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

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**Abstract**

We conducted a randomized field experiment to determine the impact of providing commodity price information to rural farmers, delivered via text messages on their mobile phones. Using a novel index of inter-village communication networks, we show that the intervention: (1) had a sustained, positive impact on the prices received by treatment group farmers, and (2) had substantial indirect benefits on the prices received by certain control group farmers. These indirect (spillover) benefits cannot be fully explained by information sharing between treatment and control farmers. Rather, we provide evidence that spillover effects are mainly generated by changes in traders' bargaining behaviors brought about by the intervention, via a phenomenon we refer to as "bargaining spillovers." After accounting for spillovers, we estimate that the price alerts led to a 9% increase in prices for the treatment group, and a comparable increase in prices for the control group. The direct return on investment of the service exceeds 200%, a result that underscores the huge potential of ICT interventions in emerging markets. Importantly, had we ignored the potential for spillovers, we would have reached the erroneous conclusion that the intervention had no long-run benefit for farmers.

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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 that do not have access to price information, and on market outcomes as a whole? These are important questions to answer given the growing interest in ICT-related informational interventions by policymakers, foundations, and governments around the world.

This paper reports results from a two-year randomized evaluation of an SMS-based market information system (MIS) in Ghana. Our study involved 1,000 smallholder commercial farmers in the northern part of the Volta region, who we followed for two years between 2011 and 2013. Working with a local technology firm, our intervention consisted of enrolling treatment group farmers in a price alert service that sent them weekly text messages with local and urban market prices for their main commercial crops. We evaluated the impact of the price alerts on the prices received by treatment farmers, as well as the indirect effects of the price alerts on the control group.

Our 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, we show that the price alerts had large positive effects on the prices received by treatment group farmers, and that these effects are sustained over time. We estimate that the alerts led to a 8-9% increase in yam prices over the course of the study. We show that these effects—which are on the high end of what others have found in comparable evaluations—are driven by improvements in farmers' bargaining outcomes with traders, rather than changes in where farmers sell crops or the timing of sales over the agricultural season.[1]

Second, we use novel data we collected on inter-village communication and marketing networks

[1] Previous economic evaluations of similar interventions have come to mixed conclusions on the benefits of these services for farmers. RCT evaluations of MIS in Columbia (Camacho and Conover, 2011) and India (Fafchamps and Minten, 2012; Mitra, Mukherjee, Torero, and Visaria, 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 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).

to identify large positive spillover effects on control group farmers. These spillover effects begin to appear several months after the start of the intervention and increase over time so that by Year 2, the indirect benefit of the price alerts for control farmers is comparable in magnitude to the direct benefit for the treatment group (about a 9% increase in prices). We show how a naïve estimate of the treatment effect that ignores these spillovers leads to the erroneous conclusion that the price alert service has no long-run benefit to farmers.

Finally, we use theoretical and empirical methods to identify the specific mechanism through which spillover effects occurred. The richness of our data allows us to rule out information spillovers from treatment to control group farmers as the primary causal mechanism. Instead, we develop and empirically validate a model of "bargaining spillovers" that can explain the indirect benefits for the control group. The basic idea captured in the model is as follows: our intervention caused traders to start interacting with informed (treatment) farmers, who used the price alerts to extract higher prices from traders. Exposure to the treatment group altered traders' beliefs about the information set of other farmers with whom they interacted, including control farmers. In cases where traders thought it was likely that they were facing an informed farmer, they adjusted their bargaining strategy accordingly and offered higher prices, irrespective of whether or not the farmer was actually informed. While most of the model's predictions are indistinguishable from an information spillovers story, there is one prediction that cannot be rationalized by information spillovers alone, and we find empirical support for this prediction in our data. Taken together, our results support the hypothesis that bargaining spillovers are the main mechanism behind the indirect benefits for control group farmers.

Our analysis of the spillover effects of the price alerts advances the experimental literature looking at the indirect effects of interventions,[2] particularly in the realm of agriculture, where few experimental studies have ventured beyond looking for evidence of informational spillovers among farmers in the same village.[3] Our paper finds large indirect effects *across* villages, through a

[2]This literature covers a range of interventions in areas such as disease control (Miguel and Kremer, 2004), labor markets (Crépon, Duflo, Gurgand, Rathelot, and Zamora, 2013), and elections (Asunka, Brierley, Golden, Kramon, and Ofosu, 2014; Giné and Mansuri, 2011).

[3]One exception is Burke (2014), who looks at the general equilibrium effects of a loan intervention for maize farmers in Kenya. A non-experimental paper that has ventured beyond looking for evidence of information spillovers is Svensson and Yanagizawa-Drott (2012), which looks at the partial and general equilibrium effects of a national MIS in Uganda. In contrast to our results, in their case uninformed farmers lose out in general equilibrium, due to reductions in urban market prices (and corresponding reductions in farm-gate prices) brought about by changes in the marketing behaviors of informed farmers. The differences between their findings and ours are likely attributable

mechanism other than information sharing, and uses a novel technique to identify these spillovers. 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. Our results imply significant benefits of the MIS we studied: for a typical yam farmer selling 1,200 tubers in a year, a 9% increase in prices translates into an additional 170 GHS (US$114) in annual revenue.[4] Given that farmers tend to operate on low margins, the impact is likely to be considerably larger when measured in terms of a percentage increase in farm profits. Importantly, the low cost of delivering information via SMS means that the intervention is also extremely attractive from a return on investment perspective.

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 some descriptive statistics. Section 4 presents our estimates of the treatment effect under the assumption of zero spillovers. Section 5 presents evidence of indirect benefits for control group farmers, and Section 6 presents the model of bargaining spillovers and the supporting empirical evidence. Section 7 presents estimates of de-biased treatment effects. Section 8 concludes.

# 2 Background and Experimental Design

## 2.1 Agricultural marketing in Ghana

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 are individuals—often women—who travel around the country purchasing agricultural output from farmers, and then transport this output to urban markets to sell.[5] Transactions between farmers and traders

to the differing scales of each MIS as well as differences in the marketing environment being studied.

[4]The median quantity of yams sold in agricultural year is 1,200 in our sample. Figures are denominated in real, August 2011 Ghana Cedis and their corresponding US dollar values at prevailing exchange rates in August 2011.

[5]There are also small-scale, local traders, who aggregate crops to re-sell to the large traders. Our focus is on the large traders, since they are the dominant actors in the market and the individuals that farmers identify as cheating them in negotiations.

usually take place at the farm gate, in the local community, or in the local market. They are conducted in an informal manner[6] and involve some amount of bargaining between the parties. The degree to which bargaining takes place varies by crop, as described further below.

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 by traders, and cite examples of traders telling them urban prices are low (which farmers are unable to verify) in order to buy at a low price. Given the information asymmetry that exists between farmers and traders, one potentially viable way to help farmers secure higher prices is by providing them with better price information. Of course, the value of an informational intervention depends on the existing market conditions. One necessary condition for observing an impact of price information is that traders are not already operating at zero-profit (Jensen, 2010). This condition is likely to be met in our study area, where traders are best described as operating under oligopoly conditions. Barriers to entry are high, since trading requires access to capital and a network of farmers with which to transact. However, the market is not monopolistic: in each agricultural season, the typical farmer in our study area sells on average to 3-5 different traders (see Table I). About half of these traders are individuals with whom farmers have a long-standing relationship.[7] Given this environment, price information has the potential to have a positive impact on farmers' bargaining outcomes with traders.

[ INSERT TABLE I HERE ]

Details on the way in which farmers and traders transact vary considerably by type of crop. Some key differences are illustrated in Table I. One crop that stands out across several dimensions is yam, the crop that is the focus of our study. 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 homogenous among sellers in the local market, and farmers often report paying the prevailing "market price" for their production. This is not the case for yam, a crop that farmers tell us has no reference "market price." Instead, the farmer's ability to successfully

[6]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, Udry, Al-hassan, and Seshie, 2012).

[7]Price information could prove ineffective if farmers rely on long-standing relationships with traders to obtain credit or access to inputs, and are not able to establish similar relationships with new buyers. For example, 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. In our field work, farmers in our study did not express concerns of this type.

negotiate with the trader is a crucial determinant of the final price. Another disparity we observed during our field work is that most yam trading takes place the day before the actual market day, in a separate area of the marketplace.[8] Finally, yam is the only crop that is sold in urban markets by a non-negligible proportion of farmers, which suggests that the value of turning down a trader's low offer in favor of selling directly in the urban market may be higher for yam than for other crops. These distinctive features of yam marketing are likely contributing explanations to our finding that the MIS had a significant effect on yam prices, but no effect on prices for other crops.

## 2.2 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.[9] Within the study area, we sampled 100 communities located within four contiguous districts.[10] From each community, we sampled 10 farmers to be included in the study among those who market at least some portion of his or her 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 was designed to (1) minimize the risk of information spillovers while also (2) ensuring balance between treatment and control groups.[11] To minimize spillovers, we opted for a design that groups highly-connected communities together into what we call a "community cluster," and then randomizes at the community cluster level. In order to form community clusters, we collected data on inter-village marketing and communication network in the baseline survey prior to the start of our study. We used this information to construct three measures of "connectedness" for each village pair $j$ and $k$:

1. *Market overlap index*: measuring the degree to which farmers in villages $j$ and $k$ sell in the same markets

[8] We also observed that large-scale traders were significantly more active in the purchase of yam than they were of other crops.

[9] 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.

[10] 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.

[11] A well-known tradeoff 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, Glennerster, and Kremer, 2007).

2. *Marketing communications index*: measuring the degree of communication about agricultural marketing between farmers in villages $j$ and $k$

3. *Geographic proximity index*: measuring the geographic distance between two villages $j$ and $k$

We used principal components analysis to create a single "connectedness index" out of the three indices listed above. The connectedness index provides a scalar measure of connectedness, $c_{jk}$, for each village pair $j$ and $k$ in the study. Higher values denote more connected village pairs, and lower values denote less connected village pairs. To form community clusters, we selected a cut-off value for $c_{jk}$, above which village pairs were put into the same cluster, and below which village pairs were kept in separate clusters. In order to preserve balance and power, we chose a fairly low cut-off value, which resulted in moving from 100 communities to 90 community clusters.[12]

In addition to informing our randomization, the connectedness index allows us to investigate the indirect effects of the intervention ex-post. As discussed in Section 5, we use this index to construct a measure of each community's "connectedness" to treatment group villages (what we call "C2T"), to investigate these indirect effects.

After creating community clusters, we carried out our randomization. To ensure balance, we stratified on two variables: district (Nkwanta North, Nkwanta South, Krachi East, Krachi West), and most commonly-grown crop (yam, or not yam). Within each strata, we randomly assigned half of the community clusters to the treatment group, and half to the control group. The procedure resulted in 45 clusters (49 villages) in the treatment group and 45 clusters (51 villages) in the control group.

**Details about the treatment**

Farmers in the treatment group were trained and given a free subscription to an MIS operated by a privately-held technology company called Esoko. The MIS provides weekly price alerts to subscribers via SMS (text message).[13] We registered farmers to receive price alerts for their two main commercial crops, for four local markets in the study region and four of the main urban

[12]For more detail on this procedure, see Appendix A.

[13]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 keep enumerators sharp. 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 16 countries across the African continent.

markets in the country.[14] Enrolled farmers started receiving weekly price alerts in late October 2011.[15] Since most markets in the country are weekly, this should in theory provide farmers with the most up-to-date price information available.

Farmers in the control group were not provided with trainings or a subscription to the price alert service. However, they were surveyed with the same frequency as treatment farmers in the treatment group.

## 3 Data

Over the course of the study, we gathered extensive data on farmers and their marketing behaviors, which enables us to understand in great detail the impact of the intervention. In order to understand our main question of interest—the impact of the price alerts on producer prices—we gathered monthly transactional data for all farmers in the study. This data, covering the period August 2011 through June 2013, provides information about every sales transaction conducted by the farmer for his two main commercial crops (quantity and variety sold, total revenue, price per unit, place of sale, and type of buyer).[16] We supplement this transactional information with annual surveys covering a wide range of topics, including demographic traits, sources of information about marketing and prices, and general marketing behaviors. The three annual surveys we conducted are: (1) a baseline survey in July-August 2011 (prior to the start of the intervention); (2) a midline survey in July-August 2012 (about nine months after the start of the intervention); and (3) an endline survey 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.

[14]The four urban markets 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.

[15]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.

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

### 3.1 Descriptive Statistics and Balance

Table II reports baseline summary statistics for the full sample and separately by treatment status, as well as tests for balance between the treatment and control groups. Overall, the variables are well balanced between treatment farmers and control farmers.

[ INSERT TABLE II HERE ]

In the full sample, farmers 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.[17] 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 low: 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, which is consistent with the fact that most farmers actively sell in local markets.

## 4 Impact on prices under the assumption of no spillovers

To measure the impact of the price alerts on producer prices, we start by estimating the treatment effect under the Stable Unit Treatment Value Assumption (SUTVA) typically invoked in RCT evaluations. This assumption says that "the potential outcomes for each person $i$ are unrelated to the treatment status of other individuals" (Angrist, Imbens, and Rubin, 1996). An important implication of this assumption is that there can be no spillovers from the treatment that end up affecting the prices of control group farmers. Under this assumption, we can estimate the causal effect of the price alerts with the following regression:

$$p_{ijt} = \lambda + \kappa T_j + X'_{ij}\psi + \omega_k + \omega_t + e_{ijt} \tag{1}$$

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

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, $\omega_k$ are randomization strata fixed effects (see Bruhn and McKenzie, 2009), $\omega_t$ are time period fixed effects, and $X_{ij}$ is a set of additional covariates. The coefficient $\kappa$ estimates the effect of the price alerts on an Intent-to-Treat (ITT) basis. It is an unbiased estimate of the treatment effect so long as SUTVA is not violated. By including randomization strata fixed effects and time period fixed effects in our regressions, the treatment effect is identified from within-period, within-strata variation between treatment and control groups.

We estimate (1) separately for Year 1 (November 2011-June 2012) and Year 2 (July 2012-June 2013). The results using Year 1 data provide an estimate of the short-run treatment effect, and the Year 2 data provide an estimate of a longer-run treatment effect. We also combine all the data (including three months of pre-treatment data, from August 2011-October 2011) to estimate the following pooled regression:

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

where $Y_s$ is an indicator for period $s$, $s \in \{0, 1, 2\}$ (pre-treatment, Year 1, and Year 2). In this regression, the $\lambda_s$ measure the average control group price in period $s$, and the $\kappa_s$ measure the average treatment effect in period $s$. As in (1), the $\kappa_s$ are unbiased estimates of the treatment effect on producer prices on an ITT basis, so long as SUTVA holds.

We estimate both (1) and (2) separately for each crop, due to the fact that there are important differences in the marketing environment across crops, and therefore there is likely to be treatment effect heterogeneity. In the paper, we focus on the impact of the intervention on yam, which is the most important crop for farmers in our study area. As we discuss below, it is also the only crop for which we find any evidence of a treatment effect.

### 4.1 Results for yam

Table III presents results from the estimation of (1) and (2) using the monthly sales data for yam. 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. The first two columns present results using the data from Year 1, the second two columns present results using data from Year 2, and the final two columns present results using the pooled data. For each cut of data, we present results from two different specifications: one that only controls for strata fixed effects, time fixed effects, and yam type; and a second that also includes additional covariates (gender, asset index, and the community's distance to the closest district market).

[ INSERT TABLE III HERE ]

For Year 1, we find strong evidence of a positive and statistically significant treatment effect on farmers' yam prices. Using the pooled data in column (6), the estimated treatment effect in the first year is about 8.73 Ghana Cedis per 100 tubers, or, from the log specification, a 5.0% increase in prices. Once the additional covariates are added to the regression, the Year 1 results are significant at the the 5% level using both price levels and logs. In contrast, the Year 2 treatment effect is small in magnitude and never significantly different from zero. Again looking at column (6), the estimated Year 2 treatment effect is -0.01 GHS per 100 tubers, or -0.8% in the log specification.[18]

We also looked at the impact of the treatment on quantities of yam sold, and found no significant impact at any point in time (see Appendix B). Thus, our estimated treatment effects on prices can also be thought of as treatment effects on farmers' revenues from crop marketing.

[ INSERT FIGURE I HERE ]

The results in Table III suggest that, under the assumption of zero spillovers, there was an initial positive treatment effect of the alerts that disappeared in the second year. We explore this further by using non-parametric methods (fan regressions) to look at the evolution of yam prices for the treatment and control groups over time.[19] The top panel in Figure I plots yam prices for the treatment and control groups, as estimated using fan regressions after controlling for strata, type of yam, and the additional covariates. The bottom panel plots the difference between the treatment and control groups, with the bootstrapped 95% confidence interval shown in grey. The figure demonstrates that, immediately following the introduction of the price alerts, there was a

[18] 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.

[19] 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).

large difference between treatment and control group prices: over 20 GHS per 100 tubers, more than twice the estimated Year 1 treatment effect in Table III. The difference steadily declines over time so that five to six months after the start of the intervention, it is no longer significantly different from zero.[20] Thus, it appears that the estimated treatment effect under the zero spillovers assumption gradually declines and eventually disappears. Most of the remainder of the paper is focused on understanding this puzzling result. First, however, we discuss the mechanisms behind the (initial) treatment effect, and our rationale for why we fail to find any impact of the alerts on the prices for other crops.

### 4.2 Bargaining and lack of treatment effects for other crops

Given the evidence of a positive treatment effect for yam (at least in the first several months of the intervention), the next step is to understand the mechanism through which this effect occurred. There are a number of candidate mechanisms, including: changes in farmers' bargaining outcomes with traders, changes in where farmers sell their crops, and changes in the timing of farmers' sales over the course of the agricultural season. Prior to the start of our study, we anticipated that bargaining would be the main mechanism behind any positive treatment effect. This prediction was partly based on our discussions with farmers already using the service in other parts of the country. It was also based on the fact that transport to distant markets is difficult for farmers, since it entails taking on substantial risk and requires knowledge of how to arrange transport. Significant changes in timing of sales seemed unlikely because many farmers face liquidity constraints that force them to sell closer to the harvest season, rather than wait until the dry season when prices are higher.

Consistent with our prediction, there is very little evidence that the price alerts led treated farmers to make substantial changes in place of sale or the timing of sales (see Appendix C for this analysis). This leaves bargaining as the likely mechanism at play. Empirical support in favor of the bargaining mechanism comes from the annual surveys, where we asked farmers to recall the details of an important transaction from the previous agricultural season. For this transaction, we gathered information on: month and place of sale, quantity sold, whether bargaining took place,

[20]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.

the farmer's initial price offer[21], the price he expected to receive, and the final sale price. If the alerts affected farmers' bargaining behavior, then initial offers made by treatment farmers should be more positively correlated with Accra prices than the initial offers made by control farmers. We investigate the correlation between farmers' initial price offers and Accra prices in Table IV. Panel A shows the relationship between farmers' initial asking prices and average Accra prices in the month that the transaction took place, as estimated via multivariate regression. The results indicate a positive and statistically significant relationship between Accra prices and asking prices for *both* treatment and control groups. Although the interaction term between Accra prices and the treatment dummy is positive, indicating a stronger relationship for treatment farmers, 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, which seems plausible given that many of these farmers have significant experience selling yam. A more appropriate test of the bargaining mechanism, then, 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 do this, 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. We then look at how deviations from this predicted price series relate to farmers' initial price offers. These results, which are shown in Panel B, reveal a striking difference between treatment and control farmers. There is no significant relationship between the Accra price deviations and price offers made by control farmers. In contrast, there is a very high and statistically significant relationship between Accra price deviations and initial offers for the treatment group. 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.[22]

[ INSERT TABLE IV HERE ]

The fact that we failed to find price effects for other crops that we study provides further indirect evidence in favor of the bargaining channel (see Appendix B for these results). As described earlier, a key difference between yam marketing and marketing of other crops is the degree to which

[21] According to our surveys, yam farmers almost always make the first price offer in negotiations with traders.

[22] Note that this is consistent with what farmers reported to us in the annual surveys: at midline, 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.

transactions involve bargaining between traders and farmers. For yam, bargaining is essentially universal: regardless of where the transaction occurs, farmers almost always report bargaining with traders over the final price. As shown in Table I, the prevalence of bargaining is substantially lower for other crops in the study. Interestingly, in the data we see a fairly strong positive correlation between the prevalence of bargaining and the amount of price variation across farmers. Figure II presents data showing the degree of price dispersion in our sample by crop, using average annual prices received by farmers in the agricultural year prior to our intervention. The price variation across farmers is highest for yam (where bargaining is universal), and lowest for maize and processed forms of cassava such as gari (where bargaining is much less prevalent). This is consistent with what we heard from farmers about the existence of prevailing "market prices" for maize and gari that are taken as given and rarely negotiated.

[ INSERT FIGURE II HERE ]

One could speculate from these findings that lower price dispersion among sellers of maize and gari is indicative that these markets are already fairly well-integrated, leaving little room for an information intervention to improve outcomes for farmers. Although a full analysis of the spatial integration of different crops in Ghana is out of the scope of this paper, other papers have suggested that markets in Ghana are well-integrated for grains such as maize and rice, but not so for yam and cassava tubers (Cudjoe, Breisinger, and Diao, 2008). If farmers are already informed about local market prices, and local market prices co-move with urban prices, then our intervention would have led to little change in farmers' information set, and thus little change in outcomes.

One remaining puzzle is why, given the similarities between yam and raw cassava with respect to price dispersion and bargaining, we do not find treatment effects for the latter. We believe this can be explained by two key differences between the marketing of the two crops. First, farmers can choose to sell cassava in raw form, in processed forms, or both.[23] This option to convert raw tubers to processed foodstuffs changes the outside option available to farmers in their negotiations with their buyers. Second, in our study area, buyers of raw cassava are generally *not* traders; rather, they are local processors that buy tubers to make gari and dough.[24] The bargaining dynamics

[23]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.

[24]More than 90% of all sales recorded in our monthly surveys are to local processors, and less than 10% are to traders.

between farmers and local cassava processors are likely to be quite different from the bargaining dynamics between farmers and large-scale itinerant traders.

The broader takeaway from these results is that the impact of the intervention appears to be highly dependent on the marketing environment of each crop. The intervention had the biggest effect on the crop with the largest price dispersion and with the greatest prevalence of farmer-trader bargaining. 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 markets for other crops we 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.[25]

## 5 Explaining the treatment effect dynamics

The results presented in Section 4 pose an interesting puzzle, which we now attempt to better understand. Crucially, our interpretation of the estimated treatment effect hinges on whether or not SUTVA has been violated. If SUTVA holds, then our estimated treatment effects are unbiased and reflect the true effect of the MIS on farmers' prices. The main conclusion of our paper would be that price alerts initially had a positive impact on farmers, but that in the long run this impact disappeared. A possible explanation for this type of finding is a "fade out" story: over time, farmers stopped paying attention to the alerts, which caused their prices to revert to what they would have been absent the intervention. However, our data are inconsistent with this explanation; as we show in Section 5.2, at endline treatment farmers are more informed about urban market prices than control farmers, which would be unlikely to be the case if fade out had occurred.

Another possibility that could explain a true decline in the treatment effect is that traders stopped transacting—or threatened to stop transacting—with informed farmers, which in the long-run led to less favorable marketing conditions for the treatment group. However, in our annual

[25] 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 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.

surveys, treatment farmers did not report experiencing a reduction in volumes sold or in trading partners, which casts doubt on this hypothesis.

The explanation that is most consistent with our data is that the intervention indirectly benefited some control group farmers, which caused control group prices to converge upward to treatment group prices. This caused a violation of SUTVA, leading to a downward bias in the estimated treatment effects presented in Section 4.

### 5.1 Empirical evidence of indirect benefits for control group farmers

In this section, we present evidence that some control group farmers indirectly benefited from the intervention. Our approach is guided by the assumption that control group farmers with stronger network ties to treatment group farmers are more likely to experience indirect benefits. We use the term "network ties" to refer to the degree to which farmers are connected to one another: physical proximity as well as how much they communicate and otherwise interact with one another.[26]

For now, we remain agnostic about the mechanism that could be driving indirect benefits for control farmers. We are simply interested in seeing whether there is evidence of any violation of the SUTVA assumption. We construct our measure of network ties to the treatment group using the connectedness index, $c_{jk}$, that was created during our randomization procedure. Recall that $c_{jk}$ represents the degree of connectedness between farmers in villages $j$ and $k$. We use this index to create, for each village $j$, a measure of that village's connectedness to the treatment group, $\mathrm{C2T}_j$. The variable $\mathrm{C2T}_j$ is constructed as the simple average of the $c_{jk}$ scores for all villages $k \in T$ and $k \neq j$, rescaled to lie between zero and one. Villages with weaker connections to treatment villages have a C2T measure that is closer to zero, while villages with stronger network connections to treatment villages have a C2T measure that is closer to one.

We examine the relationship between C2T and prices to look for evidence of indirect benefits for the control group. After calculating the C2T measure for all villages (treatment and control),

[26] 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. ADD SOURCES.

we run the following regression on the monthly sales data:

$$p_{ijt} = \sum_{s=0}^{2} \{\delta_s Y_s + \alpha_s (T_j * Y_s) + \beta_s (C_j * \mathrm{C2T}_j * Y_s) + \gamma_s (T_j * \mathrm{C2T}_j * Y_s)\} + X'_{ij}\psi + \omega_k + \omega_t + e_{ijt} \quad (3)$$

The outcome of interest, $p_{ijt}$, is the price outcome of 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 ($T_j$ and $C_j$) 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 ($\omega_k$) and time period fixed effects ($\omega_t$), as well as other covariates ($X_{ij}$).

The main coefficients of interest are the $\beta_s$, which capture the impact of C2T for control group farmers at each time period. If control farmers are increasingly realizing positive spillovers associated with the intervention, then we should see the $\beta_s$ coefficients increasing over time. Admittedly, C2T is *not* an exogenous variable; we did not randomize levels of C2T in our study, so we cannot say with certainty that $\beta_s$ solely captures the impact of positive spillovers from the intervention. The main threat to our identification strategy is that C2T may be associated with village attributes that are unrelated to our intervention but that positively affect market prices (better access to markets and traders, or better access to information from non-Esoko sources). To better understand these concerns, we can look at the impact of C2T in the pre-treatment period (from the baseline data), in order to understand the relationship between C2T and prices prior to our intervention. We can also look at the evolution of $\gamma_s$ over time, which represents the impact of C2T for treatment group farmers. If we find that the impact of C2T is not significantly different from zero in the baseline data (for treatment and control farmers), and we fail to find an upward trajectory of C2T for treatment farmers, then we can be more confident that $\beta_s$ is truly capturing the impact of spillovers of the price alerts and not any other confounding factors.

Figure III presents the key results from this regression, using log prices. The top panel of the figure shows the estimated $\beta_s$ and $\gamma_s$ coefficients from the baseline (i.e. agricultural season prior to the intervention), Year 1, and Year 2, along with 95% confidence intervals. The bottom panel shows the difference ($\beta_s - \gamma_s$) for each time period. As predicted, there is a strong upward trend in the $\beta_s$ coefficients, and in Year 2 the coefficient on C2T for control farmers is significantly different from zero at the 1% level. The differential impact of C2T on control farmers relative to treatment farmers is increasing over time as well, although the difference is never significantly different from zero. The estimated coefficients on C2T in the baseline data are small and not significantly different from zero for either treatment or control farmers, although these coefficients are not very precisely estimated.

[ INSERT FIGURE III HERE ]

Given that we failed to find an effect of the price alerts on the prices received for non-yam crops, we can also look at the impact of C2T on prices for these other crops. If C2T is truly capturing spillovers of the intervention rather than other confounding factors, we should expect to see no impact of C2T on prices received for non-yam crops, for treatment or control farmers, across all periods of time observed in the data. In order to estimate this relationship, we add price data for non-yam crops into our analysis, along with crop-strata and crop-period fixed effects. Results of this regression are presented in Figure IV. 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 significantly different from zero in Year 2.

[ INSERT FIGURE IV HERE ]

Taken as a whole, these results suggest to us that farmers in control villages with stronger network ties to treatment villages realized benefits from these network ties, in the form of higher yam prices. We now focus on determining the mechanism driving the results.

## 5.2 Testing for information spillovers

Given the nature of our intervention, the most obvious mechanism for spillover effects would be information spillovers from the treatment group to the control group. In order to test for information spillovers, we look at the impact of C2T on farmers' level of information about market prices. If information spillovers are driving our results, then we should expect to see that, for control farmers, connectedness is associated with being more informed about market prices; i.e. the impact of C2T on informedness for the control group should be positive.[27]

Our annual surveys collected several different measures of farmers' knowledge of market prices.

[27] For the treatment group, it is not clear that C2T should have any impact on informedness, since these farmers are already directly receiving the information.

The measure we focus on here comes from the endline survey (results from other measures are consistent with what we present here, and can be found in Appendix B). In the endline, we asked yam farmers to give us their best estimate of current (i.e. at the time of the survey) prices for yam in Accra. We took these responses and calculated two measures of farmer estimation error: (1) absolute error, i.e. the absolute value of the difference between the true Accra price and the farmer's response; and (2) absolute percentage error, i.e. the absolute value of the difference as a percentage of the true Accra price. We then ran regressions of the natural logarithms of these errors on treatment status, C2T, and their interaction. As shown in Table V, treatment farmers were more accurate in their guesses about Accra prices (significant at the 10% level). Thus, it appears that treatment farmers were paying attention to the alerts, and the alerts had a measurable improvement in farmers' knowledge of urban market prices. Turning to information spillovers, the estimated coefficients on C2T for the control group are not significantly different from zero, a result that casts doubt on the notion that high degrees of information sharing occurred between treatment and control farmers. Connectedness seems to have no impact on informedness for treatment farmers, and the differential impact of informedness on control farmers relative to treatment farmers is also not significantly different from zero.

[ INSERT TABLE V HERE ]

The results presented here strongly suggest that, even at the end of the study, treatment farmers were more informed about market prices than control group farmers. The absence of a statistically significant relationship between C2T and levels of market information for the control group further suggests that information spillovers were relatively weak—if they were present at all—in our study.[28]

## 6 A model of bargaining spillovers

Having cast doubt on information spillovers being the driving force behind our results, we now present a model of farmer-trader bargaining under asymmetric information that illustrates an alternative type of network externality, which we call "bargaining spillovers," that we argue can better explain our findings.

[28] These results are broadly in line with the literature. Courtois and Subervie (2014) finds 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.

The key idea of the model is that the provision of information to treatment farmers has positive externalities on some control group farmers, even though they remain uninformed. The externality arises because traders, who buy crops from both informed and uninformed farmers, adjust their bargaining strategies in response to the new market conditions introduced by the intervention. To be precise, the intervention changes the information set of some farmers, and as a result, traders update their beliefs about farmers' information sets. In the model we show that if traders' belief updating is imperfect—i.e. traders are not able to perfectly distinguish between informed and uninformed farmers—then there is scope for uninformed farmers to benefit from the intervention despite remaining uninformed about market prices.[29]

In the remainder of this section we first present the model and its assumptions. Next we show how these predictions align with our main empirical results. Since the model's main theoretical predictions are also consistent with an information spillovers story, we close the section by presenting an additional theoretical implication of the model that is hard to reconcile with information spillovers alone. We test this prediction against the data in Section 6.4 to further substantiate that our model is consistent with the experimental results.

## 6.1 Basic Setup

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 a finite set of infinitely lived farmers $N = \{1, ..., n\}$ and a finite set of $n$ one-period lived traders. Each farmer has one unit of crop for sale and discounts the future by a factor $\beta$.[30] Within a period, all traders have the same resale value $v$, which is an *iid* draw from a uniform distribution with support $[v_L, v_H]$. The resale value $v$ represents the price that traders can receive for reselling the

[29]There are other possible reasons why general equilibrium effects may emerge and generate externalities on control farmers. For example, the fact that a group of better informed farmers can extract higher prices can increase the competitive pressure among traders when they bargain with uninformed farmers. Alternatively, if informed and uninformed farmers are selling in the same local markets, competition on the market place might induce a positive correlation between the price the two groups receive.

[30]The assumption that farmers are infinitely lived while traders live only for one period gives analytical tractability and, more importantly, 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.

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.[31]

In each period, each farmer that has not yet sold his one unit of crop is randomly matched to a trader. 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 to the farmer that the farmer can either accept or reject.[32] 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. There are two types of farmers: informed farmers (I) know the value $v$, while uninformed farmers (U) only know the distribution it is drawn from.

A crucial ingredient of our model is the assumption 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 of our environment since (i) most yam sales are made to traders with whom farmers have never transacted[33]; (ii) farmers' initial asking prices do not fully reveal their underlying information set[34]; and (iii) most farmers do not show the Esoko price alerts to traders during negotiations.[35]

In our empirical work, C2T represents the empirical counterpart to $d_i$, capturing a farmers' geographical and social positioning in the farmer network.[36] In the remainder of the section we indicate $d_i$ as $d$ to ease the notation. Having set up the environment, we now describe the optimal

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

[32] $w$ and $(1 - w)$ capture the bargaining power of farmers and traders respectively.

[33] Going back to Table I, note that around 55% of yams were sold to traders that farmers had never met before.

[34] This is illustrated in Figure B1: 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. Farmers' initial asking prices provide little informative value because they are typically set well above the prices that farmers expect to receive in the sale, as well as the prices they actually receive from traders. The figure is built using data from the midline survey, where we asked farmers to recall the details of an important sale made in the previous agricultural season, including information on initial asking prices and final (actual and expected) selling prices.

[35] 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. It is worth stressing that ex-post farmers will instead reveal their type both through the offers they make and their acceptance or rejection of the separating offer made by traders.

[36] We don't know enough about how traders form beliefs to test this assumption, but it seems logical that traders would believe that farmers living in villages that are geographically or socially connected to places where they have encountered informed farmers would be more likely to have price information. Additionally, given the fact that traders tend to follow particular trading routes, villages that are geographically close to treated villages are also more likely to sell to traders that have previously interacted with treated farmers.

offer and response strategies for traders and for informed and uninformed farmers.

## 6.2 Optimal strategies and equilibrium

Following nature's draw, each farmers and trader can play as a proposer or as a respondent. We therefore describe the optimal strategies as proposer and as respondent for traders, for informed farmers and for uninformed farmers. Proposition 1 characterizes the unique equilibrium.

The optimal strategies of the respondents are easily determined. A trader accepts a price offer $p$ if and only if it is lower or at most equal to the resale value $v$, that is if and only if $v - p \geq 0$. A farmer of type $i$ accepts an offer $p$ if and only if it is higher or equal to his discounted continuation value from rejecting, which we indicate as $R^i$, $i = \{U, I\}$. His optimal strategy therefore is to accept if and only if $p \geq R^i$. We now turn to the strategy of the proposers.

***Informed farmer's strategy*** When acting as the proposer, the informed farmer always extracts the full surplus by offering $p = v$, unless such value is lower than the continuation value $R^I$. Formally, his optimal strategy is to offer $p^I(v) = max(v, R^I)$. From the discussion above we know that this offer is accepted only if $v \geq R^I$.

***Uninformed farmer's strategy*** When acting as the proposer, the uninformed farmer maximizes

$$\max_{p} \quad \int_{p}^{\infty} pf\left(v\right)dv + R^U \int_{-\infty}^{p} f\left(v\right)dv$$

The corresponding first order condition is

$$\int_{p}^{\infty} f\left(v\right)dv - pf\left(p\right) + R^U f\left(p\right) = 0.$$

and the second order condition is $-2f\left(p\right) < 0$. Because $v \sim U[v_L, v_H]$, the interior solution $p^{int}$ is

$$v_H - p - p + R^U = 0 \quad \Rightarrow \quad p^{int} = \frac{v_H + R^U}{2}.$$

and the trader accepts it only if $v \geq p^{int}$.[37] The corresponding expected utility is

$$u^{int} = \left(1 - F(p^{int})\right)p^{int} + F\left(p^{int}\right)R^U$$

[37] Notice that an interior solution exists if $R^U > 2v_L - v_H$, a condition that will come handy for the proof of the main proposition.

The uninformed farmer can also implement a corner solution, offering $v_L$. Such offer is accepted by the trader only if $v \in [v_L, v_H]$ and in this case the farmer's utility is $v_L$. It follows that the optimal strategy of the uninformed farmer is

$$p^U = \begin{cases} p^{int} & \text{if} \quad u^{int} \geq v_L \\ v_L & \text{if} \quad u^{int} < v_L \end{cases}$$

***Trader's strategy*** The trader believes the farmer to be informed with probability $d$. Let $\bar{R} = max\{R^I, R^U\}$ and $\underline{R} = min\{R^I, R^U\}$. There are two possible optimal strategies: the pooling offer $\bar{R}$, which both types of farmer accept; and the separating offer $\underline{R}$, which only the uninformed farmer accepts. No other strategy is ever optimal: any offer $p \in (\underline{R}, \bar{R})$ is only accepted by uninformed farmers and delivers strictly smaller payoffs than $\underline{R}$. Any price $p > \bar{R}$ is accepted by all farmers and delivers strictly smaller payoffs than offering $\bar{R}$.

We can now state the main proposition, which is proved in Appendix D.1.

**Proposition 1.** *For each resale value $v$ there is a threshold probability $d(v)$ such that (i) for any $d \geq d(v)$ the trader's optimal offer is $R^I$ and the offer is accepted by both types of farmers; and (ii) for any $d_i < d(v)$ the trader's optimal offer is $R^U$ and the offer is only accepted by the uninformed farmer.*

Bargaining spillovers emerge when a trader meets a control group farmer with a high probability of being informed.[38] The trader's expected payoff from the pooling strategy is $v - R^I$, which is constant in $d$. The trader's expected payoff from the separating strategy is $(1 - d)(v - R^U)$ and is decreasing in the probability $d$ that the farmer is informed. Therefore when the probability $d_i$ that the farmer is informed is above a certain cut-off, the trader is better-off offering $R^I$ rather than trying to screen the farmer's type through the separating price offer. In particular, uninformed farmers with high $d_i$ may receive the same price offer as informed farmers, which can explain the

[38] Note that bargaining spillovers only emerge if farmers have a strictly positive continuation value, that is $R^i > 0, \forall i$. Otherwise, farmers would accept any price offer made by traders, who would therefore always extract all of the gains from trade when acting as proposers. In our setting, $R^i > 0$ is a plausible assumption as farmers who reject an offer usually have an option to sell later. This is because yams can be stored for months after harvest and because numerous traders visit the village over the season. Indeed, Table B4 in Appendix B shows that even before our intervention yam farmers were willing to walk away from a negotiation when the terms were not good enough for them. Technically, this table only tells us about farmers breaking off sales in favor of selling to another trader in the same month, but it is illustrative of the basic idea that farmers are not beholden to a single trader and are able to terminate negotiations to improve their trading outcomes.

indirect benefits we observed in the data.[39] To formally prove this result, let $R^U(d)$ and $R^I(d)$ be the continuation value functions of uninformed and informed farmers, where the posterior probability $d$ is the discounted continuation value of carrying the produce to the next period. Similarly let the price functions $p^U(d)$ and $p^I(d)$ be the expected price conditional on sales for uninformed and informed farmers respectively. Proposition 2 shows that an increase in $d$ has a positive effect on prices received by uninformed farmers:

**Proposition 2.** *The price conditional on sales is increasing in $d$ for uninformed farmers and decreasing in $d$ for informed farmers, that is the derivatives of the price functions satisfy $p^U_d > 0$ and $p^I_d < 0$ for all $d$.*

*Proof.* In Appendix $D$. □

In the next section we briefly discuss how our intervention may have influenced $d$ over time and then we discuss what this implies for the dynamics of the treatment effect, given the mechanism in our model.[40]

### 6.3 Predicted evolution of the treatment effect over time

We have already discussed how our empirical results change in the short and longer run. Here we resent the predictions of our model for (i) the Pre-MIS season, (ii) the immediate impact during the first months of the intervention, (iii) the longer run, starting in the second year.[41] The bargaining environment in each season is captured through specific assumptions on $d$.

(i) **Pre-MIS season** Before the intervention, all farmers have the same very low price information. In terms of the mode, all villages have the same small $\underline{d}$, which we assume $\underline{d} < \frac{R^I - R^U}{v - R^U}$

[39] At the same time, the offers received by a treatment farmer also depend on the strength of her links to the control villages. For informed farmers an increase in $d$ has a negative impact on prices. This channel and its implications for the timing of sales will be explored in more detail in section 6.4.

[40] Notice that bargaining spillovers occur when $w \in (0, 1)$, that is in situations where both farmers and traders have some bargaining power. If farmers had all the bargaining power, traders are never in the role of proposers. To the contrary, if traders had all the bargaining power, the continuation value of informed and uninformed farmers would be the same and there would not be any difference between pooling and separating equilibria.

[41] A more realistic assumption is that the updating of $d$ takes place after each bargaining round. Also it is more realistic to assume that farmers do not know $d$ but rather that $d$ is learnt through time by recalling the fraction of times that a pooling offer is made (as this is an increasing function of $d$). Solving the equilibrium with gradual two-sided updating becomes immediately more complicated and is beyond the scope of the paper. Our intuition is that none of the predictions of the model would be altered under this scenario.

for all $v$. This implies that the separating equilibrium is always implemented and all farmers are treated as if they were certainly uninformed.

(ii) **Immediate impact** During our intervention, only treatment farmers receive price information through Esoko and start changing their behavior. We assume that in the immediate run traders have not updated their beliefs about $d$ yet. Therefore, they still use the separating strategy. The positive treatment effect arises from informed farmers rejecting low offers as receivers and asking higher prices as proposers

(iii) **Longer run impact**. Because the farmers' behavior in the first months both as respondent and receiver reveals some information about their access to price information, eventually, traders update their beliefs based on that. We assume that updating is not perfect and we formalize this idea by setting the new $d$ assigned to each farmer is equal to the proportion of treatment farmers in her neighborhood. Formally, if $N_j$ is the total number of her neighbors and $T_j$ is the set of her neighbors who are in the treatment, then $d_j$ is updated to $d_j = \frac{|T_j|}{|N_j|}$.[42] As a result, towards the end of the study strong bargaining spillovers realize. In particular, given the result in Proposition 2, the model predicts that in this phase the index of connectedness $C2T$ would be positively correlated with the prices received by control farmers.

We now formally prove that the bargaining spillovers are stronger in the longer run. In order to do this, we aggregate over the distribution of $d$ and we assume (1) that the distribution of $d$ is homogeneous across informed and uninformed farmers; and (2) that after the introduction of the MIS the likelihood of being informed has weakly increased for all farmers. Under these two assumptions, Proposition 3 shows that the average price difference between treatment and control group is larger in the longer run, because of the bargaining spillovers:

**Proposition 3.** *If Condition 1 and 2 are verified, the difference between the average price received by informed and uninformed farmers, conditional on sale, is lower in Year 2 as compared to Year 1, that is* $\frac{1}{N/2}\sum_{j\in I} p^I(d_j) - \frac{1}{N/2}\sum_{j\in U} p^U(d_j) \leq p^I(\underline{d}) - p^U(\underline{d})$.

[42]Note that $d_j$ is common knowledge, which implies that it is known by the farmer and all traders the farmer is matched with in Year 2.

*Proof.* Under Condition 1 it is sufficient to prove that:

$$\frac{1}{N/2}\sum_{j\in I}\left(p^I(d_j)-p^U(d_j)\right)<p^I(\underline{d})-p^U(\underline{d}) \tag{4}$$

which is equivalent to show that:

$$\sum_{j\in I}\left(p^I(d_j)-p^I(\underline{d})\right)+\left(p^U(\underline{d})-p^U(d_j)\right)<0 \tag{5}$$

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

### 6.4 Further evidence in support of the bargaining model

So far, the predictions of our model cannot be distinguished from an information spillovers story. We close this section by turning to a third implication of our model that is *not* consistent with a story of pure information spillovers. When traders meet farmers with low values of $d$, they are more likely to implement a separating strategy and offer $R_U$, which will be accepted by uninformed farmers but rejected by informed farmers. When they meet farmers with higher values of $d$, they are more likely to offer the pooling strategy that is accepted by everyone. It follows that treatment farmers with lower values of $d$ should reject more offers than treatment farmers with higher values $d$ (because they are more likely to get the separating offer which is below their continuation value).[43] This relationship will not hold for control farmers.

Taking this prediction to the data, we can investigate whether there is any relationship between C2T and delays in sale for treatment farmers. To do this, we use the monthly sales data to compute, for each farmer $i$, the cumulative fraction $F_{ijt}$ of yam sold at each month $t$ in the agricultural year. 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. A farmer that has more delay in sales will have a lower cumulative fraction of yam sold earlier in the season. We use this variable

[43] This prediction contrasts sharply with what would be expected in a pure information spillovers story. In that story, traders make homogenous offers to all treatment farmers, and because all treatment farmers are endowed with the same information, they play homogeneous strategies. The prediction of delayed sales by treatment farmers with low C2T is specific of the bargaining spillover model and hence allows us to test our theory.

to estimate the following equation:

$$F_{ijt} = \alpha_m + \beta_1 T_j + \beta_2 C2T_j * C_j + \beta_3 C2T_j * T_j + \alpha_s + e_{ijt}, \tag{6}$$

where $\alpha_m$ are monthly fixed effects, and $\alpha_s$ are strata fixed-effects. The bargaining spillovers model predicts that $\beta_2$ is zero: C2T has no impact on timing of sales for control farmers. It also predicts that $\beta_3$ is positive: treatment farmers with higher C2T reject fewer offers than treatment farmers with lower C2T, so that at each point in time, they have a higher cumulative fraction of yam sold. The results of this regression are shown in Table VI. Consistent with our model of bargaining spillovers, in Year 2, $\beta_2$ is estimated to be zero and $\beta_3$ is estimated to be positive (significant at the 5% level).

[ INSERT TABLE VI HERE ]

Figure V plots the raw means and 90% confidence intervals for $F_{ijt}$ for farmers above and below the sample median of C2T, by month and treatment status, using the Year 2 data. 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, for control farmers, there is no consistent difference in the mean cumulative fraction sold between the two groups.

[ INSERT FIGURE V HERE ]

# 7 Estimating the de-biased treatment effect

Given the spillovers on control group prices, the SUTVA assumption is violated and the estimates of the treatment effect presented in Section 4 are biased. To see this, consider what is being estimated by equation (2), which is repeated here for convenience:

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

In this regression, $\lambda_s$ measures the average control group price in period $s$, and $\kappa_s$ measures the difference between average control group price and average treatment group price in period $s$ (controlling for $X_{ij}$). Without spillovers, $\lambda_s$ is an accurate measure of the counterfactual of interest:

what treatment group prices would have been absent the intervention. However, when spillovers affect control group outcomes, $\lambda_s$ no longer represents the counterfactual of interest. Instead, it represents the average control group price *inclusive* of spillovers. If spillovers cause increases in control group outcomes, then $\lambda_s$ is biased upward relative to the counterfactual of interest, and $\kappa_s$ is biased downward relative to the true treatment effect.

In order to de-bias $\kappa_s$, we need to find a more accurate measure of the counterfactual of interest. Ideally, we could use data for a set of "pure control" farmers that we know are completely unaffected by the intervention. However, in our case, we do not have data for a pure control group. Instead, we adapt the techniques developed by Baird, Bohren, McIntosh, and Özler (2014) to generate an estimate of what prices for this pure control group would be.

We make two assumptions to back out an estimate of prices for a hypothetical pure control group: (1) we assume a linear relationship between C2T and prices; and (2) we assume that villages where C2T is equal to zero are unaffected by spillovers. Given these assumptions, we can recover the de-biased treatment effect from our estimates of equation (2) and equation (3), the latter which is reproduced here for convenience:

$$p_{ijt} = \sum_{s=0}^{2} \{\delta_s Y_s + \alpha_s (T_j * Y_s) + \beta_s (C_j * C2T_j * Y_s) + \gamma_s (T_j * C2T_j * Y_s)\} + X'_{ij}\psi + \omega_k + \omega_t + e_{ijt}$$

Equation (3) is similar to (2), but it also includes $C2T_j$ interacted with treatment status. In this equation, $\delta_s$ is equivalent to $E[p_{ijt}|T_j = 0, C2T_j = 0]$ in period $s$. Thus, given our two assumptions, it is a measure of mean prices for a hypothetical pure control group unaffected by spillovers. The average spillover effect for the control group is equal to the difference between the observed average price in the control group ($\lambda_s$) in equation (2) and the estimated pure control average ($\delta_s$) in equation (3). To de-bias $\kappa_s$, we need to net out the average spillover effect on the control group. In other words, the unbiased treatment effect is equal to $\kappa_s + (\lambda_s - \delta_s)$, i.e. the biased treatment effect adjusted for the impact of spillovers on the control group.[44]

Table VII shows the estimates of (2) and (3) using the monthly sales data.[45] The first set of

[44] 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 less $\alpha_s$, 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).

[45] 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.

columns presents results using price levels, and the second set of columns presents results using price logs. Table VIII presents estimates of the de-biased treatment effect for each time period, as well as estimates of the average spillover effect on control group farmers. In terms of price levels, the de-biased treatment effect is estimated to be 16.16 GHS in Year 1, and 14.32 GHS in Year 2, both of which are significant at the 5% level.[46] 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 Section 4, 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.

[ INSERT TABLE VII HERE ]

[ INSERT TABLE VIII HERE ]

Compared to many other interventions in the agricultural marketing space, our estimated treatment effects—on the order of 8-9% increase in prices—are fairly substantial.[47] 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 revenue. 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. We did not collect information on farmers' costs, so we are unable to provide an exact figure for the impact in terms of profits. However, assuming a profit margin of 50% and no change in costs a 9% increase in prices would translate into a 18% increase in profits.[48] These figures don't take into account the spillover benefits for control group farmers, who also realize a 10% benefit in Year 2.

Another way to consider the magnitude of the intervention is to compare the cost of the service with the estimated benefits to farmers. Esoko recently started offering an annual subscription to farmers for 24 GHS (18 GHS in real August 2011 cedis), which is comparable to the per-farmer cost that we paid Esoko in our study. After accounting for the cost of the service and the cost associated with training farmers to understand the alerts, the direct ROI is over 200%.[49] The ROI is even higher if the indirect benefits on control farmers are also considered. These high ROI

[46] 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.

[47] See footnote 1.

[48] 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).

[49] Our costs of training were about US$37 per farmer for our study, or about GHS 60 in real August 2011 Cedis.

figures are driven by the fact that it costs very little to disseminate information using mobile phone technologies. In fact, the training component—which we view as being essential to wide scale take-up and usage of the service—is the major cost driver of the intervention. Given that farmers only need to be trained at the outset, the ROI of the intervention grows in later years of the service.

## 8 Conclusion

We implemented a randomized experiment that gave commodity price information to rural farmers via text messages on their mobile phones. We show that the alerts had a large and meaningful impact on yam prices for the treatment group, as well as large and meaningful benefits on prices received by certain control group farmers. The richness of our data, combined with a model of bargaining with asymmetric information, allows us to identify the main causal mechanism behind these spillover effects. The main mechanism at play does *not* seem to be information sharing between treatment and control farmers. Rather, spillover benefits are being driven by changes in traders' bargaining behaviors caused by the intervention ("bargaining spillovers").

Our identification of spillover benefits accruing to control group farmers is critical to a correct interpretation of our data. Had we ignored the potential for spillovers, we would have concluded that, although the price alerts were initially beneficial to treatment farmers, in the long run they had no impact on producer prices. This conclusion stands in stark contrast to what appears to have actually occurred: the intervention had large positive effects on prices received by treatment farmers and even many control farmers. More generally, these results illustrate that indirect (spillover) effects of interventions in agricultural markets can be substantial, can cause substantial bias in standard treatment effect estimates, and are therefore extremely important to take into consideration.

A second important finding is that the efficacy of price information depends upon the specific characteristics of the marketing environment. The price alerts had an impact on prices for yam, a crop characterized by high price variability, the absence of a reference "market price" and a high prevalence of bargaining. The effect was not present for other crops. This comparative static suggests that characteristics of the marketing environment directly impact the potential usefulness of price information services.

A final point worth discussing is the impact that the price alerts had on farmers' marketing behaviors. In Appendix C, we show that the alerts had little, if any, impact on where farmers sold their crops or the timing of their sales. While changes along these dimensions of marketing behavior were largely absent during the course of our study, we believe that, with even more time, farmers may actually start to change where and when they sell in response to market information. They may even change their production decisions. We leave the exploration of these subjects to future research.

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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% |
| **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% |
| **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 midline 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 midline 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.

Table II

Descriptive statistics and balance at baseline

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

Standard errors of the difference are clustered at the community cluster level.
"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

Impact of price alerts on yam prices, assuming no spillovers

| | Year 1 | | Year 2 | | Pooled | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| *Panel A: Price, level* | | | | | | |
| Treatment, Pre-T | | | | | -0.340 | 0.641 |
| | | | | | (6.903) | (6.942) |
| Treatment, Year 1 | 6.430* | 7.723** | | | 7.589* | 8.732** |
| | (3.735) | (3.465) | | | (3.825) | (3.687) |
| Treatment, Year 2 | | | 0.325 | 0.747 | -0.393 | -0.014 |
| | | | (4.499) | (4.516) | (4.498) | (4.483) |
| N | 1,522 | 1,522 | 2,660 | 2,660 | 5,032 | 5,032 |
| $R^2$ | 0.293 | 0.301 | 0.221 | 0.225 | 0.311 | 0.315 |
| *Panel B: Price, log* | | | | | | |
| Treatment, Pre-T | | | | | -0.028 | -0.022 |
| | | | | | (0.053) | (0.053) |
| Treatment, Year 1 | 0.042 | 0.050** | | | 0.043* | 0.050** |
| | (0.025) | (0.022) | | | (0.025) | (0.025) |
| Treatment, Year 2 | | | -0.014 | -0.012 | -0.010 | -0.008 |
| | | | (0.027) | (0.027) | (0.027) | (0.028) |
| N | 1,522 | 1,522 | 2,660 | 2,660 | 5,032 | 5,032 |
| $R^2$ | 0.326 | 0.337 | 0.259 | 0.263 | 0.339 | 0.342 |
| Control group mean price | 134.15 | 134.15 | 172.35 | 172.35 | 151.02 | 151.02 |
| Other covariates | | ✓ | | ✓ | | ✓ |

*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. Analysis relies on monthly data; results using annual data are comparable.
** Significant at 5% level. * Significant at 10% level.

Table IV

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* | | |
| Accra price | 0.337*** | 0.291** |
| | (0.113) | (0.128) |
| Accra price * Treatment | 0.175 | 0.172 |
| | (0.194) | (0.174) |
| Observations | 833 | 818 |
| $R^2$ | 0.096 | 0.166 |
| *Panel B: Deviation from predicted Accra price* | | |
| Accra price (deviation) | 0.203 | 0.102 |
| | (0.185) | (0.191) |
| Accra price (deviation) * Treatment | 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 midline and endline surveys, which asked farmers to recall details from an important transaction from the prior agricultural year. 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 V

Estimation errors of yam prices in Accra, endline survey

| | Log of absolute error | | Log of absolute % error | |
|---|---|---|---|---|
| | (1) | (2) | (1) | (2) |
| 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 VI

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

Table VII

Estimating the de-biased treatment effect on yam prices

| | Price, level | | Price, log | |
|---|---|---|---|---|
| | Equation (2) | Equation (3) | Equation (2) | Equation (3) |
| Treatment, pre-T | -0.340 | 6.339 | -0.028 | 0.102 |
| | (6.836) | (14.105) | (0.053) | (0.114) |
| Treatment, Year 1 | 7.589** | 4.554 | 0.043* | 0.036 |
| | (3.788) | (10.363) | (0.025) | (0.072) |
| Treatment, Year 2 | -0.393 | 14.846 | -0.010 | 0.065 |
| | (4.455) | (11.398) | (0.027) | (0.068) |
| C2T * Control, Pre-T | | -3.468 | | -0.032 |
| | | (16.648) | | (0.136) |
| C2T * Control, Year 1 | | 12.517 | | 0.048 |
| | | (8.447) | | (0.062) |
| C2T * Control, Year 2 | | 24.637** | | 0.180*** |
| | | (10.098) | | (0.065) |
| C2T * Treatment, Pre-T | | -15.268 | | -0.263* |
| | | (19.194) | | (0.141) |
| C2T * Treatment, Year 1 | | 18.966 | | 0.065 |
| | | (15.211) | | (0.099) |
| C2T * Treatment, Year 2 | | -2.955 | | 0.042 |
| | | (18.846) | | (0.117) |
| Pre-T | 98.550*** | 100.004*** | 4.601*** | 4.616*** |
| | (6.810) | (10.794) | (0.050) | (0.087) |
| Year 1 | 157.728*** | 149.155*** | 4.984*** | 4.950*** |
| | (8.191) | (9.800) | (0.052) | (0.064) |
| Year 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. Standard errors clustered at the community cluster level are shown in parentheses. The equations are estimating using two-step system GMM. *** Significant at 1% level ** Significant at 5% level * Significant at 10% level

Table VIII

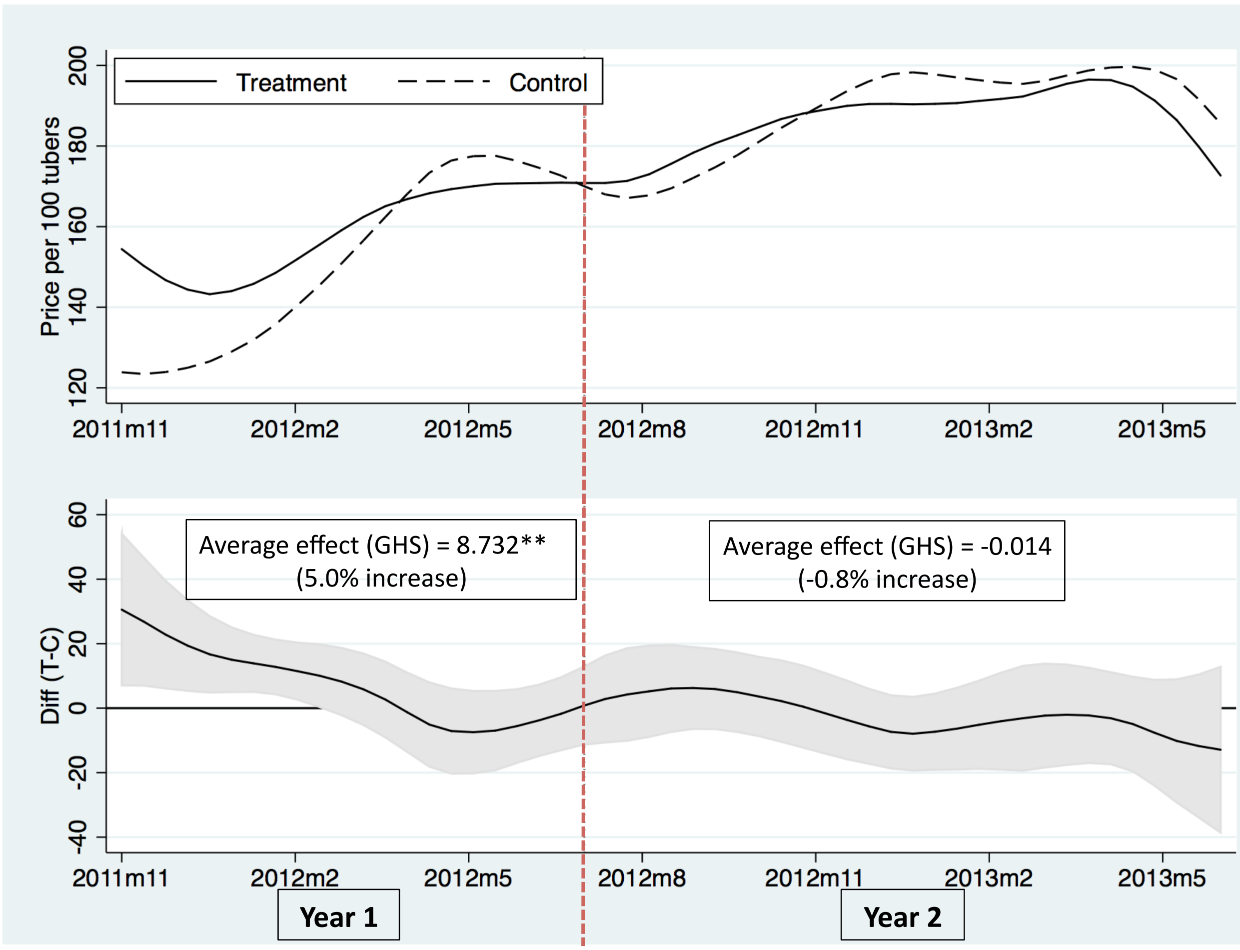
Estimate of spillovers and de-biased treatment effect

| | (1) Pre-T | (2) Year 1 | (3) Year 2 |
|---|---|---|---|
| *Panel A: Price, level* | | | |
| Biased treatment effect ($\kappa_s$) | -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 ($\kappa_s$) | -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

Figure I

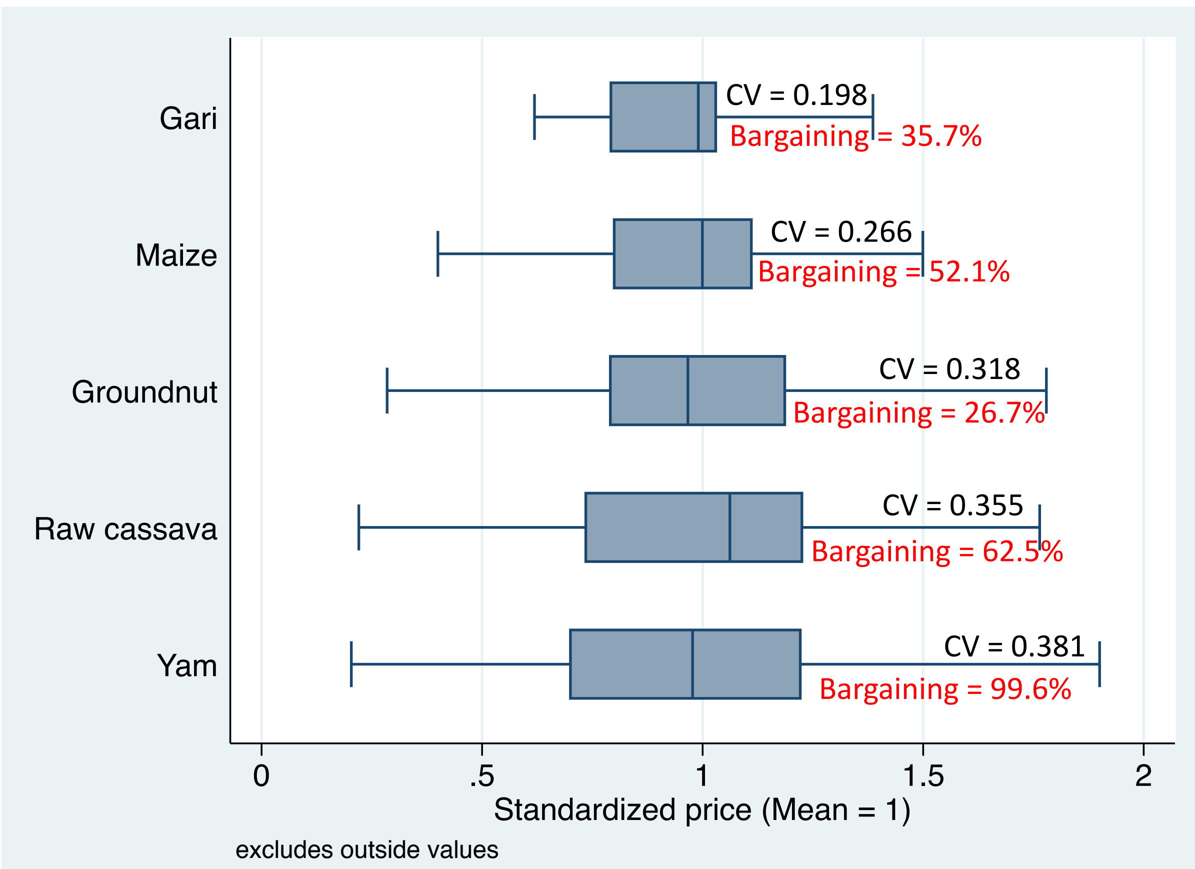
Impact of price alerts on yam prices, assuming no spillovers




*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 III). The dotted red line separates Year 1 results (November 2011-June 2012) from Year 2 results (July 2012-June 2013).

Figure II

Variation in prices for different crops, baseline survey

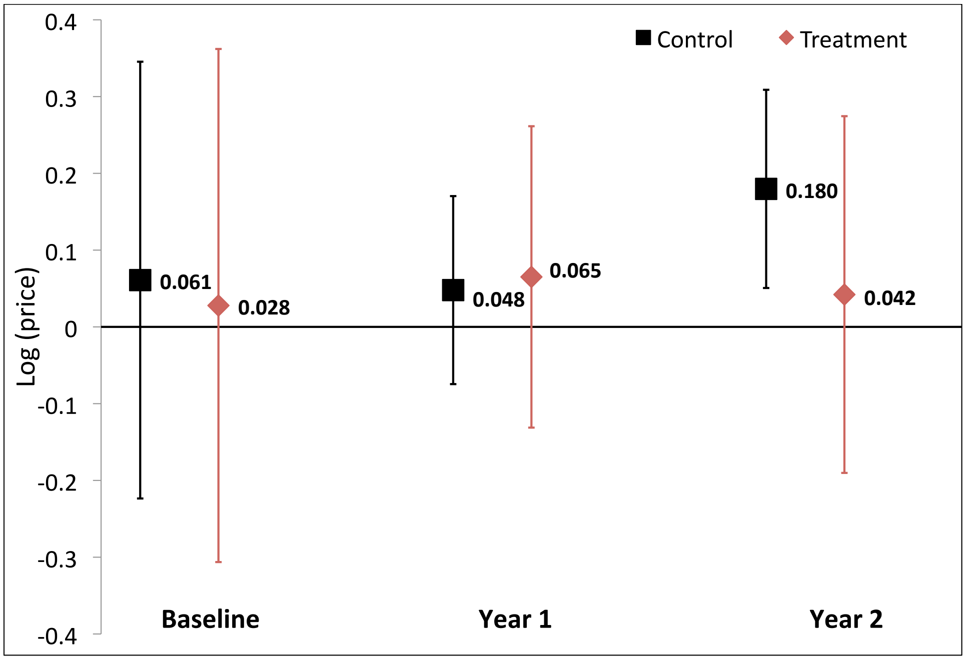



*Notes:* The figure presents box plots of average annual prices received by farmers in the study in the 2010-11 agricultural season (as recorded in the baseline survey). The figure also reports the mean within-district coefficient of variation (CV) for each crop, and the percent of farmers that report bargaining in their sales (from Table I). We ignore outliers, as well as districts with less than 4 farmers selling a particular crop.

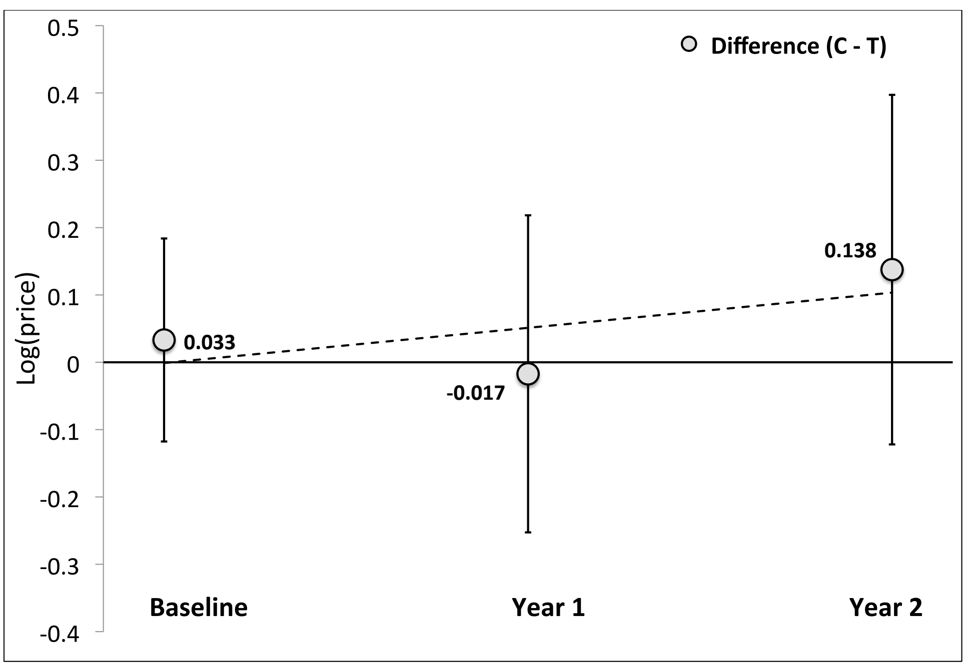
Figure III

Impact of C2T on yam prices over time

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



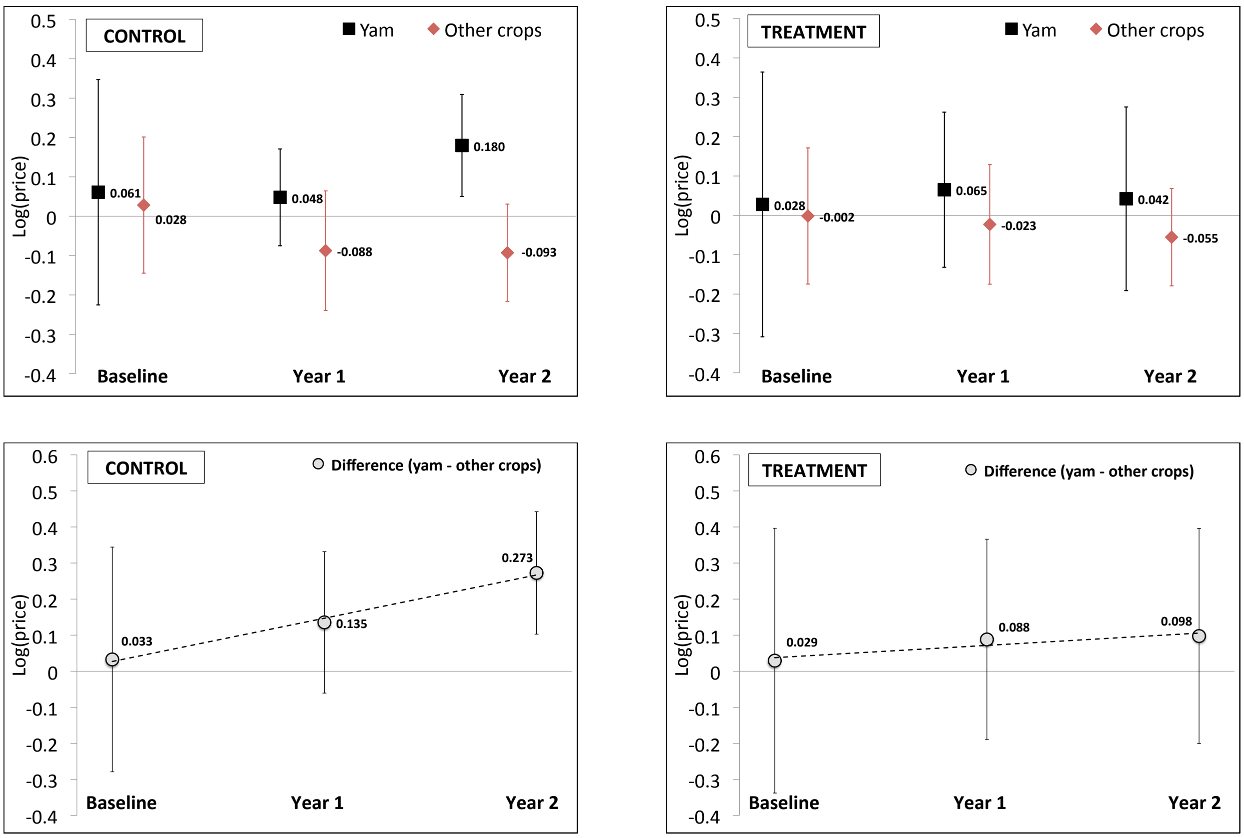

(b) **Difference (control - treatment)**




*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 IV

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

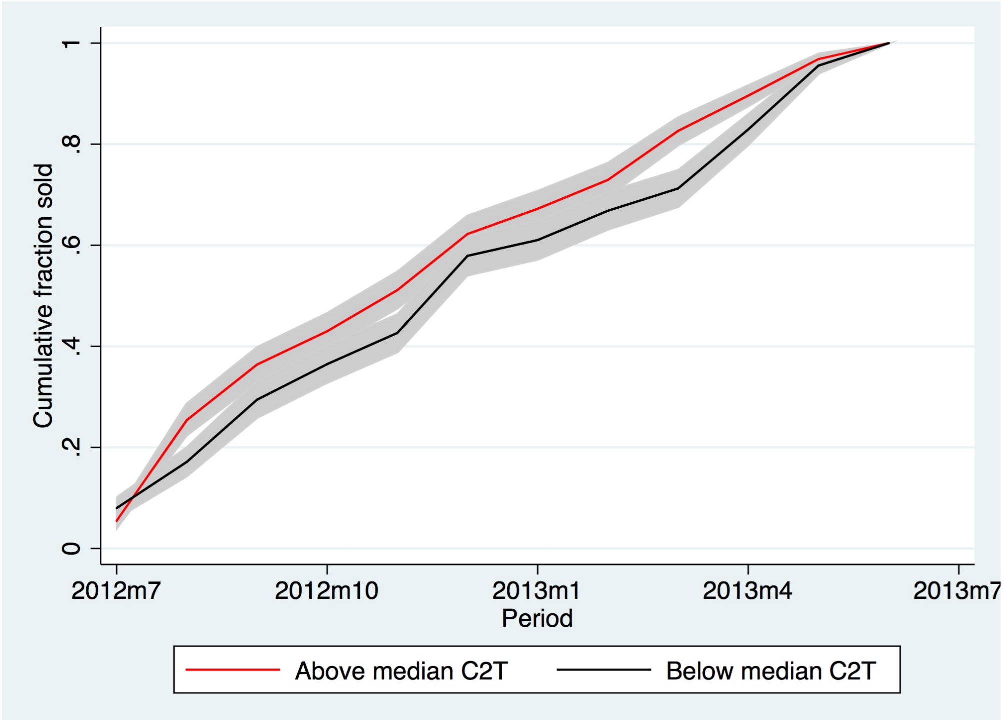



*Notes:* The top panels plot the impact of C2T on prices for yam and other crops, for control farmers (left-hand side) and treatment farmers (right-hand side). The bottom panels show the difference in the impact of C2T (yam prices vs. other crop prices). Error bars show 95% confidence intervals. Dashed 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.

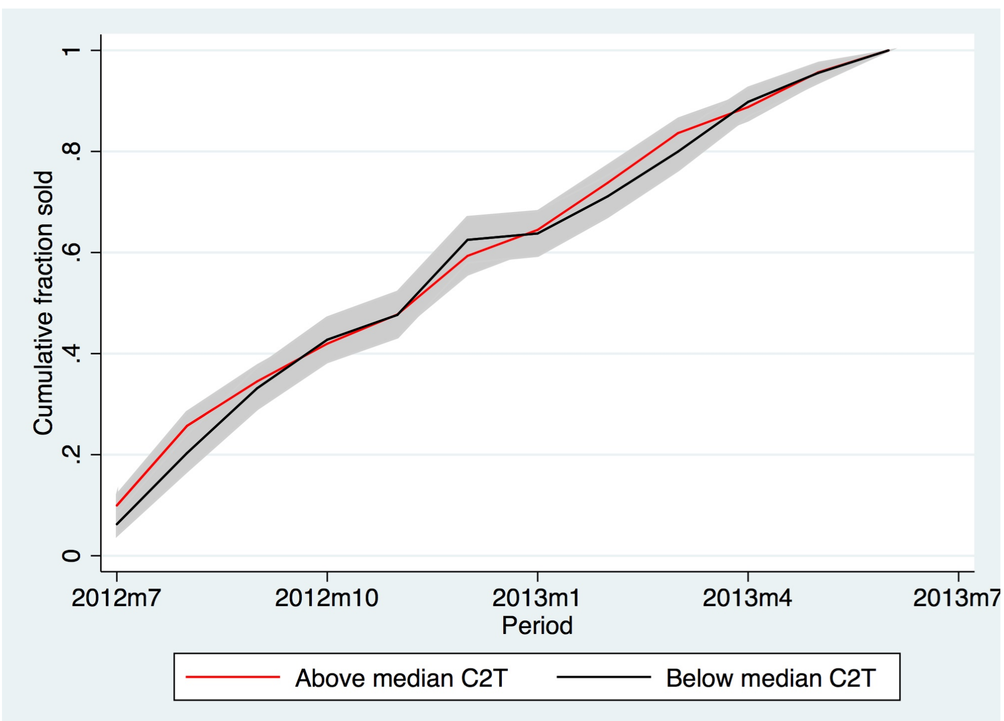
Figure V

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 Further detail on experimental design

## A.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 geographic proximity.

## A.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).

# B Additional results and robustness checks

Table B1

Impact of price alerts on yam quantities, assuming no spillovers

| | Year 1 | | Year 2 | | Pooled | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| *Panel A: Quantity, 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 |
| *Panel B: Quantity, 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 |
| Control group mean | 294.4 | 294.4 | 375.9 | 375.9 | 353.5 | 353.5 |
| 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 B2

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

| | All<br>(1) | Maize<br>(2) | Groundnut<br>(3) | Cassava: raw<br>(4) | Cassava: processed<br>(5) |
|---|---|---|---|---|---|
| *Panel A: Price, level* | | | | | |
| Treatment, Pre-T | -2.069 | -3.734 | -15.356 | 2.062 | -1.678 |
| | (2.518) | (5.249) | (29.588) | (2.253) | (2.542) |
| Treatment, Year 1 | 0.209 | -0.674 | -30.934 | 2.361 | 0.792 |
| | (2.088) | (1.731) | (26.145) | (2.313) | (1.738) |
| Treatment, Year 2 | -4.112 | 1.499 | -8.779 | 0.039 | -3.727 |
| | (2.775) | (1.773) | (15.702) | (3.559) | (2.791) |
| N | 7762 | 1568 | 569 | 1177 | 3940 |
| $R^2$ | 0.836 | 0.434 | 0.317 | 0.610 | 0.840 |
| *Panel A: Price, log* | | | | | |
| Treatment, Pre-T | -0.023 | -0.055 | -0.108 | 0.067 | -0.055 |
| | (0.035) | (0.069) | (0.149) | (0.056) | (0.047) |
| Treatment, Year 1 | 0.027 | -0.008 | -0.103 | 0.092** | 0.020 |
| | (0.026) | (0.018) | (0.100) | (0.045) | (0.030) |
| Treatment, Year 2 | -0.027 | 0.015 | -0.041 | -0.006 | -0.031 |
| | (0.023) | (0.020) | (0.066) | (0.059) | (0.028) |
| N | 7762 | 1568 | 569 | 1177 | 3940 |
| $R^2$ | 0.865 | 0.429 | 0.311 | 0.555 | 0.850 |

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. Prices in real, August 2011 Ghana Cedis (GHS). Prices are per bowl for all crops except raw cassava (per rope) and dough (per mini bag).

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

Table B3

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

| | All<br>(1) | Maize<br>(2) | Groundnut<br>(3) | Cassava: raw<br>(4) | Cassava: processed<br>(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.

Table B4

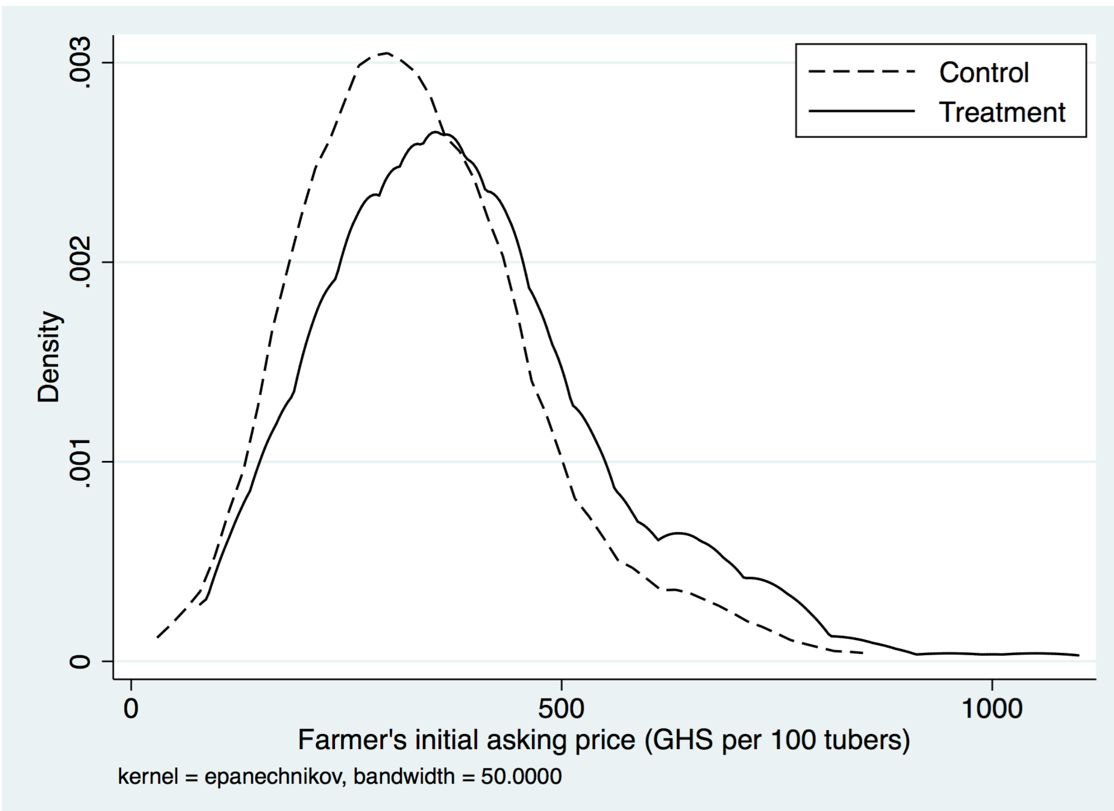
Unsuccessful negotiations between farmers and traders for yam

| | 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).

Figure B1

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 B5

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 B6

Number of markets for which farmers regularly know market prices

| | Baseline | | Midline | |
|---|---|---|---|---|
| | (1) | (2) | (1) | (2) |
| Treatment | -0.117 | 0.137 | 0.514*** | 0.730*** |
| | (0.120) | (0.398) | (0.101) | (0.272) |
| C2T * Control | | 0.654 | | 0.684 |
| | | (0.465) | | (0.432) |
| C2T * Treatment | | 0.217 | | 0.296 |
| | | (0.590) | | (0.305) |
| Difference | | 0.437 | | 0.388 |
| | | (0.603) | | (0.442) |
| Control group mean | 1.70 | 1.70 | 1.33 | 1.33 |
| Treatment group mean | 1.60 | 1.60 | 2.17 | 2.17 |
| N | 628 | 628 | 634 | 634 |

We asked farmers to list the number of markets (local and urban) for which they regularly knew market prices. We took these lists and generated counts of the number of markets listed by each farmer (ranging from 0 to a maximum of 4). We present results from a Poisson (count data) regression. 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 B7

Effect of alerts on change in land cultivated (acres)

| | Follow-up<br>(1) | Endline<br>(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.

## C Mechanisms: spatial and inter-temporal arbitrage

In this Appendix, we examine whether the intervention led farmers to make changes in whether they sold their yam, or the timing of their yam sales. We fail to find strong evidence of either of these mechanisms, leaving bargaining as the most likely channel through which the alerts led to improvements in farmers' prices.

### C.1 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 C1, we look at how the treatment affected farmers' decisions to sell at each of these locations, 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. 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.[50] 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 C1 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 C1 as supportive of that hypothesis.

[50] 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.

## C.2 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' 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 C1 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 C1

Impact of price alerts on place of sale, yam

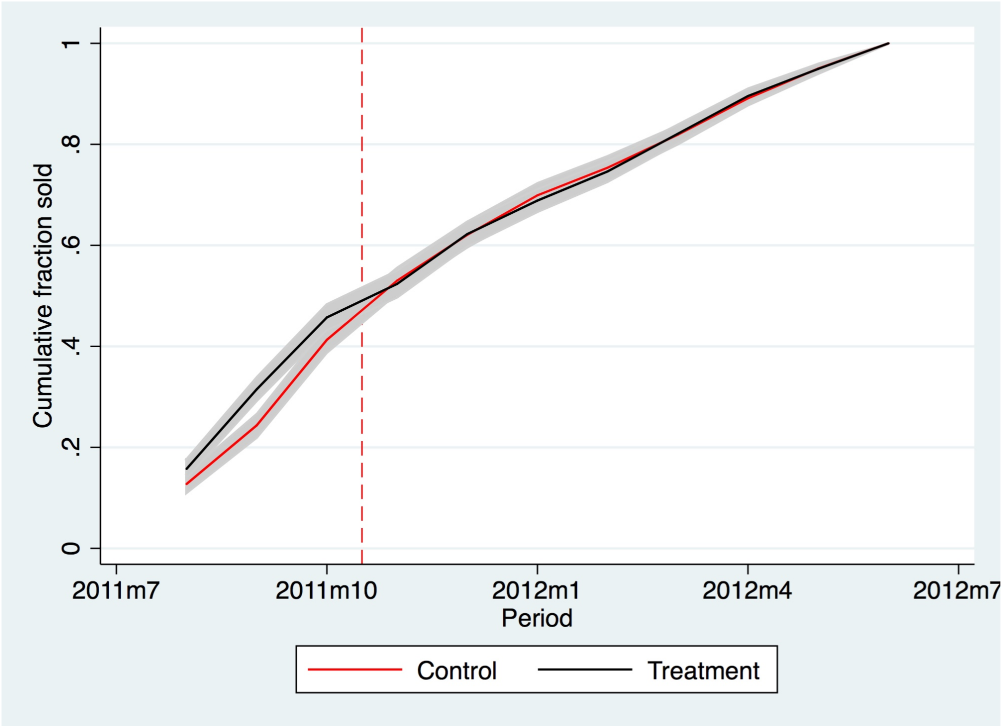
| | Sold any | | | Fraction sold | | |
|---|---|---|---|---|---|---|
| | Year 1 (1) | Year 2 (2) | Pooled (3) | Year 1 (4) | Year 2 (5) | Pooled (6) |
| *Panel A: Urban markets* | | | | | | |
| Treatment, Pre-T | | | -0.050 | | | -0.031 |
| | | | (0.047) | | | (0.039) |
| Treatment, Year 1 | 0.070 | | 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.

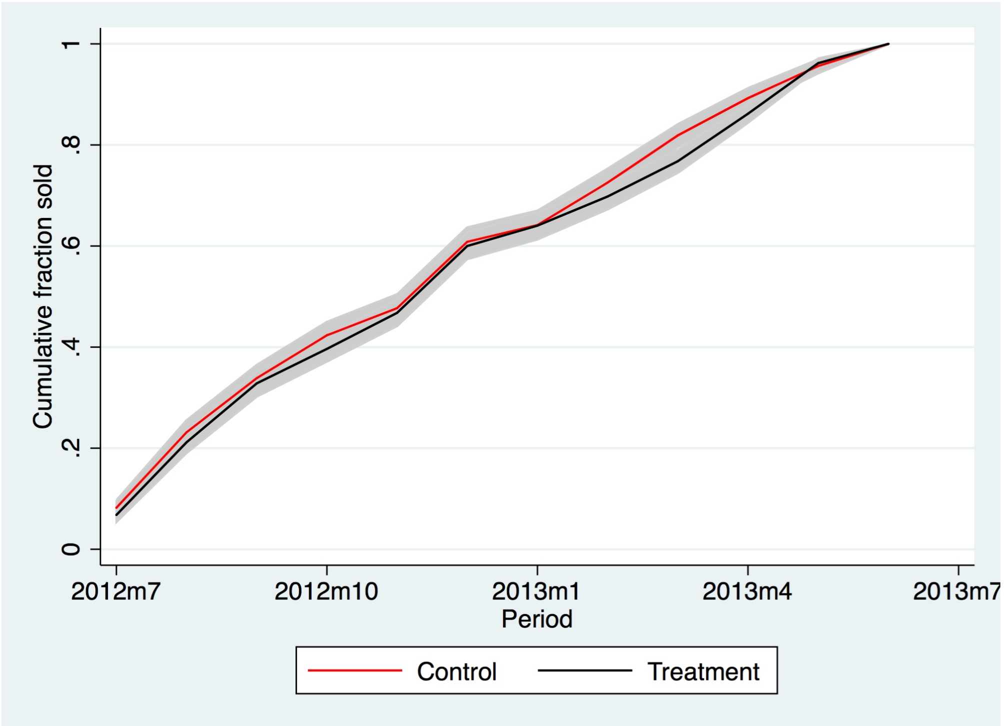
Figure C1

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.

# D Proofs

## D.1 Proof of Proposition 1 in bargaining model

The proof is divided into 4 lemmas.

**Lemma 1.** *The continuation value of the informed farmer is at least as high as the continuation value of the uninformed farmer, that is* $R^I \geq R^U$.

Lemma 1 is self-evident. As respondents, informed and uninformed farmers face the same price offer because the trader cannot distinguish between the two types. As proposer, the informed farmer can always mimic the strategy of the uninformed farmer and achieve payoffs that are at least as high.

**Lemma 2.** *The expected utility of being the proposer for the informed farmer is strictly higher than that of the uninformed farmer, that is* $O^I > O^U$.

We discuss two cases.

*Case 1:* The uninformed farmer's optimal strategy is the corner solution $p = v_L$.

In this case it is easily verified that

$O^U = v_L < (1 - F(R^I))\frac{v_H + max(R^I, v_L)}{2} + F(R^I)R^I = O^I$

*Case 2:* The uninformed farmer's optimal strategy is the interior solution $p = \frac{v_H + R^U}{2}$.

We can rewrite $O^U$ as a function of $R^U$:

$$O^U(R^U) = (1 - F(\frac{v_H + R^U}{2}))\frac{v_H + R^U}{2} + F(\frac{v_H + R^U}{2})R^U = \frac{v_H^2 + R^{U^2} + 2R^U v_H - 4R^U v_L}{4(v_H - v_L)}$$

Notice that, for all $R^U > 2v_L - v_H$, $O^U$ is strictly increasing in $R^U$[51]. It follows that:

$$\begin{aligned} O^I &= (1 - F(R^I))\frac{v_H + R^I}{2} + F(R^I)R^I \\ &> (1 - F(\frac{v_H + R^I}{2}))\frac{v_H + R^I}{2} + F(\frac{v_H + R^I}{2})R^I \\ &= O^U(R^I) \geq O^U(R^U) \end{aligned}$$

Where the first inequality follows from $\frac{v_H + R_I}{2} > R^I$ and the second inequality follows from Lemma 1 and the fact that $O^U(R^U)$ is strictly increasing in $R^U$.

**Lemma 3.** *The continuation value of the informed farmer is strictly higher than the continuation value of the uninformed farmer, that is $R^I > R^U$.*

Assume $R^I = R^U = \bar{R}$. Then the optimal strategy for the trader is offering a price equal to $\bar{R}$ which is accepted by both types of farmers.

It follows that we can write the continuation values as:

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

and if we subtract the two equations above from one another we get:

$$0 = \beta w(O^I - O^U) > 0$$

Where the inequality follows from Lemma 2, leading to a contradiction.

**Lemma 4.** *For each resell value $v$ there exists a probability $d(v) \in [0, 1]$ such that the optimal strategy of the trader is as follows:*

$$p^T = \begin{cases} R^U & \text{if} \quad d \leq d(v) \\ R^I & \text{if} \quad d > d(v) \end{cases}$$

From Lemma 3 we know $R^I > R^U$. As already shown in the trader's strategy section, the

[51] Remember that $R^U > 2v_L - v_H$ is a necessary condition for the existence of an interior solution.

trader chooses between two possible actions. The first is a pooling offer $R^I$ which is accepted by all farmers and gives expected payoffs equal to $v - R^I$. The second is a screening offer $R^U$ which is accepted only by the uninformed farmer and delivers expected payoff $(1-d)(v - R^U)$. The pooling offer dominates the screening offer if and only if:

$$
\begin{aligned}
(1-d)(v - R^U) &\leq v - R^I \\
\Rightarrow d &\geq \frac{R^I - R^U}{v - R^U}
\end{aligned}
$$

Hence the threshold strategy $d(v)$ defined in Lemma 4 is optimal with $d(v) = \frac{R^I - R^U}{v - R^U}$.

## D.2 Proof of Proposition 2 in bargaining model

*(a) Informed farmer's price function is decreasing in $d$.*

First notice that the continuation value for the informed farmer is not a function of $d$ as shown in the expression below.

$$\frac{R^I}{\beta} = w\frac{v_H - R^I}{2} + (1-w)R^I$$

The reason is that informed farmers receive the equivalent of their continuation value no matter whether traders are implementing a pooling or a separating equilibrium. The price offers of the informed farmer are always accepted, however the informed farmer only accept the pooling equilibrium offer. Define $\pi(d)$ as the probability that the trader makes a pooling offer. From Proposition 1 $\pi(d)$ is increasing in $d$. Define $\mu(d)$ as the probability that the trader is the proposer conditional on the agreement being reached in that period. We have $\mu(d) = \pi(d)(1-w)$ which is increasing in $d$ . The expected price conditional on sales can be expressed as:

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

which is a weighted average between the expected price proposed by farmers and the price proposed by traders conditional on the agreement being reached. Since $\frac{v_H - R^I}{2} > R^I$ the price function for the informed farmer is decreasing in $\mu(d)$ and therefore also in $d$.

*(b) Uninformed farmer's price function is increasing in $d$.*

**Lemma 5.** *The continuation value for the uninformed farmer is increasing in $d$.*

*Proof.* Consider any two sellers $s, h$ with $d_s < d_h$. We want to prove that $R^U(d_h) > R^U(d_s)$. The continuation value can be expressed as $\frac{1}{\beta}R^U = w\left(q\frac{v_H + R^U}{2} + (1-q)R^U\right) + (1-w)\left(\pi(d)R^I + (1-\pi(d))R^U\right)$. Where $q$ is the probability that the offer is accepted by the trader when the farmer proposes and $\pi$ is the probability of a pooling equilibrium when the trader proposes. Recall from Proposition 1 that $\pi$ is increasing in $d$. A raise of $d$ has the first order effect of increasing the probability of receiving a pooling offer and a second order effect of changing the offer asked by the farmer and the probability $q$ of it being accepted. We decompose the two effects by by studying the continuation value of an hybrid farmer that has the same probability of being informed as farmer $h$ but is forced to submit a price offer equal to the optimal offer of farmer $s$. Define as $R^{hyb}$ the continuation value of the hybrid farmer. We want to show that $R^U(d_s) < R^{hyb} \leq R^U(d_h)$. The second inequality is trivially satisfied for otherwise the strategy played by farmer $h$ would not be optimal. To prove the first inequality we subtract the continuation values from one another which leads to the following:

$$\frac{1}{\beta}R^U(d_h) - R^{hyb} = \frac{1}{\beta}\Delta_R = w(1-q)\Delta_R + (1-w)(R^I + R^L)(\pi(d_h) - \pi(d_s) - \pi_h\Delta_R$$

$$\Rightarrow \Delta_R = \frac{(1-w)(R^I + R^L)(\pi_h - \pi_s)}{1/\beta + \pi_h - w(1-q)} > 0$$

Where the last inequality follows from $\pi$ being increasing in $d$. Since $s$ and $h$ were chosen arbitrarily we conclude that the continuation value of the uninformed farmer is increasing in $\pi$ and therefore in $d$.

$\square$

The uninformed farmer always accepts the offer made by traders but there are cases where the offer that she makes is rejected by the trader (i.e. when the interior solution is optimal and $v < \frac{v_H + R^U(d)}{2}$). Define as $\nu(d)$ the probability that the farmer sells in period $t$ conditional on period $t$ being reached. We have $\nu(d) = (1-w) + w(1 - F(\frac{v_H + R^U(d)}{2}))$ which shows that $\nu(d)$ is decreasing in $d$ (farmers with higher $d$ ask a higher price which is rejected more often). The continuation value of the farmer can be now expressed as a function of $\nu$ 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.