure of quality. As no standardized quality grades of yam exist in the marketplace, we are unable to further support or reject the hypothesis of a change in quality.

idea is that such shocks capture the informative portion of the price alerts, net of the seasonality patterns that farmers could easily guess without price alerts. The resulting estimates (in Panel B of Table V) reveal a striking difference between treatment and control farmers. Namely, the association between Accra price deviations and farmers' initial asking prices is not significant for farmers in the control group (coefficient α), but it is positive, sizable and statistically significant for the treatment group (coefficient β). We take this as strong evidence that the alerts mainly led to increases in prices due to their impact on farmers' bargaining behaviors with traders.[33]

We are now ready to explore the evolution of the treatment impact on prices over time. Recall that if the assumption of zero spillovers is met, the estimates in Table IV can be interpreted as treatment effects on prices. They would then suggest an initial positive treatment effect of the alerts on the prices received by farmers for yam, and a null impact in the longer run. To explore this further, in the top panel of Figure I we reproduce the average prices received by farmers in the treatment and control groups, by month. The average prices, adjusted for strata, type of yam and the usual baseline covariates, are non-parametrically smoothed (via Fan regressions).[34] The bottom panel of Figure I plots the (smoothed) difference between the average prices in the treatment and control groups and the corresponding bootstrapped 95% confidence interval. Under the SUTVA assumption, these monthly differences can be interpreted as (a smoothed equivalent of) the monthly treatment effects on yam prices.

[INSERT FIGURE I HERE]

In the first months after the introduction of the price alerts, there was a large difference between treatment and control group prices for yam: over 20 GHS per 100 tubers, more than twice the estimated average treatment effect over the entire first year, shown in Table IV. The difference

[33] This is consistent with what farmers reported in the annual surveys: in the first marketing season after the beginning of the intervention, 68% of farmers receiving the alerts reported using them to bargain with buyers, compared to 38% reporting using the information to decide where to sell, 22% to decide when to sell, and 11% to make production decisions. An alternative way to test our hypothesis that the impact of price information is linked to bargaining is to pool together the data for the sales of all crops during the first year of treatment and estimate an extension to our basic model, where an interaction term is added between the treatment indicator and the (crop-specific) share of farmers who engaged in bargaining at the time of our baseline survey, along with the un-interacted variables. The ordinary least squares estimates for this extended model, available from the authors upon request, suggest that the treatment impact is indeed increasing with the prevalence of bargaining: the coefficient for treatment is not statistically significantly different from zero, while the coefficient for the interaction term is positive and statistically significant.

[34] A Fan regression is a local linear regression method that enables the econometrician to estimate a more flexible (non-linear) relationship between the regressor and the outcome of interest. For more detail on Fan regression methods, see Fan and Gijbels (1996) and Dinardo and Tobias (2001).

steadily declines over time and five to six months after the start of the intervention it is no longer significantly different from zero. Thus, the treatment effect estimated under the zero spillovers assumption gradually declines and eventually disappears.[35]

There are several alternative explanations why the treatment effect on yam prices may fade in the longer run. For example, farmers may stop paying attention to the alerts, causing their prices to converge to those received by the control group. It is also possible that traders may stop transacting—or threaten to stop transacting—with treatment group farmers: the resulting unfavorable marketing environment may drive down the prices received by these farmers. Our data are inconsistent with either of these two hypothesis. Both in the short and longer-run, farmers in the treated group are significantly better informed about urban market prices than those in the control group: this suggests that they still pay attention to the price alerts (evidence will be presented in Section 5.1). In addition, they do not report experiencing a reduction in volumes sold or in trading partners, as we would expect under the second hypothesis (results available on request). Based on our empirical evidence, the most convincing explanation is that indirect benefits occurred to certain control group farmers, causing the average prices received by farmers in the control group to converge upward to those of the treatment group. The next subsection provides empirical evidence of the size and nature of such spillovers.

5.1 Evidence of indirect benefits for control group farmers

In this section, we present evidence that some control group farmers indirectly benefited from the intervention, we explore the nature of such spillovers and finally present spillovers-adjusted estimates of the longer run impact of price alerts on yam prices. As discussed in Section 4.1, our empirical measure of spillovers is the correlation between the prices received by farmers in the control group and their average connection to farmers in the treated communities, which for each month are captured by the coefficients β_s in Equation 4. In a difference-in-difference spirit, we compare such correlations to those of farmers in the treatment group, captured by γ_s. The resulting estimates are reported in Table VI and summarized graphically in Figure II.

[35]The pattern displayed in the bottom panel of the figure suggests that the disappearance of the treatment effect is not a consequence of seasonality in the yam marketing season. A similar figure for cassava is reproduced in the Appendix (Figure A1) and reveals a different pattern. The average cassava prices for treated and untreated farmers are mostly very close, except for a very short period around March 2012 and for the last semester of our study, when however the differences between the two groups appear to be largely driven by noise.

[INSERT TABLE VI HERE]

The top panel Table VI shows the estimated β_s and γ_s coefficients for each agricultural season, along with the 95% confidence intervals, and the bottom panel their difference $(\beta_s - \gamma_s)$. The estimated β_s coefficients exhibit a strong upward trend, easily visible in Figure II: zero at baseline and in the first year, and positive and statistically significant in Year 2. In other words, at baseline and in Year 1 the average connection to treated communities has no impact on the prices received by farmers in the control group. In Year 2, the impact is positive and significantly different from zero at the 1% level. If this trend were due to some general change in the marketing environment, it should also be found for farmers in the treatment group. This is not the case: the estimated γ_s for the treatment group are null in each year. As a result, the difference-in-difference estimator $\beta_s - \gamma_s$ also has an upward trend.

[INSERT FIGURE II HERE]

The data on other crops offer further evidence that we can interpret the growing impact of connections to treated communities (our C2T index) on prices received by control farmers as evidence of intervention spillovers. Recall that the intervention had no impact on the prices of the other crops, and that these crops are grown in the same small communities as yams, and in many cases even by the same farmers. If what we capture is truly a spillover of the intervention, it should not be found for the prices of other crops. In Figure III we reproduce the estimated coefficients $beta_s$ and γ_s for yams (as in Figure II) along with those for other crops.[36] As anticipated, the estimated C2T coefficients for non-yam crops are never significantly different from zero and do not show an upward trend over time. As a result, the differential impact of C2T on control farmers for yam prices versus non-yam prices is upward sloping and in Year 2 it is positive and significantly different from zero.

[INSERT FIGURE III HERE]

Taken as a whole, the results suggest that, over time, the intervention created positive spillover effects for farmers in the control communities, and these effects are increasing in the degree of connection that they have to treated communities. To better understand the nature of the spillovers, we check whether over time farmers in the control group, and especially those with stronger con-

[36] To produce this figure, we include crop-strata and crop-period fixed effects to our regression based on Equation 4.

nections to treated communities, gain price information. To this end, we estimate Equations 5 and 6 and report the estimates in Table VII: the first two columns use the absolute size of the guessing error, while the third and fourth use percentage errors.

[INSERT TABLE VII HERE]

Columns (1) and (3) in Table VII suggest that, in Year 2, farmers in the treated communities are significantly better informed than those in the control group. In addition, Columns (2) and (4) reveal no association between a farmer's connection to the treated communities and his/her ability to guess the correct price.[37] As a result, we conclude that the spillovers cannot be fully explained by information sharing.[38] Rather, we believe that the spillovers may be driven by an overall re-adjustment of the bargaining strategies used by middlemen, in response to the intervention. To formalize the intuition of how this may happen, Section 6 presents a theoretical model of *bargaining spillovers.*

The presence of spillovers in the second year of the study also means that the control group is not a suitable counterfactual for the evaluation of the longer run effects of the intervention. In Year 2, the presence of positive spillovers lead us to overestimate the counterfactual price levels and, consequently, to underestimate the impact of the intervention on treated farmers. Table VIII shows our approach to correct for this bias: we sum the average spillovers, obtained in Table VI using Equation 4, to the original estimate of the treatment effect, obtained in Table IV using Equation 1.[39] The top panel presents results using price levels, and the bottom panel using price logs, for each time period. In terms of price levels, the de-biased treatment effect is estimated to be 16.16

[37]Our surveys collect additional measures of farmers' knowledge of market prices: we ask farmers how well informed they feel and for how many localk and urban markets they regularly know the prices. Re-estimating Equations 5 and 6 using these measures as outcome in lieu of the guessing error yields similar results. In particular, we observe that control farmers with strong ties to treated communities gain higher prices, but do not feel better informed (Table A5)). We also find that the estimated impact of the connections on prices for control farmers is statistically the same whether we control for quality of information held (measured via guessing errors or self-reported) or not (Table A6). All estimates shown in Appendix A.2. Note that our analysis does not rely on a causal interpretation of the impact of treatment status on price knowledge.

[38]We are not fully surprised by this: after all, our randomization strategy was carefully designed to limit the scope for information sharing across communities clusters. As described above, detailed information about ties and communications with other communities was collected. Based on this information, communities with strong connections were clustered together. The unit of randomization is the community cluster, rather than individual farmers or communities. Furthermore, similar results have been suggested in the literature. Courtois and Subervie (2014) find some suggestive evidence of information sharing across villages, but the extent of this information sharing is quite limited. Nakasone (2013) fails to detect information sharing even among farmers living in the same community.

[39]We estimate the two equations using two-step GMM so that we can conduct significance testing on our estimates of average spillover effects and the de-biased treatment effect.

GHS in Year 1, and 14.32 GHS in Year 2, both of which are significant at the 5% level.[40] The log results are 7.8% and 9.4% in Year 1 and Year 2, respectively, although only the Year 2 result is statistically significant. These estimates are substantially higher than the biased treatment effect estimates presented in Table IV, due to large positive spillovers on control group farmers: in Year 2, we estimate the average spillovers on control prices to be 14.71 GHS per 100 tubers, or a 10.4% increase in prices. When spillovers are appropriately accounted for, the intervention is shown to have a large impact, not only on treatment farmers, but also on control farmers that benefit from network externalities.

[INSERT TABLE VIII HERE]

6 A Model of Bargaining Spillovers

The results discussed above offer evidence of strong and lasting benefits to treatment group farmers, which we claim were driven by farmers' use of price information in bargaining with middlemen. In addition, in the second year of the study, we document large spillovers to specific farmers in the control group. As these farmers are not better informed than those who do not receive spillovers, we claim that the spillovers cannot be completely explained by information sharing. Such spillovers may originate from other forms of network externalities or from general equilibrium effects. In our setting, we believe the spillovers to be driven by the fact that these farmers largely interact with the same middlemen as the treated farmers. If the intervention induces middlemen to adopt a less aggressive bargaining behavior, it may benefit all farmers with whom they interact, irrespective of whether these farmers receive the price alerts.

There are various ways in which such network externalities can take place. For example, they could result from variation in competitive pressure among traders, if traders can identify which farmers are informed about prices. As uninformed farmers accept lower price offers, traders would prefer trading with them. As more and more traders switch to trading with uninformed farmers, competitive pressure among them may increase and lead to higher prices for uninformed farmers. At the same time, informed farmers would be visited by fewer traders and may have to accept lower prices. However, as we discuss below, it is highly unlikely that traders can identify which farmers

[40] Note that these figures are on-par with the estimated treatment effect in the first few months of the intervention, as shown in Figure I.

are informed. Furthermore, we do not have any anecdotal or empirical evidence of a decrease in the number of traders visiting treated communities, or in their volumes of sales. Alternatively, externalities may be driven by competitive pressure among informed and uninformed farmers in the local markets where both types of farmers sell. As a result of competition, we may expect a positive correlation between the prices received by the two groups of farmers in such markets. As a result, prices may be higher in markets with high shares of informed farmers. Note that in such markets everyone will tend to have stronger ties to treated communities, as captured by our connectedness measure. While this would explain why uninformed farmers with strong ties to treated communities receive higher prices, it would contradict our empirical finding that such ties have no impact on the prices for informed farmers (Table VI).

We posit an alternative form of network externality, which fits our empirical finding best. Our explanation hinges on the hypothesis that the treatment status of a farmers is ambiguous from the point of view of the trader. Facing this ambiguity, traders offer better prices to control farmers that are well connected to the treatment because they are *believed* to be informed, irrespective of whether they indeed are. Before presenting the formal model (in Section 6.1), let us outline the narrative it captures. Some months into the intervention, it becomes apparent to the traders that some farmers are informed about the urban prices and that they have become tougher bargainers due to their increased outside option.[41] While traders cannot perfectly distinguish the informed from the uninformed farmers, they realize that there are areas in the network where bargaining has generally become more difficult for them. In an effort to optimize their bargaining strategy, traders then form beliefs about the probability that a farmer is informed based on his proximity to areas where bargaining has become tougher. We assume that this belief is well proxied by our connectedness index. Our model shows that in equilibrium, when the probability that a farmer is informed is large enough, the trader will treat the farmer *as if* he is informed with certainty. Hence, control farmers with strong connections to the treated areas will receive better selling conditions (i.e. better prices) than their less connected peers even if there is no real difference in how informed they are. This is in line with the positive impact of connectedness-to-treated communities estimated for control group farmers in Columns 2 and 4 of Table VI. On the other hand, the model does

[41] The outside option consists of selling to other middlemen, or directly at urban markets. In particular, timely price information makes it easier and more attractive for farmers to sell at urban markets. Indeed, in Year 1 of the study a non negligible number of farmers, mostly form the treatment group, sold yams at urban markets.

not predict any effect of connectedness-to-treated communities on the prices received by treatment group farmers, in line with the statistically null impact found for this group in Columns 2 and 4 of Table VI. In this sense, this model matches the data better than any other competing mechanism we considered. Our model also offers an additional prediction, which, as reported below, finds empirical support in our data.

6.1 Formal model

Our model of bargaining spillovers is an adaptation of the Myerson (1984) bargaining model to a multi-period and multi-type framework. The game is in discrete time. The economy is populated by N infinitely lived farmers and N one-period lived traders. Each farmer has one unit of crop for sale and discounts the future by a factor β.[42] Within a period, all traders have the same resale value v, which is an *iid* draw from a uniform distribution with support $[v_L, v_H]$, and with $f(v)$ and $F(v)$ denoting the density function and cumulative distribution function, respectively. The resale value v represents the price that traders can receive for reselling the crop in the urban market, net of transport costs, if they are successful in purchasing the crop from a farmer. All agents are risk neutral.[43]

We will think of a period as representing a week within one season. Each period, the urban market price v is an i.i.d. draw from the distribution mentioned above. In each period, every farmer that has not yet sold his one unit of crop is randomly matched to a trader. As in Myerson (1984), with probability $w \in (0, 1)$ the farmer makes a take-it-or-leave-it offer p that the trader can either accept or reject. With probability $(1 - w)$ the trader makes a take-it-or-leave-it offer that the farmer can either accept or reject. Here, w and $(1 - w)$ capture the bargaining power of farmers and traders, respectively. If the offer is accepted by the respondent, the trader's utility is $(v - p)$, while the farmer's utility is p. If the offer is rejected, the trader receives utility 0 and the farmer keeps the crop, moves to the next period, and is matched with a new trader.[44] There are

[42] The assumption that farmers are infinitely lived while traders live only for one period captures a fundamental difference between farmers and traders. Farmers have a fixed supply of harvest to sell. Hence they compare the current price with the continuation value of waiting and selling in the future. Instead traders do not buy in fixed amounts. They treat each bargaining session in isolation and in each they compare the price with the resale value of the commodity in the urban market. In this sense they are modeled as short-lived. Alternatively they can be thought of as infinitely lived, but with a continuation value that does not depend on the outcome of the bargaining session currently under consideration.

[43] Adding risk aversion does not alter any of the results in a substantial way.

[44] In our setting, it is reasonable to assume that farmer's can wait to sell crops in the future. Yams can be stored

two types of farmers: informed farmers (I) know the value v, while uninformed farmers (U) only know the distribution from which it is drawn. Since farmers are infinitely lived, their reservation value for selling in the current period is equal to their discounted continuation value of waiting to sell sometime in the future. We define R^I to be the discounted continuation value of the informed farmer, and R^U to be the discounted continuation value for the uninformed farmer.

A crucial assumption is that, ex-ante, traders do not know farmers' types with certainty. Instead, they have a belief that farmers in village i are informed with probability $d_i \in (0, 1)$. We believe this assumption fairly represents our environment since (i) most yam sales are made to traders with whom farmers have never transacted; (ii) farmers' initial asking prices do not fully reveal their underlying information set; and (iii) most farmers do not show the Esoko price alerts to traders during negotiations.[45] For tractability, we assume that d_i attached to a given farmer i is common knowledge to all farmers and traders in the game. In the remainder of the section we write d_i as d to ease the notation. In what follows, we characterize optimal strategies for each type of player and the resulting equilibrium.

Optimal strategies

Trader's strategy When acting as the responder, the trader accepts a price offer p if and only if $v - p \geq 0$.[46] When acting as the proposer, the trader does not know whether she is facing an informed farmer or an uninformed farmer and attributes probability d to the event that the farmer is informed. Since an informed farmer can mimic any strategy of the uninformed farmer when acting as proposer, it must be the case that, in any equilibrium, the discounted continuation value of the informed farmer is higher than the discounted continuation value of the uninformed farmer (it is in fact *strictly* higher as proved in Appendix C.2 for more detail). Given this result, when the

for months after harvest and numerous traders visit the village over the course of the season. In Table B8 of Online Appendix B, we show that even before our intervention yam farmers would sometimes walk away from a negotiation when the terms were not good enough for them.

[45] Regarding (i), recall from Table I that around 55% of yams were sold to traders that farmers had never met before. Support for (ii) is given in Online Appendix B, Figure B1, which plots the distribution of farmers' initial asking prices for yam in their negotiations with traders. Although the two distributions are statistically different, there is substantial overlap between the two and it would be difficult for a trader to determine whether a farmer is informed based on a single draw from the initial offer distribution. Finally, with regard to (iii), some farmers reported showing the alerts to traders to "prove" their knowledge of urban market prices. But many more farmers reported *not* sharing the alerts, either because they didn't feel it was necessary or because it was inconvenient (didn't bring phone to market, battery was dead). In one case, a farmer reported not showing the alert to the trader for fear that she would use the information to find cheaper markets in which to source yams.

[46] Issues around behavior when $v = p$ are irrelevant as in our model this occurs with zero probability.

trader is the proposer, she chooses to offer either: (1) $p = R^I$, which all farmers accept, resulting in a pooling equilibrium; (2) $p = R^U$, which only uninformed farmers accept, resulting in a separating equilibrium; or (3) in the case when $v < R^U$, no farmer will choose to trade, so the trader can offer any $p \in [0, v]$ which is rejected by all farmers.[47] If $v \geq R^U$, the trader will choose the pooling equilibrium whenever:

$$(1-d)(v - R^U) \leq v - R^I. \tag{7}$$

The trade-off is between offering a lower price, R^U, which is only accepted by a fraction $(1-d)$ of farmers, and offering a higher price, R^I, which is accepted by all farmers.

We now define:

$$M\left(R^U, d, R^I\right) \equiv R^U + \frac{R^I - R^U}{d}$$

and rewrite (7) as:

$$v \geq M\left(R^U, d, R^I\right). \tag{8}$$

The trader implements a pooling strategy whenever (8) is met. Hence we can define:

$$V^{pooling} \equiv \{v \in [M, v_H]\}$$

$$V^{Separating} \equiv \left\{v \in [R^U, M)\right\}.$$

Pooling and separating equilibria occur in the eponymous sets, respectively.[48] Thus, ex-ante, the probability that a trader implements the pooling strategy, $Pr(V^{Pooling})$, is $1 - F(M)$, where F represents the cumulative distribution function of v.

Informed farmer's strategy The informed farmer knows the value of the crop in the urban market, v. When acting as the proposer, she can extract all of the gains from trade by offering the trader a price $p = v$, which the trader will accept. If v is low—in particular, if it is lower than his continuation value R^I—he can defer the sale of the crop to the future by offering a price $p = R^I > v$, which the trader will reject. Hence, for the informed farmer, when acting as proposer,

[47] No other strategy is ever optimal: any offer $p \in (R^U, R^I)$ is only accepted by uninformed farmers and delivers strictly smaller payoffs than R^U. Any price $p > R^I$ is accepted by all farmers and delivers strictly smaller payoffs than offering R^I.

[48] Note that these sets may be empty (e.g., if $M > v_H$). Note also that we ignore issues around the selection of the equilibrium at the equality $v = M$ as this occurs with zero probability.

the optimal offer strategy is:

$$p^I(v) = max\left\{v, R^I\right\}.$$

As the responder, the informed farmer accepts any price $p \geq R^I$.

We can now compute the discounted continuation value of the informed farmer. Suppose a sale does not take place in the current period. In the next period, with probability w the informed farmer will be the proposer and will receive $E_v[max\{v, R^I\}]$. With probability $(1-w)$ she will be the responder and get R^I. Hence, the discounted continuation value has to obey the following Bellman equation:

$$R^I = \beta\left[wE_v[max\{v, R^I\}] + (1-w)R^I\right]. \tag{9}$$

In Appendix C.1 we show that there is always a unique value of R^I which satisfies (9). Moreover, this value is not a function of d, as (9) is independent of d. Depending on the primitives of the model, the equilibrium value R^{I*} can be greater than, less than, or equal to v_L. If it is greater, there is a positive probability that the trader rejects the informed farmer's offer, whereas if it is smaller the offer of the informed farmer is always accepted by the trader. Appendix C.1 derives the condition for each type of equilibrium to occur. For the remainder of the paper, we focus on the case where $R^{I*} > v_L$, which occurs when $\Phi E_v[v] > v_L$, where $\Phi \equiv \frac{\beta w}{\beta w+(1-\beta)}$.[49]

Uninformed farmer's strategy First consider the case of an uninformed farmer acting as proposer. The uninformed farmer does not know v, so she cannot extract the full surplus. Instead, the farmers chooses a price p and knows that the trader will accept it whenever $v > p$ (in which case the farmer gets p) and reject it whenever $v < p$ (in which case the farmer will get the continuation value R^U). Hence, the farmer chooses p to maximize the following equation, conditional on R^U:

$$\max_p \quad \int_p^\infty pf(v)\,dv + R^U\int_{-\infty}^p f(v)\,dv$$

The first order condition for an interior solution is $\int_p^\infty f(v)\,dv - pf(p) + R^U f(p) = 0$ and the second order condition is $-2f(p) < 0$. Because v is uniformly distributed in the interval $[v_L, v_H]$, the interior solution p^{int} is

$$p^{int} = \frac{v_H + R^U}{2}$$

[49]This choice is mainly to simplify the characterization of the equilibrium. In general, the $R^{I*} > v_L$ equilibrium occurs with higher values of β w, and/or greater price dispersion in v.

which is a valid solution whenever $R^U \geq 2v_L - v_H$. Note that the trader will only accept this offer if $p^{int} \geq v$. Alternatively, instead of the interior solution, the uninformed farmer can implement a corner solution, offering v_L, which is always accepted by the trader. It follows that the optimal strategy of the uninformed farmer is to offer:

$$p^U = \begin{cases} \frac{v_H + R^U}{2} & \text{if} \quad R^U \geq 2v_L - v_H \\ v_L & \text{if} \quad R^U < 2v_L - v_H. \end{cases} \tag{10}$$

When acting as the responder, the uninformed farmer accepts any offer $p \geq R^U$. Thus, her payoff is R^I when the trader implements a pooling strategy, and R^U otherwise.[50]

We can now write the discounted continuation value of the uninformed farmer. Let O^U represent the expected utility of being the proposer for the uninformed farmer. If a sale does not take place in the current period, then in the next period, with probability w the uninformed farmer will be the proposer and will receive O^U. With probability $(1 - w)$ he will be the responder, and will receive an expected utility that is a convex combination between R^U and R^I. Hence, the discounted continuation value of the uninformed farmer must obey the following Bellman equation:

$$R^U = \beta \left[wO^U + (1 - w) \left\{ R^U F(M) + R^I [1 - F(M)] \right\} \right] \tag{11}$$

where, again, $1 - F(M)$ represents the probability of receiving a pooling offer.

The optimal strategies for each player are summarized in Figure **??**.

[INSERT FIGURE **??** HERE]

Characterization of the equilibrium

An equilibrium for our model is characterized by a set of continuation values, R^I and R^U, which obey the two Bellman equations in (9) and (11). In Appendix C.1, we prove the existence of a unique fixed point R^I of (9). We now impose the condition that $2v_L - v_H < 0$ to avoid looking at the corner solution in the uninformed farmer's problem.[51]

[50]When $v < R^U$, the trader sets a price offer which results in no trading, so the uninformed farmer gets his discounted continuation value, which is R^U by definition.

[51]This assumption is to simplify the characterization of the equilibrium, and generally holds with greater price dispersion in v.

Proposition 1. *Assume* $\Phi E_v[v] > v_L$ *and* $2v_L - v_H < 0$*. Then there exists equilibrium values* R^{U*} *and* R^{I*} *satisfying* (9) *and* (11) *with* $R^{U*} < R^{I*}$.

Proof: *See Appendix C.3.*

The discounted continuation value for the informed farmer, R^{I*}, defined in (9), is independent of the density d; however, as the proposition below shows, the discounted continuation value of the uninformed farmer is increasing in the density d (up to an issue of equilibrium selection).

Proposition 2. *There exist values* d^{LL} *and* d^{UL} *with* $0 < d^{LL} < d^{UL} < 1$ *such that:*

(A) for d *outside of* $\left[d^{LL}, d^{UL}\right]$ *the equilibrium value* $R^{U*}(d)$ *is unique and*

(i) when $d < d^{LL}$*, the equilibrium has zero probability of pooling, and* $R^{U*}(d)$ *is constant in* d*;*

(ii) when $d > d^{UL}$*, the equilibrium has positive probability of pooling, and* $R^{U*}(d)$ *is strictly increasing in* d*;*

(B) for d *in* $\left[d^{LL}, d^{UL}\right]$ *there are three equilibria: one "corner" solution involving a zero probability of pooling, and two solutions with a positive probability of pooling. The equilibrium selection which, for every* d*, picks the largest equilibrium* R^{U*} *is strictly increasing in* d*.*

Proof: *See Appendix C.3.*

The segment $[d^{LL}, d^{UL}]$ depends on the primitives of the model. For example, when $\beta = 0.9$, $v_L = 300$, $v_H = 800$, and $w = 0.4$, $[d^{LL}, d^{UL}] = [0.2005, 0.2068]$.

Proposition 2 has an immediate implication, which is that the equilibrium probability of pooling increases with d.

Corollary 1. *As in Proposition 2, whenever there are multiple equilibria for each* $d \in \left[d^{LL}, d^{UL}\right]$ *select the equilibrium with the largest value for* R^{U*}*. Let* $\pi(d)$ *be the probability of receiving the pooling offer* ($p = R^I$) *for a farmer who is believed to be informed with probability* d*, conditional on being the respondent. Then* $\pi(d)$ *is increasing in* d*.*

Proof: *See Appendix C.4.*

Corollary 1 has empirically testable implications for observed prices for treatment and control farmers as shown in Proposition 2. Let the price functions $P^I(d)$ and $P^U(d)$ be the equilibrium expected price, conditional on sale, for informed and uninformed farmers respectively.[52] Define $\mu^I(d)$ and $\mu^U(d)$ as the probability that the trader is the proposer conditional on the agreement being reached in that period, for uninformed and informed farmers respectively.[53] The expected price conditional on sales for the two farmers can be expressed as:

$$P^I(d) = \left[1 - \mu^I(d)\right] \frac{v_H + R^{I*}}{2} + \mu^I(d) R^{I*}$$
$$P^U(d) = \left[1 - \mu^U(d)\right] \frac{v_H + R^{U*}(d)}{2} + \mu^U(d) \left\{\pi(d) R^{I*} + \left[1 - \pi(d)\right] R^{U*}(d)\right\}$$

Proposition 3. *$P^I(d)$ and $P^U(d)$ satisfy the following:*

(A) $P^I(d)$ is (weakly) decreasing in d;

(B) $P^U(d)$ is (weakly) increasing in d;

(C) The difference $P^I(d) - P^U(d)$ is (weakly) decreasing in d.

Proof: *See Appendix C.5.*

Taken together, these results show how "bargaining spillovers" emerge and are associated with a reduction in the price gap between informed and uninformed farmers (Proposition 3.C). Uninformed farmers with high values of d end up receiving higher price offers due to the traders' beliefs that they are informed with high probability (Proposition 3.B). Informed farmers, on the other hand, do not benefit from higher levels of d (Proposition 3.A).

6.2 Matching the bargaining spillovers model to the data

We now describe how our findings can be explained through the lens of the model. Recall from Section 5.1 that we find a large, positive difference between average treatment and control group yam prices in the early months of the intervention, which fades over time so that, by Year 2, the difference is no longer significant. we claim that this reduction in the difference between average treatment and control prices occurs because control farmers with stronger connections to the treatment group (as measured by our index $C2T$) benefit from positive spillovers.

[52] $P^I(d)$ and $P^U(d)$ are the weighted average of the mean price received by farmers, weighted by the probability that they are proposer/respondent in a successful bargaining round.

[53] Since rejections occur in equilibrium, $\mu^I(d)$ and $\mu^U(d)$ are different from the bargaining power weight of the trader $(1 - w)$.

Under appropriate assumptions over the evolution of traders' beliefs about farmers' informed-ness, our model can explain the observed dynamics as outlined below.

Before the intervention. Traders correctly believe that farmers have low levels of price information, e.g. $d = d^0 < d_{LL}$ for all farmers, and thus always make low (separating) offers. These offers are accepted by all uninformed farmers, which, based on our pre-intervention survey data, are the vast majority of our sample.

Short-run impact: initial months post-intervention. While treatment farmers starts receiving price information, in the initial months post-intervention, no belief updating occurs on the traders' side. Traders continue to make low offers, farmers in the treatment group reject them and, when urban market prices are high, ask for higher prices (evidence presented in Table V). Uninformed farmers accept all offers from the traders: the market is in the separating equilibrium. In this first period post-intervention we observe the largest average price difference between treatment and control farmers. For later convenience, we call this price difference Δ^{SR} defined as $\Delta^{SR} = P^I(d^0) - P^U(d^0)$, where $P^I(\cdot)$ and $P^I(\cdot)$ are defined as in the model's section and d^0 is an arbitrary low probability of farmers' informed-ness that sustains the optimality of the separating offer.

Longer-run impact: Year 2 results. As an unprecedented number of farmers start turning down their offers, traders eventually realize that their bargaining conditions have changed. They understand that there are some farmers who are now informed, however they do not clearly distinguish informed and uninformed farmers. Faced with the choice of making a separating offer that only uninformed farmers will accept, or a higher pooling offer that all farmers accept, traders need to form a new belief about the likelihood that the farmer they are bargaining with is informed: they will make a pooling offer only if such probability is high enough.[54] We assume that, even if the two types cannot be separated with certainty, traders can use some geographical cues to make their predictions more accurate than a random guess. We expect that traders have a sense that in certain areas of the network, rejections are more frequent than in others. Hence they will believe that the closer a farmer is to those areas, the more likely he is to be informed. In other words, traders will use C2T (which is based on generally observable information about a farmer's social

[54]The fact that such belief updating takes some time to occur is supported by the finding from Figure I that spillovers do not start to occur until about 3-4 months after the introduction of the intervention.

and geographical relationship with other farmers in the areas) as a proxy for d, the probability that the farmer is informed. Once traders update their beliefs about d, bargaining spillovers emerge. In particular, two equilibrium outcomes follows for which we find empirical support. First, control farmers with high connections to treated communities (corresponding to high d in the model and high C2T index in the data) start receiving better offers and better average prices than their less connected counterparts. This follows directly from Proposition 3 which shows that $P^U(d)$ is increasing in d.

Second, the model predicts that the price gaps between the treatment and control group decreases with d: in other words, that on average the control group, "catches up" as d increase, to a smaller or larger extent, for all farmers.[55] To show this formally, let Δ^{LR} be the average expected price difference received by informed and uninformed farmers in the long run, i.e. $\Delta^{LR} = \frac{1}{N/2}\sum_{j\in I} P^I(d_j) - \frac{1}{N/2}\sum_{j\in U} P^U(d_j)$. We impose one additional (arguably realistic) condition:

Condition 1: After the introduction of the MIS, the beliefs held by traders about the likelihood that a farmer is informed has weakly increased for all farmers, and $d > d^{LL}$ for some farmers.

Under this condition, it is easy to show that $\Delta^{LR} < \Delta^{SR}$:

Proposition 4. *If Condition 1 holds, the difference between the average price received by informed and uninformed farmers, conditional on sale, is lower in the longer run as compared to the short run, that is* $\Delta^{LR} < \Delta^{SR}$.

Proof. It is sufficient to prove that:

$$\Delta^{LR} - \Delta^{SR} = \frac{1}{N/2}\sum_{j\in I}\left(P^I(d_j) - P^U(d_j)\right) < P^I(d^0) - P^U(d^0) \tag{12}$$

which is equivalent to showing that:

$$\sum_{j\in I}\left\{\left(P^I(d_j) - P^I(d^0)\right) + \left(P^U(d^0) - P^U(d_j)\right)\right\} < 0 \tag{13}$$

Where both terms in parenthesis are negative, as implied by Condition 2 and Proposition 2. □

[55] In the model this is driven by the fact that (i) average control group prices ($P^U(d)$) are (weakly) increasing in d, while (ii) average treatment group prices are (weakly) decreasing in d. While we do not have sound empirical evidence for point (ii), our data support the predictions both on (i) and on the shrinking on the gap.

6.3 Further evidence in support of the bargaining model

Our model gives us an additional testable prediction which would be difficult to reconcile with a scenario where spillovers are driven exclusively by control farmers getting urban market price information from treatment farmers. The prediction is that, within the treatment group, farmers with high connections to other treated communities should sell faster in Year 2. This is because, once spillovers start arising, having stronger connections implies a higher probability of receiving a high price offer at any interaction with a trader, which they accept. Treated farmers with low connections, on the other hand, are more likely to receive low price offers, which they reject, hence delaying sales. This relationship will not hold for control farmers, since they accept both high and low price offers.[56]

To take this prediction to the data, we use the monthly sales data for the second year of the study and compute, for each farmer i, the cumulative fraction F_{ijt} of yam sold at each month t in the agricultural year.[57] We then estimate the following equation:

$$F_{ijt} = \alpha_m + \beta_1 T_j + \beta_2 C2T_j * C_j + \beta_3 C2T_j * T_j + \alpha_s + e_{ijt}, \tag{14}$$

where α_m are monthly fixed effects, and α_s are strata fixed-effects. Delays in sales should translate in lower cumulative fractions of yam sold at any point in time. Therefore, our model predicts that β_3 is positive: treatment farmers with higher connections reject fewer offers and sell faster than treatment farmers with lower connections. The model also predicts that β_2 is zero: connections with treated communities have no impact on timing of sales for control farmers. Estimates for Equation 14 are shown in Table IX and support both predictions. In Year 2, the estimated β_2 is zero, and β_3 is positive and significant at the 5% level.

[INSERT TABLE IX HERE]

Further support is found in Figure V, which plots the raw means and 90% confidence intervals for cumulative sales of yams for farmers above and below the sample median of C2T, by month

[56] To see this formally, recall that the probability that bargaining ends successfully in each period for the two types of farmers are: $w \cdot Pr(R^I > \nu) + (1-w) \cdot Pr(pooling\ offer)$ for the treatment group and $w \cdot Pr(R^U > \nu) + (1-w) \cdot 1$ for the control group, where Pr indicates a probability. It is easily seen that the probability of a pooling offer does not affect the probability of successful bargaining (i.e. of a sale) for control group farmers.

[57] For example, for October 2012, the fourth month in the Year 2 (2012-2013) agricultural season, we calculate F_{ijt} as the total amount of yam sold from July 2012 through October 2012, divided by the total amount of yam sold over the entire agricultural season.

and treatment status, in the secon year of the study. For treatment farmers, the mean cumulative fraction sold for the above-median C2T group always lies above the mean for the below-median C2T group, and in several months the difference in means is statistically significant. In contrast, there is no significant difference for control farmers.

[INSERT FIGURE V HERE]

7 Conclusion

We implement a randomized experiment that gives commodity price information to rural farmers via text messages on their mobile phones. We show that the price alerts have a large and meaningful impact on yam prices for the treatment group (+7% in the first year, and +9% in the second), and positive spillovers on prices received by certain control group farmers. Higher prices are not associated with changes in the timing or place of sale or in the quantities sold. Given the low cost of information delivery via mobile phone, the intervention is highly cost-effective, with returns equal to over 200% of total costs.

The richness of our data, combined with a model of bargaining with asymmetric information, allows us to investigate the causal mechanisms behind the spillover effects. Our analysis suggests that the spillovers are best explained by changes in traders' bargaining behaviors caused by the intervention (we refer to such effects as "bargaining spillovers"), a mechanism which so far has not received much attention in the literature. Information sharing between treatment and control farmers cannot explain the dynamics we observe in the data, as control farmers remain substantially uninformed.

From a methodological perspective, our study showcases the importance of considering both direct and indirect channels of spillovers. While the risk of treatment contagion and direct externalities is generally well understood in the impact evaluation literature, the indirect effects which could arise from a market setting are generally understudied. Yet, they can induce substantial bias. In the context of MIS intervention, for example, network externalities above and beyond information sharing may arise because prices are endogenously determined.

A second important finding is that the impact of price information depends upon the specific characteristics of the marketing environment. The price alerts have large impacts on prices for yam, a crop characterized by high price variability, the absence of a reference “market price” and a high prevalence of bargaining. However, they induce no effect on other crops, for which price variability is lower and farmers refer to a prevalent “market price”, or to crops which are sold to local traders with whom farmers may have long standing relationships.

A final point worth discussing is the impact of price alerts on farmers’ marketing behaviors. Farmers in our study might be able to profit from increasing their production of yam, and from implementing spatial and inter-temporal arbitrage (selling at urban markets, when prices are high). Yet, we find nearly no evidence of such changes. This suggests that price information, by and in itself, might not be enough, at least within the time range that we explore. Farmers might need access to credit, markets, reliable transportation, or simply more time to adapt.

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Table I

Background on agricultural marketing, by crop

	Yam	Maize	Raw cassava	Processed cassava	Groundnut
Percent of crop sold at:					
Farm gate	23.5%	0%	99.2%	0%	0%
Home (community)	18.3%	64.9%	0.8%	55.0%	39.2%
Local market	46.0%	35.1%	0%	45.0%	58.9%
Urban market	11.6%	0%	0%	0%	1.9%
Bargaining:					
Percent that bargain with buyers	99.6%	52.1%	62.5%	35.7%	26.7%
Price dispersion	0.381	0.266	0.355	0.198	0.318
Number of long-term buyers:					
0	30.1%	29.4%	23.5%	6.4%	35.1%
1	18.6%	23.5%	6.0%	14.7%	23.6%
2–3	29.5%	24.7%	35.6%	31.8%	22.7%
4 or more	21.8%	22.5%	34.9%	47.2%	18.7%
Average	0.61	0.70	0.92	0.80	0.55
Number of buyers last season:					
1	11.3%	35.2%	6.4%	10.1%	41.2%
2–3	36.2%	28.8%	17.5%	18.4%	22.6%
4–6	19.1%	18.2%	24.0%	30.0%	4.0%
7 or more	33.4%	17.9%	52.1%	41.6%	32.2%
% of buyers not long-term	58.4%	45.8%	37.2%	54.3%	54.0%

"Percent of crop sold" comes from the monthly data, pre-treatment period (Aug-Oct 2011). Figures are the percent of volume (quantity) sold at each location type. "Percent that bargain with buyers" comes from the mid-line survey (not asked at baseline), from a section which asks farmers to recall the details of a specific transaction that occurred in the prior agricultural season. The figures show the percent of farmers that report bargaining with the buyer in that particular sale. "Number of long-term buyers" comes from the baseline survey. "Number of buyers last season" comes from the mid-line survey (not asked at baseline), and reflects the number of buyers the farmer sold a particular type of crop to over the previous agricultural season. Price dispersion: average within-district coefficient of variation (CV), excluding outliers and districts with fewer than four farmers selling.

Table II

Descriptive statistics and balance at baseline

	Control	Treatment	T - C	S.E. (T-C)
Farmer characteristics				
Age	41.00	40.60	-0.40	(1.15)
Schooling - JHS or higher	44.7%	38.8%	-5.9%	(0.06)
Male	78.7%	81.7%	2.9%	(0.05)
Farming is main source of income	76.8%	79.7%	2.8%	(0.05)
Land cultivated last season (acres)	6.72	7.21	0.50	(0.65)
Median income from two main crops (GHS)	1,400	1,400	0	(205.7)
Mean income from two main crops (GHS)	2,064	2,320	256	(288.3)
Mean of asset index	0.081	-0.077	-0.158	(0.19)
Owns a bicycle	83.2%	82.2%	-0.9%	(0.04)
Owns a motorbike	27.7%	29.8%	2.0%	(0.04)
Owns a radio	73.1%	71.2%	-1.9%	(0.05)
Owns a TV	36.4%	30.4%	-6.1%	(0.06)
Phone ownership and usage				
Owns a mobile phone	72.3%	69.8%	-2.5%	(0.04)
Sends SMS messages	22.6%	14.7%	-7.9%*	(0.04)
Receives SMS messages	32.0%	22.9%	-9.1%	(0.06)
Crops grown				
Yam	60.7%	65.9%	5.1%	(0.07)
Cassava	37.0%	43.8%	6.8%	(0.08)
Maize	46.1%	35.7%	-10.4%*	(0.06)
Groundnut	19.2%	26.0%	6.8%	(0.05)
Where crops are sold				
Percent sell at farm/home	73.6%	75.1%	1.6%	(0.07)
Percent sell at local markets	67.6%	65.7%	-1.9%	(0.08)
Percent sell at urban markets	15.5%	18.7%	3.2%	(0.05)
Mean distance to nearest district market (mi)	10.97	10.82	-0.147	
Knowledge of market prices				
Percent well informed about urban prices	33.3%	26.2%	-7.2%	(0.05)
Percent well informed about local prices	84.6%	75.1%	-9.5%	(0.06)
Price received at baseline				
Yam	121.6	128.4	6.8	(7.10)
Number of communities	49	51		
Number of clusters	45	45		
Number of observations	484	507		

Standard errors of the difference are clustered at the community cluster level. To test for statistically significant differences in medians, we followed Parente and Silva (2016).
"Sends SMS" and "Receives SMS" figures include mobile phone owners only.
*** Significant at 1% level. ** Significant at 5% level. * Significant at 10% level.

Table III

Descriptive statistics and balance at baseline - Yam

	Control	Treatment	T - C	S.E. (T-C)
Farmer characteristics				
Age	40.60	40.51	-0.09	(1.32)
Schooling - JHS or higher	41.3%	34.5%	-6.4%	(0.07)
Male	95.5%	97.9%	2.3%	(0.02)
Farming is main source of income	84.2%	85.5%	1.21%	(0.04)
Land cultivated last season (acres)	7.99	8.85	0.86	(0.79)
Median income from two main crops (GHS)	1,400	2,250	850	(300.4)
Mean income from two main crops (GHS)	2,657	2,999	341	(370.8)
Mean of asset index	0.22	-0.06	-0.28	(0.23)
Owns a bicycle	90.5%	88.0%	-2.9%	(0.03)
Owns a motorbike	37.1%	35.0%	2.1%	(0.05)
Owns a radio	69.7%	68.6%	-1.2%	(0.06)
Owns a TV	37.8%	29.3%	-8.4%	(0.07)
Phone ownership and usage				
Owns a mobile phone	73.5%	74.6%	1.1%	(0.05)
Sends SMS messages	22.7%	13.7%	-9.0%**	(0.04)
Receives SMS messages	31.9%	16.9%	-15.1%***	(0.05)
Where crops are sold				
Percent sell at farm/home	71.8%	75.8%	4.0%	(0.08)
Percent sell at local markets	65.7%	68.6%	2.9%	(0.09)
Percent sell at urban markets	24.8%	24.9%	0.02%	(0.07)
Mean distance to nearest district market (mi)	9.85	9.62	-0.23	()
Knowledge of market prices				
Percent well informed about urban prices	44.2%	28.8%	-15.3%**	(0.07)
Percent well informed about local prices	81.5%	74.5%	-6.6%	(0.08)
Number of communities	49	51		
Number of clusters	45	45		
Number of observations	484	507		

Standard errors of the difference are clustered at the community cluster level. To test for statistically significant differences in medians, we followed Parente and Silva (2016).

"Sends SMS" and "Receives SMS" figures include mobile phone owners only.

*** Significant at 1% level. ** Significant at 5% level. * Significant at 10% level.

Table IV

Impact of price alerts, assuming no spillovers

Equation (1): $p_{ijt} = \sum_{s=0}^{2}\{\lambda_s Y_s + \kappa_s(T_j * Y_s)\} + X'_{ij}\psi + \omega_k + \omega_t + e_{ijt}$							
	Yam (1)	Yam (2)	All others (3)	Maize (4)	Groundnut (5)	Cassava, Raw (6)	Cassava, Processed (7)
Panel A: Price, level							
Treatment, Pre-T (κ_0)	-0.340	0.641	-2.069	-3.734	-15.356	2.062	-1.678
	(6.903)	(6.942)	(2.518)	(5.249)	(29.588)	(2.253)	(2.542)
Treatment, Shorter-run (κ_1)	7.589*	8.732**	0.209	-0.674	-30.934	2.361	0.792
	(3.825)	(3.687)	(2.088)	(1.731)	(26.145)	(2.313)	(1.738)
Treatment, Longer-run (κ_2)	-0.393	-0.014	-4.112	1.499	-8.779	0.039	-3.727
	(4.498)	(4.483)	(2.775)	(1.773)	(15.702)	(3.559)	(2.791)
R^2	0.311	0.315	0.836	0.434	0.317	0.610	0.840
Panel B: Price, log							
Treatment, Pre-T	-0.028	-0.022	-0.023	-0.055	-0.108	0.067	-0.055
	(0.053)	(0.053)	(0.035)	(0.069)	(0.149)	(0.056)	(0.047)
Treatment, Shorter-run	0.043*	0.050**	0.027	-0.008	-0.103	0.092**	0.020
	(0.025)	(0.025)	(0.026)	(0.018)	(0.100)	(0.045)	(0.030)
Treatment, Longer-run	-0.010	-0.008	-0.027	0.015	-0.041	-0.006	-0.031
	(0.027)	(0.028)	(0.023)	(0.020)	(0.066)	(0.059)	(0.028)
R^2	0.339	0.342	0.865	0.429	0.311	0.555	0.850
Control group mean price	151.02	151.02	134.15	172.35	172.35	151.02	151.02
Individual covariates		✓	✓	✓	✓	✓	✓
N. Observations	5,032	5,032	7,762	1,568	569	1,177	3,940

Notes: Prices are per 100 tubers, denominated in real, August 2011 Ghana Cedis (GHS). Year 1 = Nov 2011-Jun 2012. Year 2 = Jul 2012-Jun 2013. Pre-T = Aug 2011-Oct 2011 (before the start of the intervention). All regressions include strata fixed effects, period fixed effects, and controls for yam type. Other covariates include farmer's gender and asset index level, and the community's distance to the closest district market. Standard errors clustered at the community cluster level are shown in parentheses. As a robustness check, we also repeat the estimations including the self-reported quality of information at baseline as an additional covariate, getting very similar estimates, available from the authors upon request. Analysis relies on monthly data; results using annual data are comparable.
** Significant at 5% level. * Significant at 10% level.

TABLE V

Relationship between Accra prices and farmers' initial asking prices (yam)

Dependent variable: farmers' initial asking prices	Basic controls	Full controls
Panel A: Actual Accra price, Eq. (2)		
Accra price (α)	0.337***	0.291**
	(0.113)	(0.128)
Accra price * Treatment (β)	0.175	0.172
	(0.194)	(0.174)
Observations	833	818
R^2	0.096	0.166
Panel B: Deviation from predicted Accra price, Eq. (3)		
Accra price (deviation) (α)	0.203	0.102
	(0.185)	(0.191)
Accra price (deviation) * Treatment (β)	0.601**	0.720***
	(0.299)	(0.262)
Observations	833	818
R^2	0.087	0.163

Notes: The regression looks at the impact of the monthly average price for yam in Accra on farmers' initial asking prices in their bargaining with traders. Data are from the mid-line and end-line surveys, which asked farmers to recall details from an important transaction from the prior agricultural year. The question was designed to gather pertinent information on a memorable transaction, in order to facilitate the analysis of mechanisms without overburdening farmers to provide us with excessive information for all transactions. All regressions include strata fixed effects, and survey-by-treatment fixed effects. The "full controls" columns present results that also control for quantity of yam sold (quadratic) and place of sale (home, farm gate, local market, or urban market). The predicted Accra price is taken from a regression of Accra prices on a linear time trend and monthly fixed effects. *** Significant at 1% level ** Significant at 5% level * Significant at 10% level

Table VI

Estimating the effect on yam prices of connections to treated communities

	Price, level		Price, log	
	Equation (1)	Equation (4)	Equation (1)	Equation (4)
	(1)	(2)	(3)	(4)
Treatment, pre-T (α_0)	-0.340	6.339	-0.028	0.102
	(6.836)	(14.105)	(0.053)	(0.114)
Treatment, Year 1 (α_1)	7.589**	4.554	0.043*	0.036
	(3.788)	(10.363)	(0.025)	(0.072)
Treatment, Year 2 (α_2)	-0.393	14.846	-0.010	0.065
	(4.455)	(11.398)	(0.027)	(0.068)
C2T * Control, Pre-T (β_0)		-3.468		-0.032
		(16.648)		(0.136)
C2T * Control, Year 1 (β_1)		12.517		0.048
		(8.447)		(0.062)
C2T * Control, Year 2 (β_2)		24.637**		0.180***
		(10.098)		(0.065)
C2T * Treatment, Pre-T (γ_0)		-15.268		-0.263*
		(19.194)		(0.141)
C2T * Treatment, Year 1 (γ_1)		18.966		0.065
		(15.211)		(0.099)
C2T * Treatment, Year 2 (γ_2)		-2.955		0.042
		(18.846)		(0.117)
Pre-T (λ_0)	98.550***	100.004***	4.601***	4.616***
	(6.810)	(10.794)	(0.050)	(0.087)
Year 1 (λ_1)	157.728***	149.155***	4.984***	4.950***
	(8.191)	(9.800)	(0.052)	(0.064)
Year 2 (λ_2)	162.684***	147.976***	4.970***	4.866***
	(7.487)	(9.741)	(0.055)	(0.066)

Notes: Prices are per 100 tubers, denominated in real, August 2011 Ghana Cedis (GHS). Year 1 = Nov 2011-Jun 2012. Year 2 = Jul 2012-Jun 2013. Pre-T = Aug 2011-Oct 2011 (before the start of the intervention). Regressions include strata fixed effects, period fixed effects, and controls for yam type. Columns (1) and (3) therefore correspond to Column (1) in Table IV. Standard errors clustered at the community cluster level are shown in parentheses. The equations are estimated using two-step system GMM. *** Significant at 1% level ** Significant at 5% level * Significant at 10% level. Equation (1): $p_{ijt} = \sum_{s=0}^{2}\{\lambda_s Y_s + \kappa_s(T_j * Y_s)\} + X'_{ij}\psi + \omega_k + \omega_t + e_{ijt}$. Equation (4): $\ln p_{ijt} = \sum_{s=0}^{2}\{\delta_s Y_s + \alpha_s(T_j * Y_s) + \beta_s(C_j * \mathrm{C2T}_j * Y_s) + \gamma_s(T_j * \mathrm{C2T}_j * Y_s)\} + X'_{ij}\psi + \omega_k + \omega_t + e_{ijt}$

Table VII

Estimation errors of yam prices in Accra, endline survey

	Log of absolute error		Log of absolute % error	
	(1)	(2)	(3)	(4)
Treatment	-0.244*	-0.580*	-0.292*	-0.613
	(0.141)	(0.346)	(0.163)	(0.379)
C2T * Control		0.007		0.044
		(0.396)		(0.441)
C2T * Treatment		0.664		0.691
		(0.635)		(0.710)
Difference		-0.658		-0.647
		(0.676)		(0.754)
N	541	541	541	541
R^2	0.103	0.105	0.095	0.097

The endline survey asked farmers to provide an estimate of contemporaneous prices for yam in Accra. We calculated "errors" by taking the difference between the price provided in the Esoko alerts and the farmer's estimate. All regressions include strata fixed effects, interview week fixed effects, and yam type fixed effects. "Difference" shows the linear combination (C2T * Control − C2T * Treatment). Huber-White robust standard errors clustered by community cluster are in parentheses.
*** Significant at 1% level. ** Significant at 5% level. * Significant at 10% level.

Table VIII

Estimate of spillovers and de-biased treatment effect

	(1) Pre-T	(2) Year 1	(3) Year 2
Panel A: Price, level			
Biased treatment effect (κ_s)	-0.340	7.589**	-0.393
	[-13.737, 13.058]	[0.165, 15.013]	[-9.124, 8.338]
Average spillovers for Control ($\lambda_s - \delta_s$)	-1.454	8.574*	14.708**
	[-20.715, 17.807]	[-1.532, 18.679]	[3.042, 26.373]
De-biased treatment effect $[\kappa_s + (\lambda_s - \delta_s)]$	-1.793	16.163**	14.315**
	[-22.872, 19.285]	[2.702, 29.623]	[1.017, 27.613]
Panel B: Price, log			
Biased treatment effect (κ_s)	-2.81%	4.32%*	-0.99%
	[-13.12%, 7.50%]	[-0.62%, 9.26%]	[-6.33%, 4.35%]
Average spillovers for Control ($\lambda_s - \delta_s$)	-1.49%	3.43%	10.40%***
	[-17.23%, 14.26%]	[-3.77%, 10.63%]	[2.91%, 17.88%]
De-biased treatment effect $[\kappa_s + (\lambda_s - \delta_s)]$	-4.30%	7.75%	9.41%**
	[21.44%, 12.85%]	[-1.87%, 17.37%]	[1.48%, 17.34%]

Notes: Prices are per 100 tubers, denominated in real, August 2011 Ghana Cedis (GHS). Year 1 = Nov 2011-Jun 2012. Year 2 = Jul 2012-Jun 2013. Pre-T = Aug 2011-Oct 2011 (before the start of the intervention). Regressions include strata fixed effects, period fixed effects, and controls for yam type. Standard errors clustered at the community cluster level are shown in parentheses. The equations are estimating using two-step system GMM. Figures in square brackets denote 95% confidence intervals.
*** Significant at 1% level ** Significant at 5% level * Significant at 10% level

Table IX

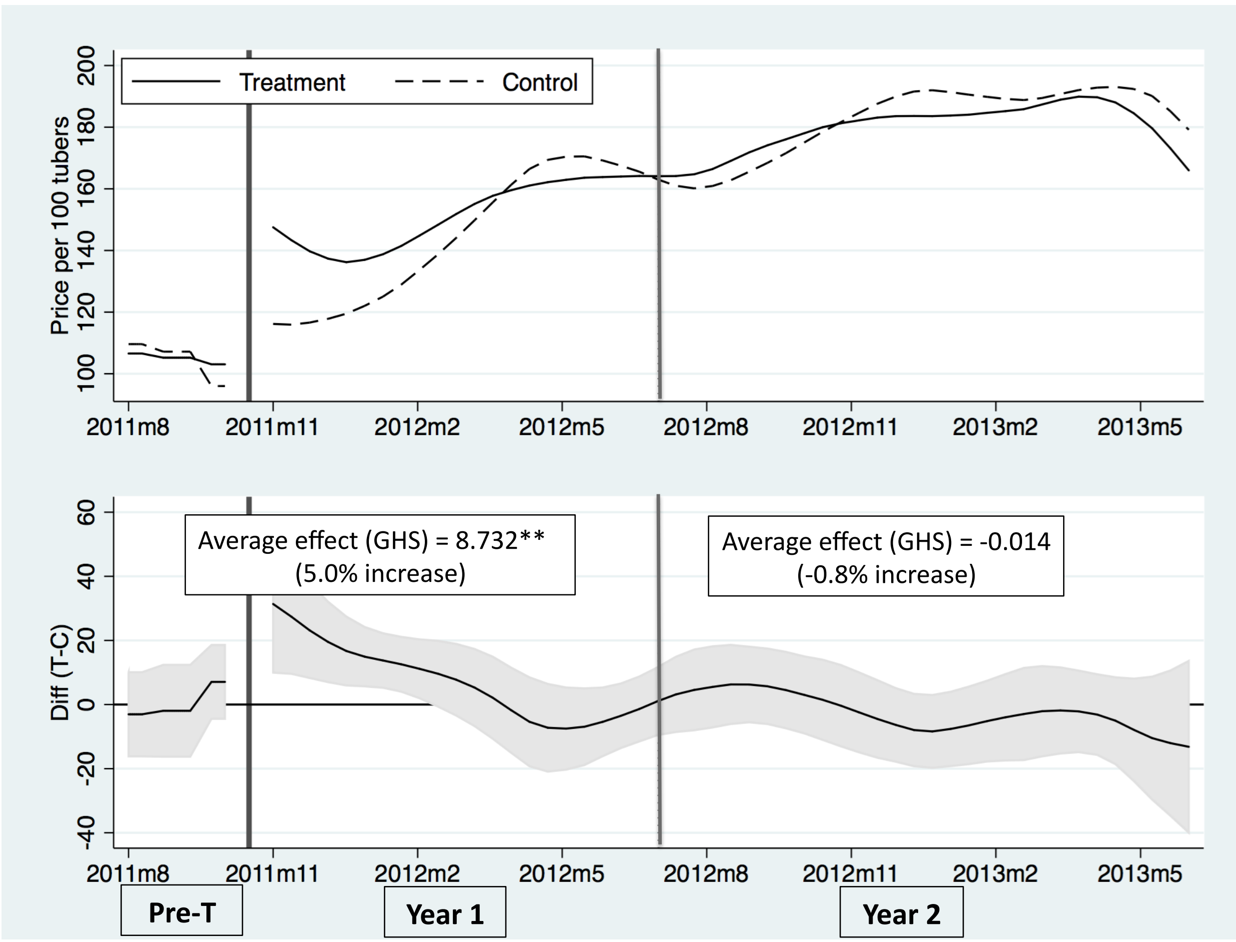
Impact of C2T on timing of yam sales

	Year 1	Year 2
Treatment	0.002	-0.120*
	(0.063)	(0.064)
C2T * Control	0.045	0.038
	(0.068)	(0.039)
C2T * Treatment	0.068	0.238**
	(0.121)	(0.106)
N	4,590	6,875
R^2	0.542	0.560

Notes: The dependent variable is the cumulative fraction of yam that each farmer has sold by a given period in the agricultural year. The regression includes monthly fixed effects and strata fixed effects. Standard errors are adjusted for clustering at the community cluster level. ** Significant at 5% level * Significant at 10% level

Figure I

Impact of price alerts on yam prices, assuming no spillovers

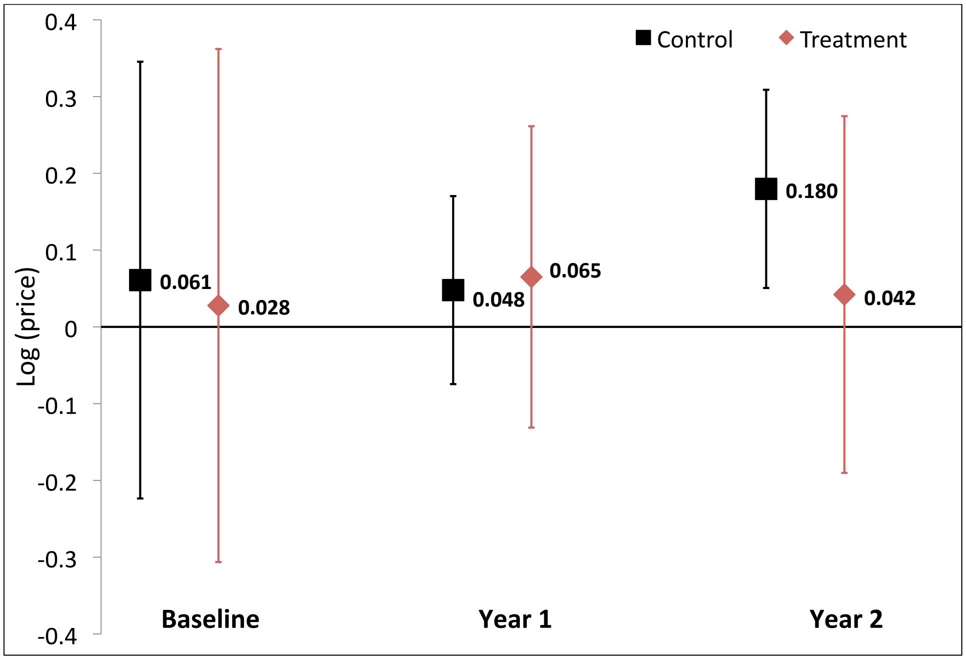


Notes: The top figure plots yam prices for treatment and control groups, estimated using non-parametric (Fan) regression, controlling for strata fixed effects, yam type, gender, asset index, and distance to the nearest local market. The bottom figure plots the difference between treatment and control group prices, with the bootstrapped 95% confidence interval shown in grey (cluster-bootstrap by community cluster, 1000 replications with replacement). The bottom figure also displays the average estimated treatment effect for each agricultural year, using results from the pooled regression with additional covariates (column (6) of Table **??**). The dotted red line separates Year 1 results (November 2011-June 2012) from Year 2 results (July 2012-June 2013).

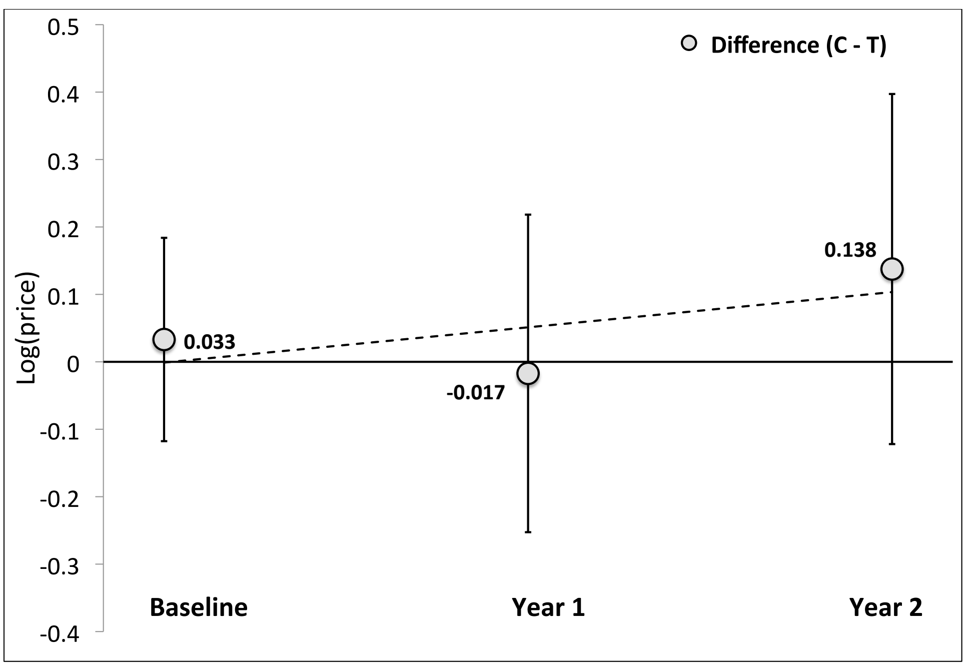
Figure II

Impact of C2T on yam prices over time

(a) **Estimated impact of C2T on prices**



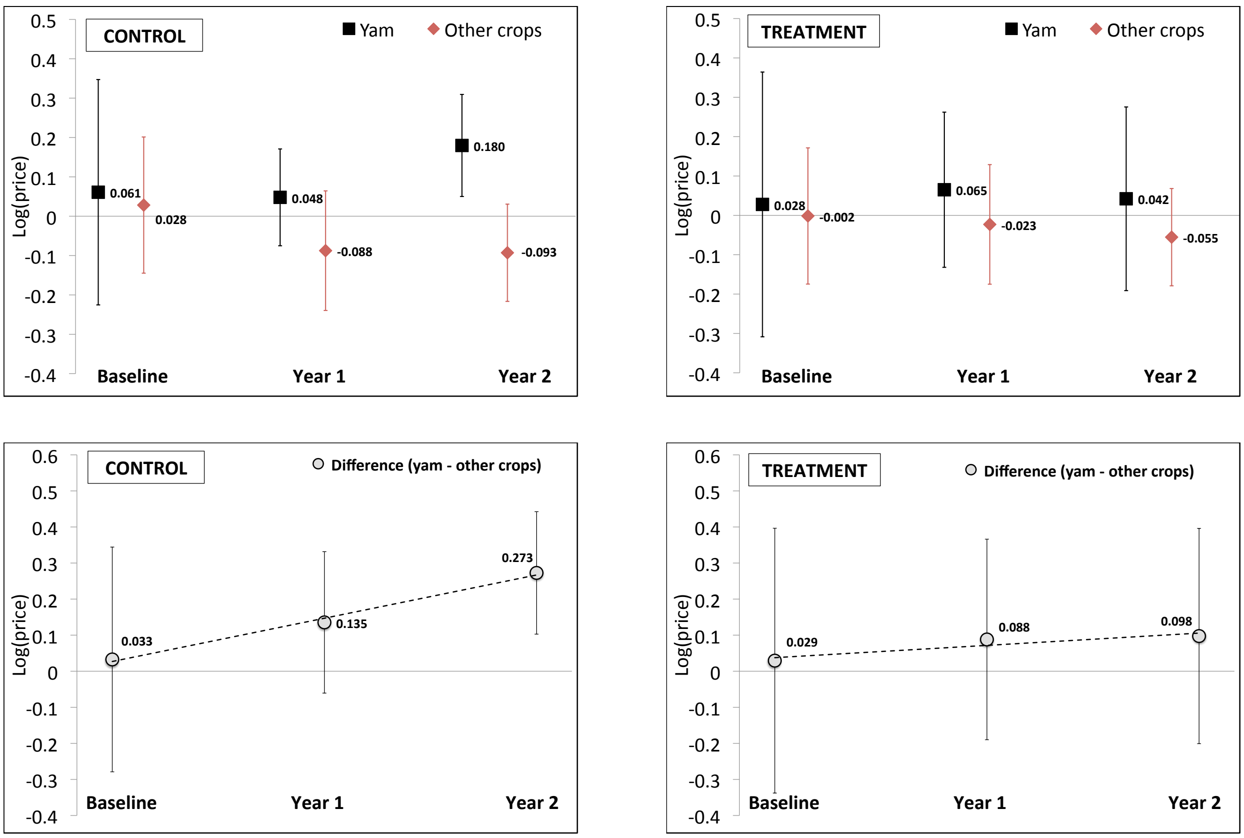
(b) **Difference (control - treatment)**



Notes: The top panel plots the impact of C2T on logged yam prices for the control group and the treatment group. The bottom panel shows the difference in the impact of C2T on yam prices (control - treatment). Error bars show 95% confidence intervals. Dashed line is a linear trend line. Baseline data reflect agricultural season prior to the intervention. Year 1 and Year 2 data are taken from monthly surveys. Includes controls for strata, period, and yam type.

Figure III

Impact of C2T on prices of crops - yam versus other crops

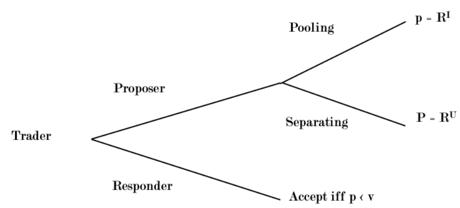


Notes: The top panels plot the impact of C2T on prices for yam and other crops, for control farmers (left-hand side) and treatment farmers (right-hand side). The bottom panels show the difference in the impact of C2T (yam prices vs. other crop prices). Error bars show 95% confidence intervals. Dashed line is a linear trend line. Baseline data reflect agricultural season prior to the intervention. Year 1 and Year 2 data are taken from monthly surveys. Regressions include crop-strata and crop-period fixed effects, and controls for yam type.

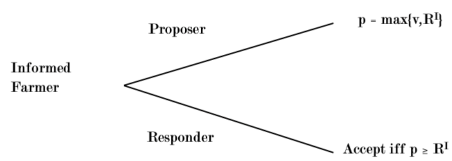
Figure IV

Optimal strategies

(a) Trader



(b) Informed Farmer



(c) Uninformed Farmer

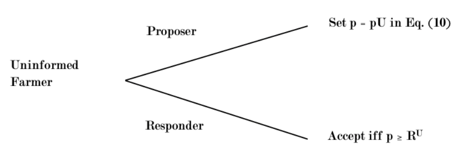
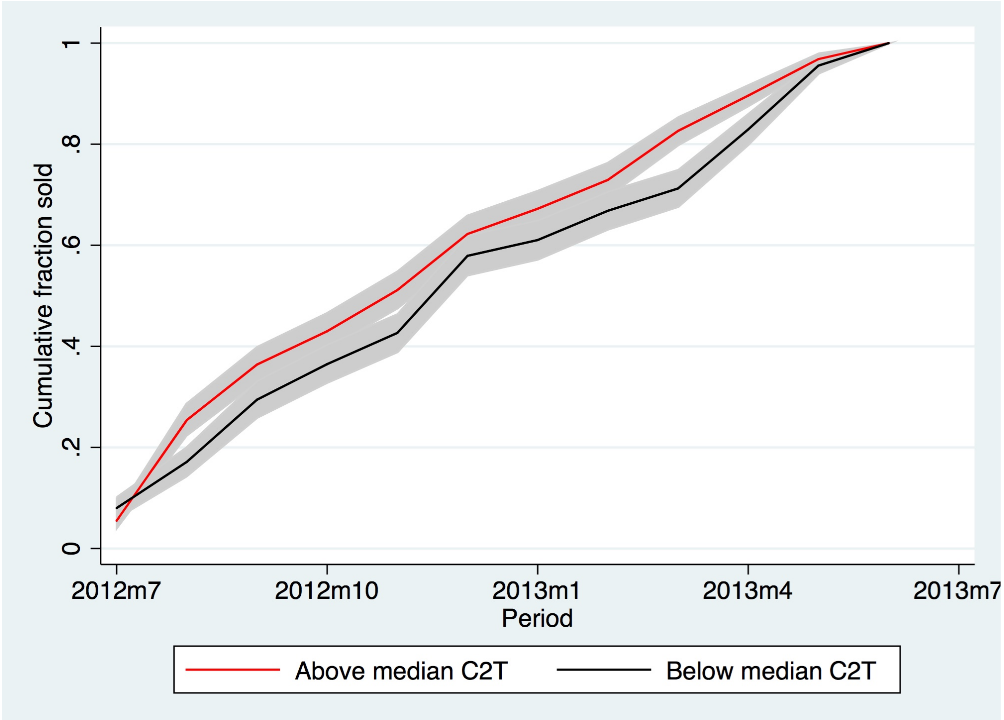


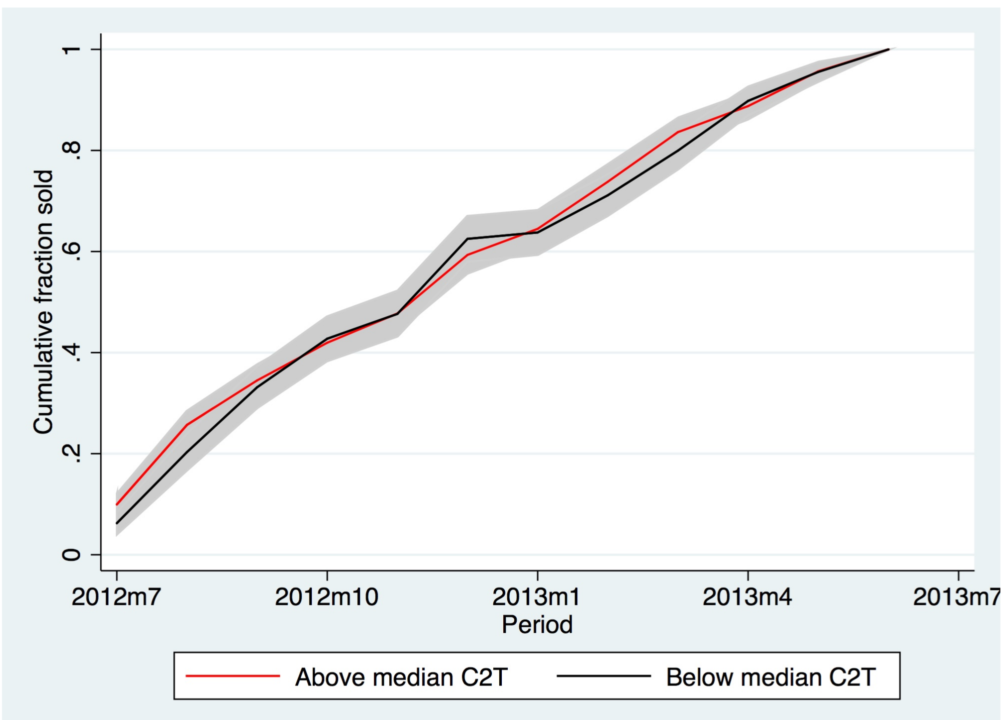
Figure V

Cumulative fraction of yam sold in Year 2, by month and C2T level

(a) Treatment



(b) Control



Notes: The figures plot the mean cumulative fraction sold by each month, for farmers with above median C2T and for farmers with below median C2T. 90% confidence intervals are shaded in grey.

A Appendix A: Impact on other outcomes

A.1 Spatial and Inter-temporal arbitrage

We examine whether the intervention led farmers to make changes in whether they sold their yam, or the timing of their yam sales. Because we found no evidence of spillovers to control farmers for crops different from yam, we report only results which rely on the SUTVA and are based on simple comparisons between treatment and control group outcomes. We also conducted additional analysis to explicitly relax the SUTVA, exploiting our C2T measure to compute "de-biased" estimates of treatment effects for place and time of sale and for non-yam crops. Those additional results confirm the findings discussed here and are available upon request. Over all, we fail to find any strong evidence of either of spatial or inter-temporal arbitrage.

Spatial arbitrage

Related studies on mobile phones and mobile phone-based information services consider the impact that better access to information has on producers' decisions about *where* to sell (Jensen 2007; Aker 2008; Aker and Fafchamps 2014). In our study, the key spatial decision faced by yam farmers is whether to sell at the urban market (Accra), a local market (i.e. at the district headquarters), the community (home), or at the farm-gate. In Table A4, we look at potential differences in farmers' decisions to sell at each of these locations by treatment status, along both the extensive margin (columns (1)-(3)) and the intensive margin (columns (4)-(6)). There is no evidence that the intervention led to significant changes in the prevalence or magnitude of sales at urban or local markets in either year of the study. Given the high cost of transporting to Accra, and the difficulties faced by farmers that try to sell there, it is not surprising that our intervention did not lead to a large shift in direct sales in the city.[58] The fact that the intervention had little or no effect on local market sales could similarly reflect barriers to accessing local markets, or simply the fact that farmers already had decent information on local markets prices prior to our intervention.

Interestingly, the results of Table A4 suggest that some farmers reduced sales made at home in favor of sales at the farm-gate. If this is truly the case, it could be viewed as providing additional support for the notion that the intervention improved farmers' bargaining position with traders. As pointed out by Fafchamps and Minten (2012), information may give farmers the "confidence" to sell at the farm-gate rather than incur costs to transport crops to more central selling locations (such as the community), because they feel that with the information they can better negotiate with a farm-gate buyer. One could interpret the findings in Table A4 as supportive of that hypothesis.

Inter-temporal arbitrage

In addition to affecting decisions about where to sell, the price alerts could have impacted decisions made about the timing of sales over the course of the agricultural season. We rely on the monthly data to study changes in farmers'

[58] The larger urban markets in Ghana are typically overseen by a "market queen" who has considerable power over who is permitted to sell. In the field work prior to the start of our study, we heard stories from a few farmers about paying to transport yams to Accra, only to find that they were unable to access the market and felt compelled to sell their stock to a trader at a low price to avoid the cost of taking it back home.

TABLE A1

Impact of price alerts on yam quantities, assuming no spillovers

	Year 1		Year 2		Pooled	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Quantity sold, level						
Treatment, Pre-T					-42.589	-29.329
					(46.045)	(46.289)
Treatment, Year 1	5.498	6.227			21.747	34.655
	(19.493)	(18.302)			(24.996)	(24.663)
Treatment, Year 2			42.060	50.756	40.957	45.636
			(32.117)	(32.252)	(32.038)	(31.963)
N	1,522	1,522	2,659	2,659	5,030	5,030
R^2	0.063	0.091	0.071	0.105	0.078	0.104
Control group mean	294.4	294.4	375.9	375.9	353.5	353.5
Panel B: Quantity sold, log						
Treatment, Pre-T					-0.186	-0.169
					(0.114)	(0.113)
Treatment, Year 1	0.023	0.030			0.049	0.065
	(0.061)	(0.059)			(0.069)	(0.067)
Treatment, Year 2			0.027	0.035	0.034	0.038
			(0.062)	(0.062)	(0.064)	(0.064)
N	1,522	1,522	2,659	2,659	5,030	5,030
R^2	0.046	0.074	0.076	0.095	0.083	0.103
Panel C: Land cultivated for yam, level						
Treatment, Pre-T					0.394	0.454
					(0.444)	(0.442)
Treatment, Year 1	0.388	0.438			-0.086	-0.087
	(0.314)	(0.307)			(0.414)	(0.413)
Treatment, Year 2			0.173	0.199	-0.231	-0.243
			(0.420)	(0.410)	(0.384)	(0.383)
Observations	1179	1179	1138	1138	1686	1686
R^2	0.078	0.102	0.096	0.121	0.083	0.116
Control group mean	4.484	4.484	4.495	4.495	4.380	4.380
Other covariates		✓		✓		✓

Notes: Quantity of yam sold is in number of tubers. Year 1 = Nov 2011-Jun 2012. Year 2 = Jul 2012-Jun 2013. Pre-T = Aug 2011-Oct 2011 (before the start of the intervention). All regressions include strata fixed effects, period fixed effects, and controls for yam type. Other covariates include farmer's gender and asset index level, and the community's distance to the closest district market. Standard errors clustered at the community cluster level are shown in parentheses. Analysis relies on monthly data; results using annual data are comparable.
** Significant at 5% level. * Significant at 10% level.

Table A2

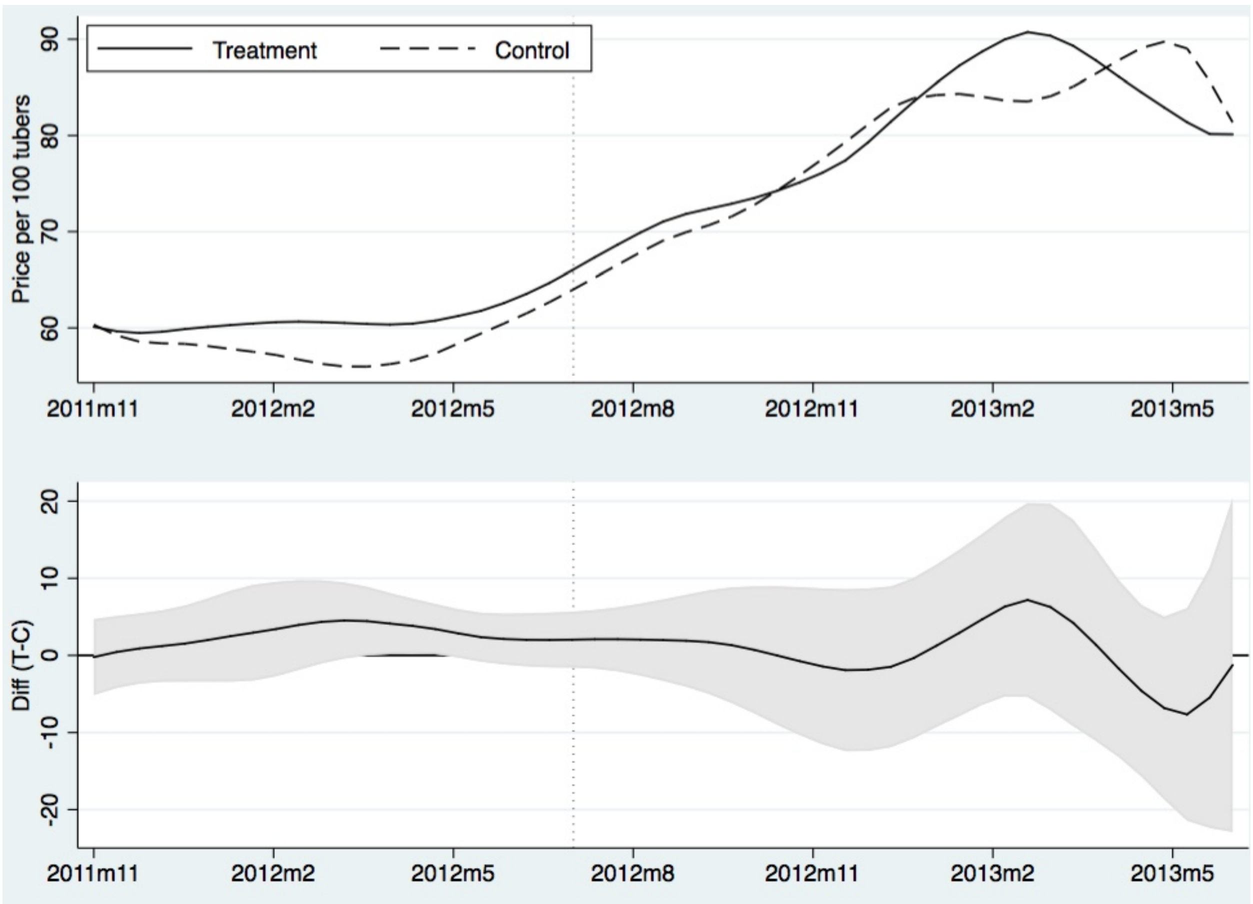
Impact of price alerts on quantities for other crops, assuming no spillovers

	All (1)	Maize (2)	Groundnut (3)	Cassava: raw (4)	Cassava: processed (5)
Panel A: Quantity, level					
Treatment, Pre-T	-3.921	-4.932	-19.136	-0.293	-1.880
	(7.928)	(25.989)	(34.628)	(1.001)	(6.081)
Treatment, Year 1	1.284	-25.522	-14.863	0.100	21.209
	(12.905)	(34.472)	(25.423)	(0.714)	(14.625)
Treatment, Year 2	0.698	2.466	-7.480	0.029	0.748
	(8.645)	(31.511)	(7.727)	(0.882)	(6.818)
N	7,753	1,563	568	1,177	3,939
R^2	0.366	0.123	0.183	0.105	0.589
Panel A: Quantity, log					
Treatment, Pre-T	0.065	0.048	0.178	-0.021	0.108
	(0.084)	(0.137)	(0.337)	(0.185)	(0.102)
Treatment, Year 1	0.052	0.033	0.208	0.107	0.096
	(0.074)	(0.183)	(0.270)	(0.104)	(0.069)
Treatment, Year 2	-0.075	-0.065	-0.132	-0.060	-0.104
	(0.081)	(0.189)	(0.170)	(0.131)	(0.072)
N	7,753	1,563	568	1,177	3,939
R^2	0.870	0.173	0.282	0.215	0.928

Monthly data using OLS (pooled specification with controls for farmer traits). "All" includes sales of the most prevalent crops (maize, cassava, groundnut, rice) exclusive of yam. Regressions include crop-period and crop-strata fixed effects. Huber-White robust standard errors clustered by community cluster are in parentheses. Quantities are in long bags for all crops except processed cassava (quantities are in ropes).
*** Significant at 1% level. ** Significant at 5% level. * Significant at 10% level.

Figure A1

Impact of price alerts on cassava prices, assuming no spillovers



Notes: The top figure plots cassava prices for treatment and control groups, estimated using non-parametric (Fan) regression, controlling for strata fixed effects, gender, asset index, and distance to the nearest local market. The bottom figure plots the difference between treatment and control group prices, with the bootstrapped 95% confidence interval shown in gray (cluster-bootstrap by community cluster, 1000 replications with replacement). The vertical line separates shorter- (November 2011-June 2012) and longer-run results (July 2012-June 2013).

Table A3

Effect of alerts on change in land cultivated (acres)

	Follow-up (1)	Endline (2)
Panel A: Yam		
Treatment	0.239	-0.016
	(0.254)	(0.325)
N	614	604
R^2	0.606	0.515
Panel B: Maize		
Treatment	-0.141	0.020
	(0.190)	(0.292)
R^2	0.708	0.643
Panel C: Cassava		
Treatment	0.316	0.594
	(0.258)	(0.379)
N	382	366
R^2	0.729	0.480
Panel D: Groundnut		
Treatment	-0.278	-0.101
	(0.208)	(0.197)
N	201	214
R^2	0.770	0.868

Dependent variable is the change in the acres of land cultivated for a particular crop, relative to baseline. All regressions include strata fixed effects and individual controls. *** Significant at 1% level. ** Significant at 5% level. * Significant at 10% level.

selling decisions over time. Arguably the most important dimension of timing that the price alerts could affect is decisions about whether to (a) sell early in the agricultural season, around harvest time, when aggregate supply is higher and prices are often lower; or (b) wait to sell later in the agricultural season, in the "lean" season of March to May, when aggregate supply is lower and prices tend to be higher. To look for evidence of a treatment effect on the timing of sales, we compute the cumulative fraction F_{ijt} of yam sold at each month t of the agricultural season for each farmer i for the 2011-2012 and 2012-2013 agricultural seasons. In Figure A2 we plot the raw means and 90% confidence intervals for the cumulative fraction sold, by month and treatment status. In both years of data, the overall pattern of sales across time is extremely similar between the treatment and control groups. Thus, we conclude that the price alerts did not greatly alter the timing of yam sales.

Table A4

Impact of price alerts on place of sale, yam

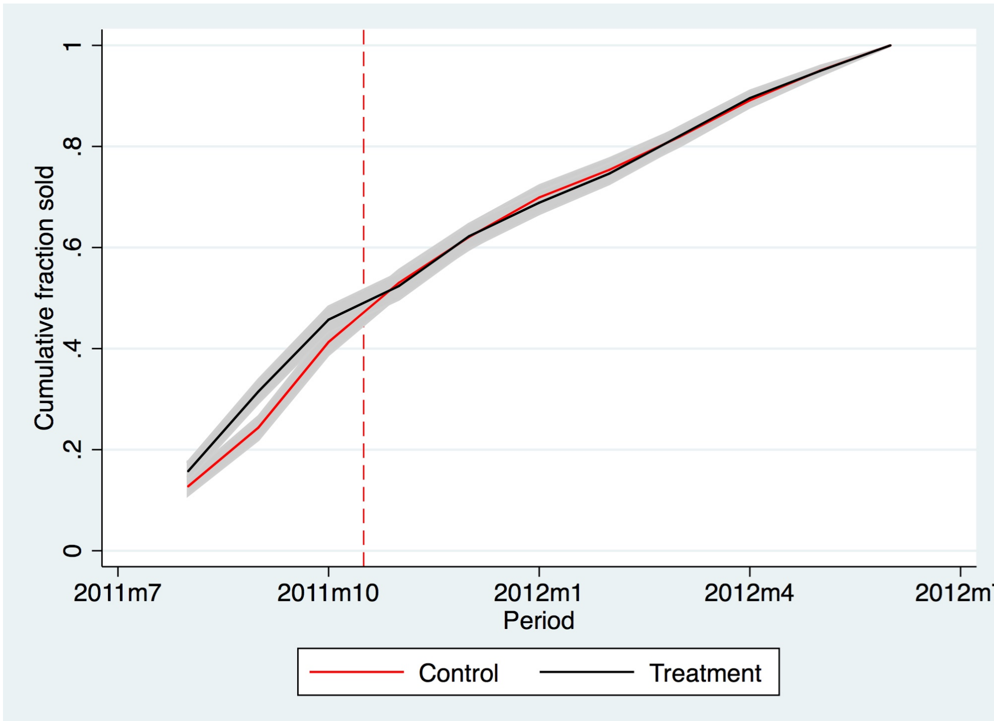
	Sold any			Fraction sold		
	Year 1 (1)	Year 2 (2)	Pooled (3)	Year 1 (4)	Year 2 (5)	Pooled (6)
Panel A: Urban markets						
Treatment, Pre-T			-0.050			-0.031
			(0.047)			(0.039)
Treatment, Year 1	0.070		0.071	0.021		0.016
	(0.055)		(0.055)	(0.027)		(0.029)
Treatment, Year 2		0.074	0.068		0.034	0.027
		(0.051)	(0.050)		(0.033)	(0.032)
R^2	0.113	0.125	0.122	0.097	0.107	0.088
Control group mean	0.085	0.176	0.145	0.047	0.092	0.086
Panel B: Local markets						
Treatment, Pre-T			-0.054			-0.058
			(0.080)			(0.081)
Treatment, Year 1	-0.026		-0.033	0.009		-0.003
	(0.069)		(0.068)	(0.067)		(0.069)
Treatment, Year 2		-0.044	-0.036		-0.060	-0.055
		(0.054)	(0.055)		(0.064)	(0.063)
R^2	0.501	0.412	0.407	0.436	0.323	0.341
Control group mean	0.620	0.709	0.641	0.483	0.533	0.501
Panel C: Farm gate						
Treatment, Pre-T			0.115			0.129*
			(0.073)			(0.068)
Treatment, Year 1	0.126*		0.134*	0.109*		0.117*
	(0.068)		(0.069)	(0.063)		(0.063)
Treatment, Year 2		0.030	0.026		0.027	0.026
		(0.059)	(0.058)		(0.054)	(0.053)
R^2	0.536	0.457	0.481	0.514	0.511	0.496
Control group mean	0.286	0.328	0.299	0.237	0.252	0.240
Panel D: Home (community)						
Treatment, Pre-T			-0.017			-0.028
			(0.072)			(0.053)
Treatment, Year 1	-0.154***		-0.151**	-0.143***		-0.134***
	(0.056)		(0.059)	(0.034)		(0.034)
Treatment, Year 2		-0.004	0.011		-0.006	-0.003
		(0.054)	(0.052)		(0.029)	(0.028)
R^2	0.266	0.320	0.280	0.260	0.194	0.192
Control group mean	0.451	0.291	0.328	0.233	0.122	0.170
Observations	422	626	1,450	422	625	1,448

Monthly data using OLS. Controls: strata fixed effects, gender, asset index, and community's distance to closest district market. Huber-White robust standard errors clustered by community cluster in parentheses. *** Significant at 1% level. ** Significant at 5% level. * Significant at 10% level.

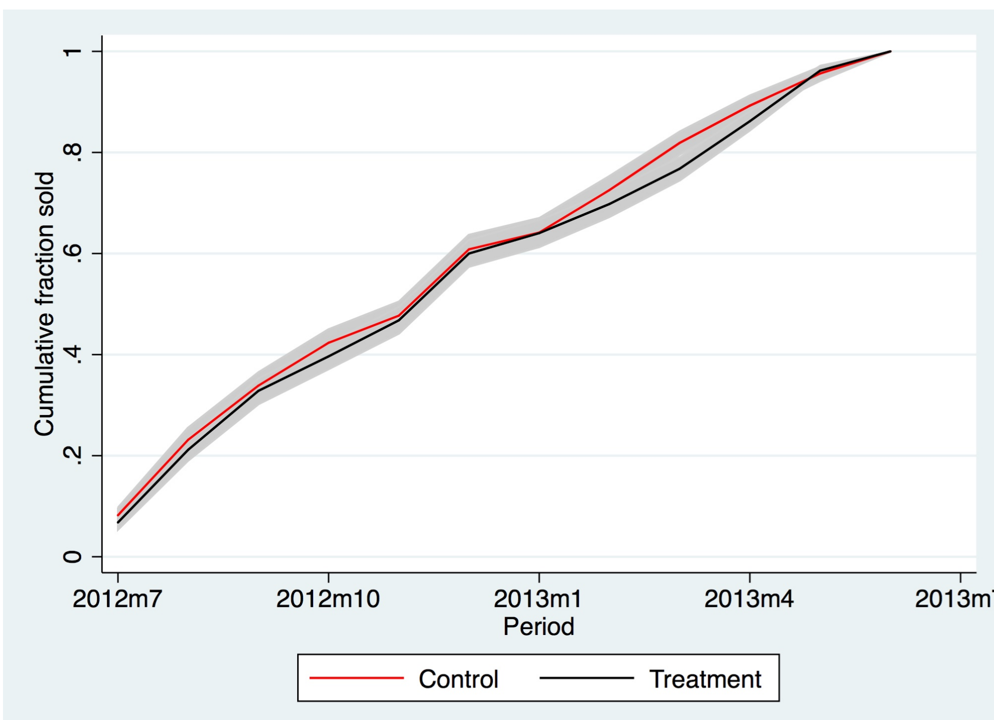
Figure A2

Cumulative fraction of yam sold over time, by month and treatment status

(A) 2011-12 agricultural season



(B) 2012-13 agricultural season



Notes: Figures plot the mean cumulative fraction sold by each month, for farmers in the treatment and control group. 90% confidence intervals are shaded in grey. The red dotted line marks the start of the intervention.

A.2 Information Sharing

Table A5

Farmers' feelings about being "well informed" about market prices

	Baseline		Endline	
	(1)	(2)	(1)	(2)
Panel A: Urban markets				
Treatment	-0.440**	-0.113	0.822***	0.953**
	(0.180)	(0.340)	(0.178)	(0.444)
C2T * Control		-0.353		-0.607
		(0.622)		(0.720)
C2T * Treatment		-1.032		-0.868
		(0.697)		(0.705)
Difference		0.679		0.261
		(0.716)		(0.819)
N	628	628	622	622
Pseudo R^2	0.037	0.042	0.158	0.162
Panel B: Local markets				
Treatment	-0.437*	0.189	0.487***	1.261***
	(0.225)	(0.470)	(0.174)	(0.406)
C2T * Control		1.598**		0.301
		(0.683)		(0.712)
C2T * Treatment		0.203		-1.243*
		(0.970)		(0.647)
Difference		1.394		1.544*
		(1.026)		(0.807)
N	628	628	622	622
Pseudo R^2	0.171	0.187	0.169	0.178

Farmers were asked to respond to the following questions: "Do you feel that you are well informed about URBAN [LOCAL] market prices?" We present results from ordered probit regressions, where answers are coded as: 1 = "no, not at all"; 2 = "no, not very well"; 3 = "yes, fairly well"; 4 = "yes, very much". All regressions include strata fixed-effects. In the table above, we only include farmers that sell yam. "Difference" shows the linear combination (C2T * Control − C2T * Treatment). Huber-White robust standard errors clustered by community cluster are in parentheses.

*** Significant at 1% level. ** Significant at 5% level. * Significant at 10% level.

Table A6

Impact of connections to treated communuties on prices, controlling for information held

	Effect of C2T	Controlling for price information held	
		Self-reported measure	Objective measure
	(1)	(2)	(3)
Treatment	13.568	12.254	18.770
	(13.771)	(13.888)	(14.691)
C2T * Control	-4.031	-3.351	-1.702
	(23.558)	(23.599)	(24.855)
C2T * Treatment	40.801**	41.394**	50.110***
	(16.129)	(16.104)	(17.237)
Farmer feels informed		4.669	
		(6.265)	
Log of absolute % error			-2.060
			(1.480)
Observations	620	620	541
R^2	0.0621	0.0631	0.0686

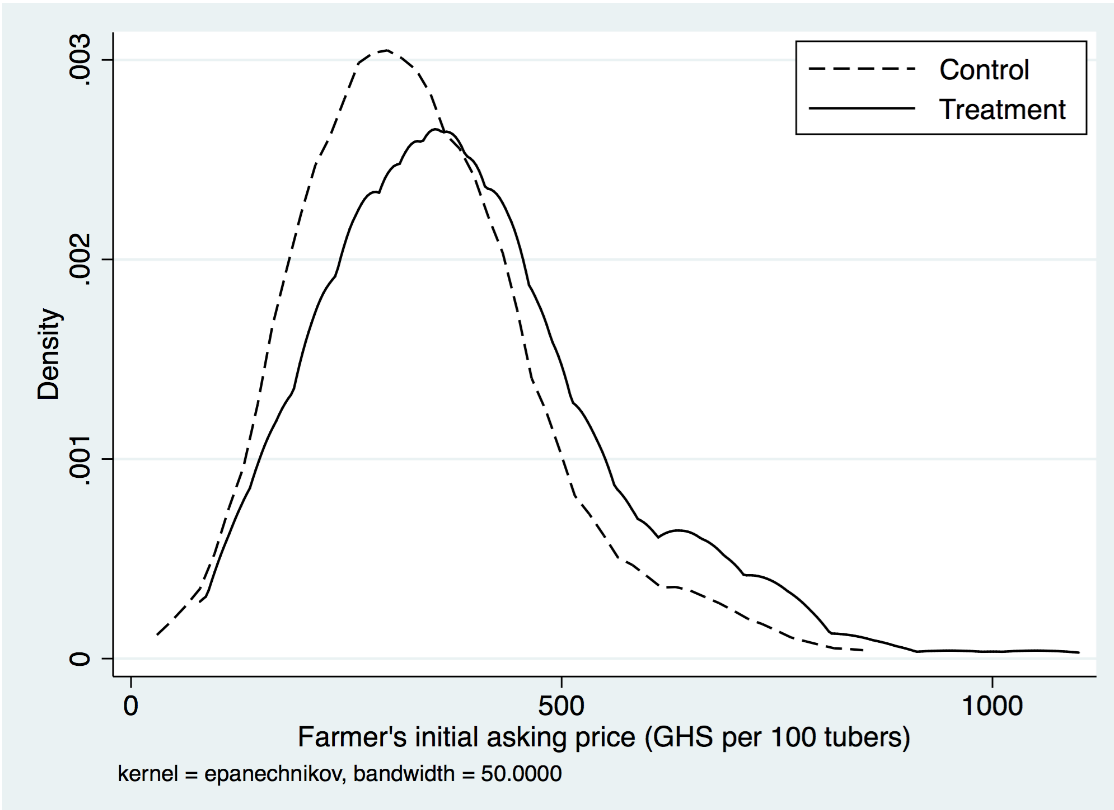
The effect of C2T on prices obtained by control farmers does not seems to significantly change when we control for information held, in Columns (2) and (3), as opposed to when we do not, in Column (1). Both Columns (2) and (3) use informed-ness as measured at the endline survey. Column (2) relies on the farmer's self reported feeling of informedness about prices in distant markets, while Column (3) relies on the log of the difference between the price provided in the Esoko alerts and the farmer's estimate at endline. All regressions include strata fixed effects, interview week fixed effects, and yam type fixed effects. Huber-White robust standard errors clustered by community cluster are in parentheses.

*** Significant at 1% level. ** Significant at 5% level. * Significant at 10% level.

A.3 Support for assumptions in bargaining spillovers model

FIGURE A3

Farmers' initial asking price in bargaining with traders



Notes: Distribution of initial asking price by farmers in negotiations with traders (yam sales only). Data is based on the midline survey, which asked farmers to recall an important transaction from the prior agricultural year. A Kolmogorov-Smirnov equality-of-distributions test rejects the null hypothesis of equality of distributions for the treatment and control group.

Table A7

Unsuccessful negotiations between farmers and traders for yam

	Number of buyers with whom farmer discussed sale (mean)	Number of buyers farmer actually sold to (mean)	Cases where farmer spoke to more traders than sold to (%)
Urban markets	3.90	2.29	78.3%
Local markets	3.96	1.89	72.5%
Home	2.61	1.57	47.3%
Farm Gate	1.86	1.22	16.7%
All locations	3.53	1.83	64.5%

Notes: Data on yam bargaining from monthly survey, pre-treatment period (Aug-Oct 2011).

B Further details on experimental design

B.1 Creation of "connectedness" indices

Market overlap index

The market overlap index measures the extent to which farmers in communities j and k overlap in their marketing activities. We asked each farmer to list up to three markets where they had sold their production in the previous agricultural season. We then used this information to identify the number of farmers in a given community that sell in each market. Let n_{jm} represent the number of farmers in community j that report selling in market m, and n_{km} represent the number of farmers in community k that report selling in market m. To come up with a measure of market overlap for communities j and k, we multiply n_{jm} and n_{km} together for each market m, and sum over all the possible markets:

$$mo_{jk} = \sum_{m=1}^{M} n_{jm} n_{km} \tag{1}$$

In this calculation, we ignore overlapping sales in Accra because we don't believe it is likely that farmers in our sample would actually encounter one another in the Accra market, or would otherwise be affected by the presence of farmers from other study communities.

Marketing communications index

In the baseline survey, we asked people to list up to two communities that they communicate with about their marketing. Farmers were also asked to provide details on:

- Frequency of communication: daily (which we code =1), weekly (=2), or occasionally (=3).
- Number of contacts in the community. The options were: many (=1), few (=2), or one (=3).

Let f_{njk} represent the frequency with which farmer n in community j communicates with people in community k, and c_{njk} represent the number of contacts that farmer n in community j has with people in community k. We take this information and construct a single measure of communication intensity, $s_{njk} = 7 - f_{njk} - c_{njk}$, which can range from 1 (lowest intensity) to 5 (highest intensity). We set s_{njk} equal to zero for all communities that are not mentioned by a farmer.

To construct our measure of marketing communications between communities j and k, we add together the sum of the s_{njk} for farmers in community j and the sum of the s_{nkj} for farmers in community k:

$$mc_{jk} = \sum_{n=1}^{N_j} s_{njk} + \sum_{n=1}^{N_k} s_{nkj} \tag{2}$$

Geographic proximity index

Finally, we use GPS coordinates for each community to identify the distance (as-the-crow-flies) between each community pair j and k. In our geographic proximity index, gp_{jk}, we multiply distances (reported in km) by negative 1 so that a larger number represents closer proximity.

B.2 Cluster formation

Once we calculated the three indices described above, we needed to find a way to combine them into a single measure of connectedness, c_{jk}, that we could use for cluster formation. We started by standardizing all indices to have a mean of 0 and standard deviation of 1. Next, we ran principal components analysis on the three standardized indices. We used the first principal component (which in our case, explains about 53% of the total variance in the data) to calculate a weighted average of our three indices. The weights generated through the principal components analysis were:

$$c_{jk} = 0.6381(gp_{jk}) + 0.4565(mc_{jk}) + 0.6201(mo_{jk}) \tag{3}$$

Finally, we chose a cut-off value for c_{jk}, above which communities j and k would be considered connected enough to warrant assignment to the same community cluster, and below which they would be kept in separate clusters. We combined our results for the c_{jk} and the anecdotal information we gathered during our field work to settle on a cut-off value of 6. This value ensured that communities we knew to be highly connected were grouped into the same cluster, but also kept the total number of community clusters large (90 in total).

C Proofs and additional details on the theorethical model

C.1 Existence and uniqueness of R^{I*}

We start by rewriting the informed farmer's discounted continuation value as:

$$R^I = \Phi E_v[max\{v, R^I\}] \tag{4}$$

where

$$\Phi = \frac{\beta w}{\beta w + (1-\beta)} \in (0,1) \quad \text{for } \beta, w \in (0,1)\,.$$

Consider the left-hand side and right-hand side of (4) each as functions of R^I. The left-hand side is the 45 degree line on a graph with R^I on the horizontal axis; the right-hand side is a function which is a constant $\Phi E_v[v]$ for $R^I \in [0, v_L]$, the increasing function $\Phi E_v[max\{v, R^I\}]$ for $R^I \in (v_L, v_H)$, and the constant Φv_H at $R^I = v_H$.

Given these general properties, we can now prove existence and uniqueness of R^{I*}.

Proposition A1. *The equilibrium discounted continuation value of the informed farmer, R^{I*}, is characterized by the following properties:*

(A) There is a unique value R^{I*} of R^I which satisfies (4).

(B) Assume $\Phi E_v[v] > v_L$. Then $R^{I*} \in (v_L, v_H)$ and is given by

$$R^I = \left[v_H + \frac{(v_H - v_L)(1-\beta)}{\beta w}\right] - \sqrt{\left[\frac{(v_H - v_L)(1-\beta)}{\beta w}\right]^2 + 2v_H\left[\frac{(v_H - v_L)(1-\beta)}{\beta w}\right]}.$$

(C) Assume $\Phi E_v[v] \leq v_L$. Then $R^{I*} = \Phi E_v[v]$.

Proof: (A) Define $\Delta\left(R^I\right) \equiv \Phi E_v[max\{v, R^I\}] - R^I$. Then $\Delta(0) = \Phi E_v[v] > 0$ and $\Delta(v_H) = \Phi v_H - v_H < 0$. Since $\Delta\left(R^I\right)$ is continuous in R^I, from the intermediate value theorem we know there exists a value R^{I*} such that $\Delta\left(R^{I*}\right) = 0$, which by definition is a solution to (4). Now we show uniqueness. Note that when $R^I \in [0, v_L)$, $\Delta\left(R^I\right) = \Phi E_v[v] - R^I$, which is strictly decreasing in R^I. To proceed, we note that[59]:

$$\text{for all } R^I \in (v_L, v_H) : \frac{\partial E_v[max\{v, R^I\}]}{\partial R^I} < 1.$$

This implies that $\frac{\partial \Delta\left(R^I\right)}{\partial R^I} < 0$ so again $\Delta\left(R^I\right)$ is strictly decreasing in R^I on (v_L, v_H). If $\Delta\left(R^I\right)$ is strictly decreasing in R^I then there can not be 2 points where $\Delta\left(R^I\right) = 0$ which proves the uniqueness of any solution to (4).

[59] Fix any $R^I, R^{I'} \in (v_L, v_H)$, and then consider $max\{v, R^{I'}\}$ and $max\{v, R^I\}$ as functions of v. Note that they only differ on $v \in [0, max\{R^I, R^{I'}\}]$ where they take values of either $R^{I'}$, R^I, or something in between. Hence $\mid E_v[max\{v, R^{I\prime}\}] - E_v[max\{v, R^I\}] \mid \leq \mid R^{I'} - R^I \mid Pr\left(v \in [v_L, Max\{R^I, R^{I\prime}\}\right)$. Hence, $\frac{|E_v[max\{v, R^{I\prime}\}] - E_v[max\{v, R^I\}]|}{|R^{I\prime} - R^I|} \leq Pr\left(v \in [v_L, Max\{R^I, R^{I\prime}\}]\right) < 1$. Letting $(R^{I\prime} - R^I) \to 0$ proves the claim.

(B) Suppose that $\mathrm{R}^I \in (v_L, v_H)$. Then

$$\begin{aligned} E_v[max\{v, R^I\}] &= \int_{v_L}^{R^I} R^I f(v)\, \mathrm{d}v + \int_{R^I}^{v_H} v f(v)\, \mathrm{d}v \\ &= R^I \left(\frac{R^I - v_L}{v_H - v_L}\right) + \frac{1}{2}\left(\frac{v_H^2 - \left(R^I\right)^2}{v_H - v_L}\right) \\ &= \frac{1}{2}\left(\frac{1}{v_H - v_L}\right)\left\{\left(R^I\right)^2 - 2R^I v_L + v_H^2\right\} \end{aligned}$$

Putting this in (4):

$$R^I = \Phi \frac{1}{2}\left(\frac{1}{v_H - v_L}\right)\left\{\left(R^I\right)^2 - 2R^I v_L + v_H^2\right\}$$

which is a quadratic equation in R^I. The two roots are:

$$R^I = \left[v_H + \frac{(v_H - v_L)(1-\beta)}{\beta w}\right] \pm \sqrt{\left[\frac{(v_H - v_L)(1-\beta)}{\beta w}\right]^2 + 2v_H\left[\frac{(v_H - v_L)(1-\beta)}{\beta w}\right]}.$$

The larger root is greater than v_H, which violates the assumption that $R^I \in (v_L, v_H)$. The smaller root is in the appropriate range (v_L, v_H). Thus the smaller root is the only feasible solution when $R^I \in (v_L, v_H)$.

(C) Follows immediately from the figure and the discussion earlier in the text.

C.2 Proof that $R^{I*} > R^{U*}(d)$ for all d

Here we show that, if an equilibrium value for R^{U*} exists, then it must be that $R^{I*}(d) > R^{I*}$ for all d.[60] The argument follows three steps:

(A) R^{I*} is weakly greater than $R^{U*}(d)$ for all values of d.

(B) The expected value of being the proposer is strictly higher for the informed farmer than for the uninformed farmer;

(C) Given (A) and (B), R^{I*} is strictly greater than $R^{U*}(d)$ for all values of d.

Proof: (A) is self-evident. As respondents, informed and uninformed farmers face the same price offer because traders cannot distinguish between the two types. As proposer, the informed farmer can always mimic the uninformed farmer's strategy and achieve payoffs that are at least as high.

(B) Let O^I be the expected value of being the proposer for the informed farmer, and let O^U be as defined in (8) in Appendix C.3. Given the informed farmer's optimal strategy, $O^I = E_v[max\{v, R^{I*}\}]$. Since O^I is always strictly greater than v_L, (B) always holds when the uninformed farmer is playing the corner solution (offering v_L). To see that (B) also holds when the uninformed farmer is playing the interior solution, note that, for all values of $R^U \geq 2v_L - v_H$,

[60] The previous section established that R^{I*} is independent of d. R^{U*}, however, is not.

$O^U(R^U)$ is strictly increasing in R^U. It follows that, for all d,:

$$\begin{aligned} O^I &= (1-F(R^{I*}))\frac{v_H + max\{v_L, R^{I*}\}}{2} + F(R^{I*})R^{I*} \\ &> \left(1 - F\left(\frac{v_H + R^{I*}}{2}\right)\right)\frac{v_H + R^{I*}}{2} + F\left(\frac{v_H + R^{I*}}{2}\right)R^{I*} \\ &= O^U(R^{I*}) \geq O^U(R^{U*}(d)) \end{aligned}$$

where the first inequality follows from $\frac{v_H + R^{I*}}{2} > R^{I*}$ and the second inequality follows from $R^{I*} \geq R^{U*}$ (proved in (A)) and the fact that O^U is strictly increasing.

(C) We prove this by contradiction. Assume that there exists a $\hat{d} \in [0,1]$ such that $R^{I*} = R^{U*}(\hat{d}) = \bar{R}$. Then the optimal strategy for the trader is to offer a price equal to $min\{v, \bar{R}\}$. It follows that we can write the continuation values of informed and uninformed farmers as:

$$\begin{aligned} R^{I*} &= \beta w O^I + \beta(1-w)\bar{R} \\ R^{U*}(\hat{d}) &= \beta w O^U + \beta(1-w)\bar{R}. \end{aligned}$$

If we subtract the two equations above from one another we get:

$$0 = \beta w(O^I - O^U(R^{U*}(\hat{d})) > 0$$

where the inequality follows from (B), leading to a contradiction.

C.3 Proof of Propositions 1 and 2 in the bargaining model

Proposition C.1 proves the existence of a unique fixed point R^{I*} to the informed farmer's Bellman equation. We now seek to prove the existence of a fixed point R^{U*} to the uninformed farmer's Bellman equation.

We start by re-writing the uninformed farmer's Bellman equation as:

$$R^U = Y\left(R^U\right) \tag{5}$$

where

$$Y\left(R^U\right) \equiv \beta\{wO^U\left(R^U\right) + (1-w)Z\left(d, R^U, R^{I*}\right)\} \tag{6}$$

and

$$Z(d, R^U, R^{I*}) \equiv [1 - F(M)]\, R^{I*} + F(M)R^U. \tag{7}$$

$Z(d, R^U, R^{I*})$ represents the expected value of being a respondent for an uninformed farmer believed to be informed with probability d, with a discounted continuation value of R^U, and for which R^{I*} is the unique fixed point to the informed farmer's Bellman equation.

Given the uninformed farmer's optimal strategy, the expected value of being the proposer can be written as:

$$O^U\left(R^U\right) = \begin{cases} v_L & \text{if} \quad R^U < 2v_L - v_H \\ \tilde{O}^U & \text{if} \quad R^U \geq 2v_L - v_H \end{cases} \tag{8}$$

where:

$$\begin{aligned} \tilde{O}^U\left(R^U\right) &= Pr(p^{int} \leq v)p^{int} + Pr(p^{int} > v)R^U \\ &= \frac{1}{v_H - v_L}\left\{\left(\frac{v_H + R^U}{2}\right)^2 - R^U v_L\right\}. \end{aligned}$$

The assumption that $2v_L - v_H < 0$ implies that the uninformed farmer proposer is always at an interior solution, so that $O^U\left(R^U\right) = \tilde{O}^U\left(R^U\right)$ for all R^U.

We now describe key properties of $\tilde{O}^U$ and Z.

Lemma A1. *Key properties of $\tilde{O}^U(R^U)$. For all R^U:*

(A) $\tilde{O}^U\left(R^U\right) > R^U$ and $\tilde{O}^U(0) > 0$.

(B) The slope of $\tilde{O}^U\left(R^U\right)$ is everywhere positive and less than 1.

(C) $\tilde{O}^U\left(R^U\right)$ is convex.

Proof: (A) follows from

$$\begin{aligned} \tilde{O}^U\left(R^U\right) - R^U &= \frac{1}{v_H - v_L}\left\{\left(\frac{v_H + R^U}{2}\right)^2 - R^U v_L\right\} - R^U \\ &= \frac{1}{v_H - v_L}\left\{\left(\frac{1}{4}\right)\left[\left(v_H - R^U\right)^2\right]\right\} > 0. \end{aligned}$$

Setting $\mathrm{R}^U = 0$ in the above proves the second part of (A).

(B)

$$\begin{aligned} \frac{\partial \tilde{O}^U\left(R^U\right)}{\partial R^U} = \; & \tfrac{1}{v_H - v_L}\left\{\left(\tfrac{1}{2}\right)\left(v_H + R^U\right) - v_L\right\} = \left(\tfrac{1}{2}\right)\tfrac{v_H + R^U - 2v_L}{v_H - v_L} \\ & = \left(\tfrac{1}{2}\right)\left(1 + \tfrac{R^U - v_L}{v_H - v_L}\right) \in (0, 1) \end{aligned}$$

(C)

$$\frac{\partial^2 \tilde{O}^U\left(R^U\right)}{\partial\left(R^U\right)^2} = \left(\frac{1}{2}\right)\frac{1}{v_H - v_L} > 0$$

Lemma A2. *Key properties of Z. Fix any $d < 1$ and suppose that $R^{I*} > v_L$:*

(A) Define:

$$\bar{R}^U(d) \equiv \frac{R^{I*} - dv_H}{1 - d}.$$

Then

$$\begin{aligned} Pr(V^{pooling}) > 0 \quad &\textit{for} \quad R^U > \bar{R}^U(d) \\ Pr(V^{pooling}) = 0 \quad &\textit{for} \quad R^U \leq \bar{R}^U(d) \end{aligned} \tag{9}$$

and

$$Z\left(d,R^U,R^{I*}\right)=\begin{cases}\tilde{Z}(d,R^U,R^{I*}) & \textit{for } R^U>\bar{R}^U(d)\\ R^U & \textit{for } R^U\le\bar{R}^U(d)\end{cases}$$

where

$$\tilde{Z}(d,R^U,R^{I*})\equiv\left(-\frac{\frac{1}{d}-1}{v_H-v_L}\right)\left(R^U\right)^2+R^U\left(\frac{(2-d)R^{I*}-dv_L}{d\,(v_H-v_L)}\right)+\frac{R^{I*}\left(dv_H-R^{I*}\right)}{d\,(v_H-v_L)}.$$

(B) Define:

$$\bar{d}\equiv\frac{R^{I*}}{v_H}.$$

Then $\bar{R}^U(d)\gtreqless 0$ *as* $d\lesseqgtr\bar{d}$ *and for* $d\le\bar{d}$, $\bar{R}^U(d)$ *is decreasing in* d. *When* $d>\bar{d}$, $\bar{R}^U(d)<0$ *so* $Pr(V^{pooling})>0$ *for all* $R^U\ge 0$.

(C) Z *is strictly increasing in* R^U *and weakly increasing in* d.

Proof: (A) $Pr(V^{pooling})>0$ whenever $M<v_H$. Since

$$\begin{aligned} v_H-M(d,R^U,R^{I*}) &= v_H-\left(R^U-\frac{R^{I*}-R^U}{d}\right)\\ &= \frac{1-d}{d}\left\{R^U-\bar{R}^U(d)\right\}\end{aligned}$$

we can conclude that

$$R^U>\bar{R}^U(d)\qquad\Longleftrightarrow M\left(R^U,d,R^I\right)<v_H$$

which proves (9). Next, when $R^{I*}>v_L$, it can be shown that $M>v_L$[61] so that:

$$F(M)=F\left(R^U+\frac{R^{I*}-R^U}{d}\right)=\frac{R^U+\frac{R^{I*}-R^U}{d}-v_L}{v_H-v_L}. \tag{10}$$

When $R^U>\bar{R}^U(d)$, replace the $F(M)$ in (7) with the expression in (10) to conclude, after algebraic simplification, that Z is equal to the value $\tilde{Z}$ defined above. When $R^U\le\bar{R}^U(d)$, $F(M)=0$ and thus $Z=R^U$.

(B) This follows from observing that $\bar{R}^U(d)=v_H\left\{\frac{\frac{R^I}{v_H}-d}{1-d}\right\}=v_H\left\{\frac{\bar{d}-d}{1-d}\right\}$.

(C) When $Z=R^U$, $\frac{dZ}{dR^U}=1$ and $\frac{dZ}{dd}=0$. When $Z=\tilde{Z}$:

$$\frac{\partial Z}{\partial R^U}=\frac{1}{v_H-v_L}\left\{2\left(R^I-R^U\right)\left(\frac{1}{d}-1\right)+R^I-v_L\right\}>0$$

and

$$\frac{\partial Z}{\partial d}=\frac{(R^{I*}-R^U)^2}{d^2(v_H-v_L)}>0.$$

Finally, define:

$$L(R^U)=\beta\left\{wO^U(R^U)+(1-w)R^U\right\}.$$

$L(R^U)$ represents the discounted continuation value of the informed farmer at any R^U when the probability of pooling

[61] Since $\frac{dM}{dR^U}<0$, we know that $M(d,R^U,R^{I*})\ge M(d,R^{I*},R^{I*})=R^{I*}>v_L$ for all $R^U\in[0,R^{I*}]$.

is zero. Since $\frac{d\tilde{O}^U}{dR^U} \in (0,1)$ from Lemma A1, $\frac{dL}{dR^U} \in (0,1)$ as well. Further, $L(0) > 0$ and $L(R^{I*}) < R^{I*}$[62], so there exists a unique fixed point of L, $R^{UL} > 0$, such that $L(R^U) \gtreqless R^U$ as $R^U \lesseqgtr R^{UL}$. This is illustrated graphically below in Figure A4.

Next, note that $\bar{R}^U(d)$ is strictly decreasing in d. Define d^{UL} to be the unique value of d such that $\bar{R}^U(d) = R^{UL}$. Since $R^{UL} > 0$, $\mathrm{d}^{UL} \in (0, \bar{d})$. Since $Z(d, R^{I*}, R^{I*}) = R^{I*}$, it is easy to check that $Y\left(R^{I*}\right) = L\left(R^{I*}\right) < R^{I*}$.

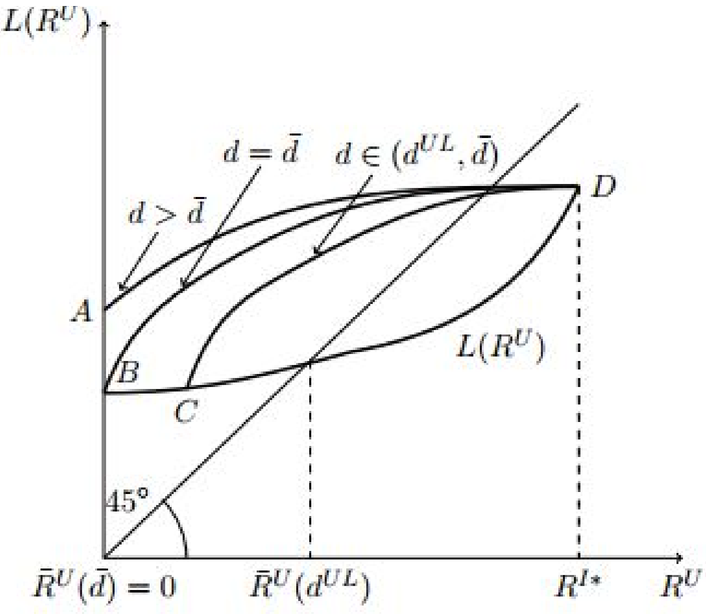
We can now characterize the fixed points of Y. Define:

$$\tilde{Y}\left(R^U\right) \equiv \beta\{w\tilde{O}^U\left(R^U\right) + (1-w)\tilde{Z}\left(d, R^U, R^I\right)\}$$

Since $\tilde{O}^U$ and $\tilde{Z}$ are quadratic in R^U so too is $\tilde{Y}$. By our assumption that $2v_L - v_H < 0$, we know that $O^U = \tilde{O}^U$ for all R^U. Thus, in cases where there is a positive probability of pooling, $Y = \tilde{Y}$. In cases where there is zero probability of pooling, $Y = L(R^U)$. To summarize:

$$\begin{aligned} &(1) \text{ For } d \in [\bar{d}, 1],\ Y\left(R^U\right) &&= \tilde{Y}\left(R^U\right) \text{ for all } R^U \\ &(2) \text{ For } d \in (0, \bar{d}),\ Y\left(R^U\right) &&= \begin{cases} L\left(R^U\right) & \text{for } R^U \in [0, \bar{R}^U(d)] \\ \tilde{Y}\left(R^U\right) & \text{for } R^U \in \left(\bar{R}^U(d), R^{I*}\right) \end{cases} \end{aligned}$$

FIGURE A4: The Y function for $d \in (d^{UL}, 1]$



Consider Figure A4 above. The curve AD represents the Y function for a given $d \in (\bar{d}, 1]$; curve BD represents the Y function when $d = \bar{d}$; and the curve BC along L and CD along $\tilde{Y}$ represents the Y function for a given

[62] From Appendix C.2, we know that $O^I(R^{I*}) > O^U(R^{I*})$. Therefore, $L(R^{I*}) = \beta\{wO^U(R^{I*}) + (1-w)R^{I*}\} < \beta\{wO^I(R^{I*}) + (1-w)R^{I*}\} = R^{I*}$.

$d \in \left(d^{UL}, \bar{d}\right)$. Recall also that $\tilde{Y}$ is quadratic so it is either concave as drawn in Figure A4, linear or convex. At each of points A, B, and C we have $\tilde{Y}\left(R^U\right) > R^U$. Also $\tilde{Y}\left(R^{I*}\right) < R^{I*}$. Lemma A3 below implies that in each of these cases Y admits a unique fixed point R^{U*} in $(0, R^{I*})$.

FIGURE A5: The Y function for $d \in (0, d^{UL}]$

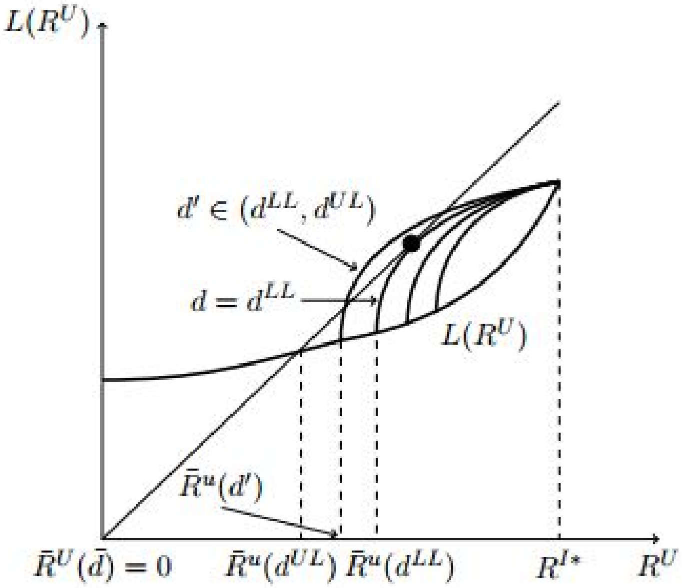


Figure A5 gives examples of Y for $d \in (0, d^{UL}]$. Y is equal to L at all R^U from 0 to up a point $\bar{R}^U(d) > \bar{R}^U\left(d^{UL}\right)$ and thereafter it becomes equal to the function $\tilde{Y}\left(R^U\right)$. The $\tilde{Y}\left(R^U\right)$ function is quadratic in R^U, so it is either concave (as drawn in Figure A5), linear or convex and it shifts up as d gets smaller. Suppose that for some $d < d^{UL}$,

$$\tilde{Y}\left(R^U\right) < R^U \text{ for all } R^U > R^{UL}. \tag{11}$$

One can show that for all d sufficiently small, (11) will hold.[63] Hence $\mathrm{d}^{LL} > 0$. Since $\tilde{Y}\left(R^U\right)$ is increasing in d, this means that (11) is also true for all $d' < d$. Define d^{LL} to be the supremum of all $d < d^{UL}$ such that (11) holds. If (11) holds for all $d < d^{UL}$ then $d^{LL} = d^{UL}$. For all such d values, there is only fixed point at $R^U(d^{UL})$.

For cases when $d^{LL} < d^{UL}$, fix any $d \in (d^{LL}, d^{UL})$ and define R^{LL} as the value of R^U where $\tilde{Y}(d^{LL})$ is tangent to the 45 degree line. Then, using Figure A5 as a guide, it should be clear that $\tilde{Y}\left(\bar{R}^U(d), d\right) = L\left(\bar{R}^U(d)\right) < \bar{R}^U(d)$ and $\tilde{Y}\left(R^{LL}, d\right) > \tilde{Y}\left(R^{LL}, d^{LL}\right) = R^{LL}$ so from Lemma A4 there is one fixed point of $\tilde{Y}$ on $(\bar{R}^U(d), R^{LL})$. Similar arguments show that there is one fixed point on (R^{LL}, R^{I*}). In particular, when $d \in (d^{LL}, d^{UL})$, $\tilde{Y}$ has three fixed points: one at $\bar{R}^U\left(d^{UL}\right)$, one in $(\bar{R}^U(d), R^{LL})$, and one in (R^{LL}, R^{I*}).

Lemma A3. *Let f: $[x_1, x_2] \rightarrow [x_1, x_2]$ be continuous and increasing with slope everywhere strictly less than 1 (i.e., for all $x' < x''$, $f(x'') - f(x'))/(x'' - x') < 1$. Suppose that f($x_1$) $> x_1$ and f(x_2) $< x_2$. Then f admits a unique fixed*

[63]To see this note that $\bar{R}^U(d) \rightarrow R^{I*}$ as d$\rightarrow$ 0, so since $\mathrm{L}\left(R^{I*}\right) < R^{I*}$, we we can choose d small enough so that $\bar{R}^U(d) > L\left(R^{I*}\right)$. Since $\tilde{Y}$ is increasing, for $\mathrm{R}^U \geq \bar{R}^U(d), \tilde{Y}\left(R^U\right) \leq \tilde{Y}\left(R^{I*}\right) = L\left(R^{I*}\right) < \bar{R}^U(d) < R^U$.

point on $[x_1, x_2]$.

Proof: Existence of a fixed point follows from the intermediate value theorem. If there are two fixed points the slope between those points is equal to one and not strictly less than one, which is a contradiction which proves uniqueness of the fixed point.

Lemma A4. *Let* f: $[x_1, x_2] \rightarrow [x_1, x_2]$ *be continuous and either everywhere strictly concave or everywhere strictly convex or everywhere linear. Suppose that either* $f(x_1) > x_1$ *and* $f(x_2) < x_2$ *or* $f(x_1) < x_1$ *and* $f(x_2) > x_2$*. Then* f *admits a unique fixed point on* $[x_1, x_2]$.

Proof: From the intermediate value theorem, we know that f admits a fixed point. First suppose that f is everywhere concave or everywhere linear. Let x^* be the smallest fixed point. Then $f(x) - x$ will be strictly positive for all $x < x^*$. Since f is concave there will be a linear hyperplane $L(x^*)$ supporting f at x^* - a linear function such that $L(x^*) = f(x^*)$ and $f(x) \leq L(x)$ for all x (when f is linear, $f = L$). Since $f(x_1) > x_1$, $L(x_1) > 0$ and so the slope of L is strictly less than one. This in turn means that $L(x) < x$ for all $x > x^*$ so $f(x) < x$ for all such x, and hence there can not be any fixed point at $x > x^*$. This proves the uniqueness of the fixed point when f is everywhere strictly concave or everywhere linear.

Next suppose that f is everywhere strictly convex. Suppose there exists another fixed point and let x^{**} be the largest fixed point. Let $L(x)$ be the supporting hyperplane of f at x^{**}. Then $f(x) > L(x)$ for all $x \neq x^*$. Since $f(x_2) < x_2$ by assumption, the slope of L will have slope strictly less than one. This implies that $L(x) > x$ for all $x < x^{**}$ so $x^* < L(x^*) < f(x^*)$, which is a contradiction to the fact that x^* is a fixed point. Hence there is only one fixed point when f is strictly convex.

C.4 Proof of Corollary 1

Pooling occurs when $v \geq M$. Hence:

$$\frac{\partial \pi}{\partial d} \geq 0 \;\Leftrightarrow\; \frac{\boldsymbol{d}M}{\boldsymbol{d}d} = \left(\frac{d-1}{d}\right)\frac{\partial R^{U*}}{\partial d} - \frac{R^I - R^{U*}}{d^2} \leq 0$$

Where the last inequality is always verified if $\frac{\partial R^{U*}}{\partial d} \geq 0$.

C.5 Proof of Proposition 3

(A) Informed farmer's price $P^I(d)$ *is decreasing in* d.

We want to establish the negative relationship between d and $P^I(d)$, where the latter is given by:

$$P^I(d) = (1 - \mu^I(d))\frac{v_H + R^{I*}}{2} + \mu^I(d)R^{I*}$$

Since $\frac{v_H + R^{I*}}{2} > R^{I*}$, $P^I(d)$ is decreasing in $\mu^I(d)$. Hence, it is sufficient to prove that $\mu^I(d)$ is increasing in d. The intuition is that the probability that the trader is the proposer in a successful bargaining round depends positively on the probability of pooling which is increasing in d from Corollary 1. The formal derivation of

$\mu^I(d)$ is as follows. When the farmer is the proposer trading occurs with probability $(1 - F(R^{I*}))$. When the trader is the proposer trading occurs only under pooling, and therefore with probability $\pi(d)$. Hence the total probability that a bargaining round is successful is $w(1 - F(R^{I*})) + (1 - w)\pi(d)$. It follows that $\mu^I(d) = \frac{(1-w)\pi(d)}{(1-F(R^{I*}))w+(1-w)\pi(d)}$ which is increasing in $\pi(d)$, hence in d from Corollary 1, completing the proof.

(B) Uninformed farmer's price $P^U(d)$ is increasing in d.

Define as $\nu(d)$ the probability that the uninformed farmer sells in the current bargaining round prior to knowing who is the proposer. The agreement is reached when $v \geq R^{U*}(d)$ if the trader proposes and if $v \geq \frac{v_H+R^{U*}(d)}{2}$ if the farmer proposes. Hence we have:

$$\nu(d) = w\, pr\left(v \geq \frac{v_H + R^{U*}(d)}{2}\right) + (1 - w)pr\left(v \geq R^{U*}(d)\right)$$

Hence $\nu(d)$ is decreasing in d (a raise in d increases R^{U*} which reduces the likelihood that the agreement is reached both when farmers are proposers and respondents). The continuation value of the farmer can be expressed as a function of ν and $P^U(d)$ as follows:

$$\frac{1}{\beta}R^{U*}(d) = \nu(d)P^U(d) + (1 - \nu(d))R^{U*}(d)$$
$$\Rightarrow P^U(d) = R^{U*}(d) \cdot \frac{1 - \beta + \beta\nu(d)}{\beta\nu(d)}$$

Since $R^{U*}(d)$ and $\frac{1-\beta+\beta\nu(d)}{\beta\nu(d)}$ are both increasing in d, $P^U(d)$ must also be increasing in d. Intuitively, if an uninformed farmer with higher d delays sales more often but has a higher continuation value, it must be the case that she receives higher prices conditional on sales.

(C) The difference $P^I(d) - P^U(d)$ is decreasing in d.

Follows immediately from (A) and (B).