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

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September 7, 2015

Abstract

We conducted a randomized field experiment to determine the impact of providing rural farmers with commodity price information 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. We discuss a novel mechanism of bargaining spillovers which can explain the rise of such positive externalities, even in the absence of information sharing between the treatment and the control groups. After accounting for spillovers, we estimate that the price alerts led to a 9% increase in prices for the treatment group and for some control farmers who, we argue, benefited from bargaining spillovers. Accounting for spillovers is crucial because otherwise the longer-run estimates would be biased and one could erroneously conclude that the intervention had no long-run benefit for farmers. The direct return on investment of the service exceeds 200%, a result that underscores the huge potential of ICT interventions in emerging markets.

JEL Codes: D82, O13, Q11, Q12, Q13.
Keywords: Price information, Agriculture, Bargaining, ICTs, Networks, Externalities.

*New York University, NYU Abu Dhabi and CTED (Center for Technology and Economic Development). We are extremely grateful to NYU-Abu Dhabi Institute, as well as anonymous donors, for generous financial funding, without which this project could not have taken place. We are also very grateful to Isaac Boateng and our team of field interviewers for logistical support. We thank Jenny Aker, Hunt Allcott, Marcel Fafchamps, Raquel Fernández, Nancy Qian, Debraj Ray as well as numerous seminar and conference participants for helpful comments and discussion. Corresponding Author: Nicole Hildebrandt, 19 West 4th Street, 6th Floor, New York, NY 10009; tel: 1-212-998-8900; fax: 1-212-995-4186; nth211@nyu.edu.

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. We partnered with the local agricultural information service provider Esoko. 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 are driven by improvements in farmers' bargaining outcomes with traders, rather than changes in other aspects of farmers' marketing behavior.[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. Randomized control trial (RCT) evaluations of MIS in Columbia (Camacho and Conover, 2011) and India (Fafchamps and Minten, 2012; Mitra et al., 2014) have failed to find measurable impacts on producer prices. In contrast, Svensson and Yanagizawa (2009) and Nakasone (2013) find that MIS in Uganda and Peru, respectively, increase producer prices by 13%-15%. Courtois and Subervie (2014) use propensity score matching methods to evaluate the impact of the same MIS we study here, and find impacts ranging from 7%-10%, albeit for a set of crops 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. 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. Standard estimates of the treatment effect, that ignore spillovers, would lead to the erroneous conclusion that the price alert service had no long-run benefit to farmers.

Third, our analysis of the spillover effects of the price alerts advances the field experimental literature looking at the indirect effects of interventions.[2] Particularly in the realm of agriculture, other studies have focused primarily on the possibility of information spillovers occurring within a village.[3] Our results indicate strong spillovers *across* villages, and do not offer strong evidence of information sharing between treatment and control farmers. We therefore believe that the externalities were mainly driven by another channel: traders' reaction to the intervention.

To formalize this intuition, we present a model of bargaining in the presence of asymmetric information. Based on our understanding of the study environment, the model assumes that traders cannot perfectly observe the treatment status of farmers, or whether they know urban market prices. This allows for "bargaining spillovers" to occur, as traders adjust their optimal bargaining strategy and offer higher prices to farmers they believe are likely to be informed, irrespective of their true informational status. We assume that the trader's belief about the probability that farmers in any given community are informed is increasing in the extent to which the village is connected (socially and geographically) to treated villages in the area. Given this assumption, the model predicts that control farmers with strong connections to the treatment group would benefit from the strongest positive externalities, which is what we observe in the data. The data also support an additional prediction of the model related to the timing of sales for treatment farmers. As we discuss in the paper, this final prediction would be very hard to reconcile with a scenario where spillovers are driven exclusively by control farmers getting urban price information from the treatment group.

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

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

In this sense, the additional prediction supports our hypothesis that traders' reactions to the intervention—and thus, bargaining spillovers—played an important role in our setting.

Our results illustrate that indirect impacts can be substantial, and if ignored can lead to serious misinterpretations of the effects of the programs being studied. Our results are also significant for policymakers and foundations engaged in the ongoing debate about the benefits of ICT-based interventions in the developing world. Compared to many other interventions in the agricultural marketing space, our estimated treatment effects—on the order of 8-9% increase in prices—are fairly substantial.[4] For the median yam farmer selling 1,200 tubers in a year, our estimated treatment effect translates into an additional 170 GHS (US$114) in annual 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.[5] 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 we paid for the service (i.e. 18 GHS) and the cost associated with training farmers to understand the alerts (i.e. about 60 GHS), the direct ROI is over 200%. The ROI is even higher if the indirect benefits on control farmers are also considered. These high ROI 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.[6]

The remainder of this paper is structured as follows. Section 2 provides an overview of agricul-

[4] See footnote 1.

[5] 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).

[6] See footnote 50 for details on the computation of the ROI.

tural 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 5.2 presents estimates of de-biased treatment effects. Section 7 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.[7] Transactions between farmers and traders usually take place at the farm gate, in the local community, or in the local market. They are conducted in an informal manner[8] 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 in a perfectly competitive manner (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

[7] 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.

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

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.[9] 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 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.[10] 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.[11] Within the study area, we sampled 100 com-

[9]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.

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

[11]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.

munities located within four contiguous districts.[12] 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.[13] 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;
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.[14]

[12] 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.

[13] 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 et al., 2007).

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

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: the 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 company called Esoko. The MIS provides weekly price alerts to subscribers via SMS (text message).[15] 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 markets in the country.[16] Enrolled farmers started receiving weekly price alerts in late October 2011.[17] 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.

[15]Esoko relies on a network of "market enumerators" to collect these market prices. Esoko trains enumerators to ensure that prices are collected in a consistent manner across markets, and holds twice-yearly refresher trainings to reinforce the enumeration methodology. In addition, the company quality reviews all prices before they are sent out and occasionally employs "mystery shoppers" to validate the information sent in by enumerators. Esoko operates its MIS in 10 countries across the African continent.

[16]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.

[17]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.

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).[18] 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.

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

[18] 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.

season ending in June 2011.[19] The main crops grown by farmers in the sample are yam, cassava, maize, and groundnut. Yam is by far the most commonly grown crop, with over 60% of farmers reporting it as one of their two main commercial crops. Farmers' knowledge of urban market prices is very 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 Randomized Control Trial (RCT) evaluations. This assumption says that "the potential outcomes for each person i are unrelated to the treatment status of other individuals" (Angrist et al., 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:

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

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, ω_k are randomization strata fixed effects (see Bruhn and McKenzie, 2009), ω_t are time period fixed effects, and X_{ij} is a set of additional covariates. The coefficient κ 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

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

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 λ_s measure the average control group price in period s, and the κ_s measure the average treatment effect in period s. As in (1), the κ_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.[20]

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 Online Appendix A). 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.[21] 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, in the first months after the introduction of the price alerts, there was a large difference between treatment and control group prices: over 20 GHS per 100 tubers, more than twice the estimated average treatment effect over the entire first year, shown 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.[22] 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.

[20] 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.

[21] 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).

[22] 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.

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, but our prior was 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 relative to the control group (see Online Appendix A 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, the farmer's initial price offer[23], 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

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

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.[24]

[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 Online Appendix B for these results). As described earlier, a key difference between yam marketing and marketing of other crops is the degree to which 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]

Lower price dispersion among sellers of maize and gari could be indicative that these markets

[24] 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.

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 et al., 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.[25] 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.[26] The bargaining dynamics 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.[27]

[25] 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.

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

[27] 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.

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 is violated, then the estimated treatment effects presented above are biased. If SUVTA holds, then our estimated treatment effects are unbiased and our conclusion 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 discuss in Section 5.1, 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 surveys, treatment farmers did not report experiencing a reduction in volumes sold or in trading partners, which casts doubts 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. Supporting evidence is presented in Section 5.1, where we also discuss why we believe that information sharing is not the main driving force of the indirect benefit. Our alternative explanation hinges on the bargaining between farmers and traders, but we postpone the discussion of this mechanism to Section 6. Because the existence of indirect benefits violates SUTVA, leading to a downward bias in the estimated treatment effects presented in Section 4, Section 5.2 presents a "de-biased" estimation.

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.[28]

We construct our measure of network ties to the treatment group using the connectedness index, c_{jk} discussed in Section 2.2. 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 over time to look for evidence of indirect benefits for the control group. After calculating the C2T measure for all villages (treatment and control), 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 for treatment and C_j for control) and a set of time indicators Y_s, $s \in \{0, 1, 2\}$ (pre-treatment, Year 1, and Year 2). As before, the regression includes strata fixed effects (ω_k) and time period fixed effects (ω_t), as well as other covariates (X_{ij}).

The main coefficients of interest are the β_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 β_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 β_s solely captures the impact of positive spillovers from the intervention.

[28] 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.

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 γ_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 β_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 β_s and γ_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 β_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 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. What mechanism could be driving these indirect benefits? Given the nature of our intervention, the most obvious mechanism would be the rise of information sharing between the treatment and control groups. Our surveys collect several different measures of farmers' knowledge of market prices, which can be used to indirectly test whether information sharing truly could be the mechanism driving the indirect benefits. In particular, in each yearly survey we ask farmers how well informed they feel they are about local and urban markets. Furthermore, at 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 used this data to measure farmers' estimation errors. Based on these measures, we observe that (i) even at the endline, when the treatment effect on prices is long gone and spillovers are strong, treatment farmers are significantly (at 10%) more informed than control farmers; (ii) while C2T has a positive impact on prices received by control farmers, it does not have any statistically significant effect on the quality of information they hold; (iii) the estimated impact of C2T on prices for control farmers is statistically the same whether we control for quality of information held or not.[29] These results cast some reasonable doubts on the notion that information sharing could be the main driving force of the indirect benefit collected by control farmers.[30] Section 6 presents a model of bargaining spillovers which can explain the spillovers patterns we observed in the data even in the absence of information sharing.

5.2 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

[29] All estimates shown in Online Appendix B. Note that our analysis does not rely on a causal interpretation of the impact of treatment status on price knowledge.

[30] 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.

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, λ_s measures the average control group price in period s, and κ_s measures the difference between average control group price and average treatment group price in period s (controlling for X_{ij}). Without spillovers, λ_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, λ_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 λ_s is biased upward relative to the counterfactual of interest, and κ_s is biased downward relative to the true treatment effect.

In order to de-bias κ_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 et al. (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, δ_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 (λ_s) in equation (2) and the estimated pure control average (δ_s) in equation (3). To de-bias κ_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.[31]

Table VI shows the estimates of (2) and (3) using the monthly sales data.[32] The first set of columns presents results using price levels, and the second set of columns presents results using price logs. Table VII 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.[33] 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 VI HERE]

[INSERT TABLE VII HERE]

6 A model of bargaining spillovers

We propose a model of farmer-trader bargaining under asymmetric information that illustrates an alternative mechanism—which we call "bargaining spillovers"—that can explain the externalities, even in the absence of information sharing. The key idea of the model is that the provision of information to treatment farmers has positive externalities on some control group farmers, even if 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' available information. In the

[31] 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 α_s, which is the treatment effect for a farmer with a C2T score of zero (which, by our assumption, implies that the farmer is completely unaffected by spillover effects associated with the intervention).

[32] 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.

[33] 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.

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 even in the absence of information spillovers.[34] We will first setup and solve the model and then discuss how its predictions map into our empirical findings, in particular with respect to the evolution of the treatment effect over time and to the role of the C2T index. Specifically, we shall show that traders will on average offer higher prices to those farmers whom they believe are more likely to be informed. The continuation values of the informed and uninformed farmers will play a key role in characterizing the equilibrium.

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 N infinitely lived farmers and N one-period lived traders.[35] Each farmer has one unit of crop for sale and discounts the future by a factor β.[36] Within a period, all traders have the same resale value v, which is an *iid* draw from a uniform distribution with support $[v_L, v_H]$, and with $f(v)$ and $F(v)$ denoting the density function and cumulative distribution function, respectively. The resale value v represents the price that traders can receive for reselling the crop in the urban market, net of transport costs, if they are successful in purchasing the crop from a farmer. All agents are risk neutral.[37]

We will think of a period as representing a week within one season. Each period, the urban market price v is an i.i.d. draw from the distribution mentioned above. In each period, every farmer that has not yet sold his one unit of crop is randomly matched to a trader. With probability

[34] Our model offers one possible explanation for what we observe in the data and delivers one additional testable prediction. There may be other possible mechanisms that can generate externalities on some control farmers, but we cannot explicitly test them. For example, the fact that a group of better informed farmers can extract higher prices may increase the competitive pressure among traders when they bargain with uninformed farmers. Similarly, 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.

[35] For the model as currently stated we just need that in each period each farmer gets visited by one trader.

[36] 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.

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

$w \in (0, 1)$ the farmer makes a take-it-or-leave-it offer p that the trader can either accept or reject. With probability $(1-w)$ the trader makes a take-it-or-leave-it offer that the farmer can either accept or reject. Here, w and $(1 - w)$ capture the bargaining power of farmers and traders, respectively. If the offer is accepted by the respondent, the trader's utility is $(v - p)$, while the farmer's utility is p. If the offer is rejected, the trader receives utility 0 and the farmer keeps the crop, moves to the next period, and is matched with a new trader.[38] There are two types of farmers: informed farmers (I) know the value v, while uninformed farmers (U) only know the distribution from which it is drawn. Since farmers are infinitely lived, their reservation value for selling in the current period is equal to their discounted continuation value of waiting to sell sometime in the future. We define R^I to be the discounted continuation value of the informed farmer, and R^U to be the discounted continuation value for the uninformed farmer.

A crucial 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; (ii) farmers' initial asking prices do not fully reveal their underlying information set; and (iii) most farmers do not show the Esoko price alerts to traders during negotiations.[39] For tractability, we assume that d_i attached to a given farmer i is common knowledge to all farmers and traders in the game. In the remainder of the section we write d_i as d to ease the notation.

[38] In our setting, it is reasonable to assume that farmer's can wait to sell crops in the future. Yams can be stored for months after harvest and numerous traders visit the village over the course of the season. In Table B8 of Online Appendix B, we show that even before our intervention yam farmers would sometimes walk away from a negotiation when the terms were not good enough for them.

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

6.2 Optimal strategies

Trader's strategy When acting as the responder, the trader accepts a price offer p if and only if $v - p \geq 0$.[40] When acting as the proposer, recall that we assume that the trader does not know whether she is facing an informed farmer or an uninformed farmer. Instead, she only knows the probability d that the farmer is informed.

Since an informed farmer can mimic any strategy of the uninformed farmer when acting as proposer, it must be the case that, in any equilibrium of our model, the discounted continuation value of the informed farmer is strictly higher than the discounted continuation value of the uninformed farmer (see Appendix B.2 for more detail). Given this result, when the trader is the proposer, she chooses to offer either: (1) $p = R^I$, which all farmers accept, resulting in a pooling equilibrium; (2) $p = R^U$, which only uninformed farmers accept, resulting in a separating equilibrium; or (3) in the case when $v < R^U$, no farmer will choose to trade, so the trader can offer any $p \in [0, v]$ which is rejected by all farmers.[41] If $v \geq R^U$, the trader will choose the pooling equilibrium whenever:

$$(1-d)(v - R^U) \leq v - R^I. \tag{4}$$

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

We now define:

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

in which case (4) is equivalent to:

$$v \geq M\left(R^U, d, R^I\right). \tag{5}$$

The trader implements a pooling strategy whenever (5) is met. Hence we can define:

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

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

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

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

Pooling and separating equilibria occur in the eponymous sets, respectively.[42] Thus, ex-ante, the probability that a trader implements the pooling strategy, $Pr(V^{Pooling})$, is $1 - F(M)$, where F represents the cumulative distribution function of v.

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

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

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

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

$$R^I = \beta\left[wE_v[max\{v, R^I\}] + (1 - w)R^I\right]. \tag{6}$$

In Appendix B.1, we show that there is always a unique value of R^I which satisfies (6). Moreover, this value is not a function of d, as (6) is independent of d. Depending on the primitives of the model, the equilibrium value R^{I*} can be greater than or less than (or equal to) v_L. In the former case, there is a positive probability that the trader rejects the informed farmer's offer, whereas in the latter case the informed farmer makes offers which are always accepted by the trader. Appendix B.1 derives the condition for each type of equilibrium to occur. For the remainder of the paper, we focus on the case where $R^{I*} > v_L$, which occurs when $\Phi E_v[v] > v_L$, where $\Phi \equiv \frac{\beta w}{\beta w + (1-\beta)}$.[43]

Uninformed farmer's strategy The uninformed farmer does not know v, so he cannot extract the full surplus. Instead he chooses p to maximize the following equation, conditional on the value

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

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

of R^U:

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

The first order condition at an interior solution is

$$\int_p^{\infty} f(v)\,dv - pf(p) + R^U f(p) = 0$$

and the second order condition is $-2f(p) < 0$. Because v is uniformly distributed in the interval $[v_L, v_H]$, the interior solution p^{int} is

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

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

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

When acting as the responder, the uninformed farmer accepts any offer $p \geq R^U$. Thus, his payoff is R^I when the trader implements a pooling strategy, and R^U otherwise.[44]

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

$$R^U = \beta \left[wO^U + (1 - w) \left\{ R^U F(M) + R^I [1 - F(M)] \right\} \right] \tag{7}$$

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

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

6.3 Characterization of the equilibrium

An equilibrium for our model is characterized by a set of continuation values, R^I and R^U, which obey the two Bellman equations in (6) and (7). As mentioned previously, in Appendix B.1, we prove the existence of a unique fixed point R^I of (6). We now impose the condition that $2v_L - v_H < 0$ to avoid looking at the corner solution in the uninformed farmer's problem.[45]

Proposition 1. *Assume* $\Phi E_v[v] > v_L$ *and* $2v_L - v_H < 0$*. Then there exists equilibrium values* R^{U*} *and* R^{I*} *satisfying* (6) *and* (7) *with* $R^{U*} < R^{I*}$.

Proof: *See Appendix B.3.*

The discounted continuation value for the informed, R^{I*}, defined in (6), is independent of the density d; however, as the proposition below shows, the discounted continuation value of the uninformed farmer is increasing in the density d (up to an issue of equilibrium selection).

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

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

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

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

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

Proof: *See Appendix B.3.*

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

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

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

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

Proof: *See Appendix B.4.*

Corollary 1 has empirically testable implications for observed prices for treatment and control farmers as shown in Proposition 2. Let the price functions $P^I(d)$ and $P^U(d)$ be the equilibrium expected price, conditional on sale, for informed and uninformed farmers respectively.[46] Define $\mu^I(d)$ and $\mu^U(d)$ as the probability that the trader is the proposer conditional on the agreement being reached in that period, for uninformed and informed farmers respectively.[47] The expected price conditional on sales for the the two farmers can be expressed as:

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

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

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

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

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

Proof: *See Appendix B.5.*

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

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

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

6.4 Empirical predictions of bargaining spillovers

We now describe how our empirical findings can be explained through the lens of our model of bargaining spillovers. Recall from Section 4 that we find a large, positive difference between average treatment and control group prices in the early months of the intervention, which fades over time so that, by Year 2, the difference is no longer significant. In Section 5, we show that this reduction in the difference between average treatment and control prices occurs because control farmers with stronger ties to the treatment group (as measured by $C2T$) start to to benefit from positive spillovers. Given the limited evidence of information spillovers, we set out to provide a model that could show, even in the absence of information spillovers, how these indirect benefits could occur.

Our model of bargaining spillovers suggests that these indirect benefits arise once traders appropriately update their beliefs about farmers' information sets. Although we lack data on traders' beliefs, we view our $C2T$ variable as a good proxy for d in our model. When traders form beliefs about farmers, they are likely to rely on information about a farmer's social and geographical relationship with other farmers in the area. In the context of our study, we find it likely that traders would believe that farmers living in villages that are more "connected" to places where they have previously encountered informed farmers are more likely to be informed themselves. Additionally, since traders tend to follow particular trading routes, villages that are geographically close to treated villages are also more likely to interact with traders that have previously encountered treated farmers.

Our empirical findings suggest, however, that this belief updating does not happen instantaneously, instead taking several months to occur.[48] Thus, we can contrast what happens in early months of the intervention, prior to traders' belief updating, to what happens in the longer run when belief updating has taken place.

Short-run impact: initial months post-intervention. The data suggest that it takes some time for traders to update their beliefs about farmers' informedness, so that in the initial months post-intervention, no belief updating occurs. Traders continue to believe that farmers have low (pre-intervention) levels of price information, e.g. $d = d^0 < d_{LL}$ for all farmers, and thus always make low (separating) offers. However, these beliefs are incorrect because, in fact, informed farmers now

[48]This is supported by the finding from Figure I that spillovers do not start to occur until about 3-4 months after the introduction of the intervention.

have access to information on urban market prices. Define Δ^{SR} to be the average difference between informed and uninformed farmer prices in the short run; i.e. $\Delta^{SR} = P^I(d^0) - P^U(d^0)$. $\Delta^{SR} > 0$ because informed farmers ask for higher prices when urban market prices are high (evidence for which we presented in Table IV), and reject low (separating) offers made by traders when acting as respondents. Note that this is an out-of-equilibrium result; traders haven't appropriately updated their beliefs.

Longer-run impact: Year 2 results. Eventually traders update their beliefs about d. The updating is not perfect: traders never know with certainty the type of the farmer to whom they are matched, but instead make an assessment based on the farmer's placement in the larger farmer network, similar to the manner in which we construct our $C2T$ measure. Once traders update their beliefs about d, bargaining spillovers emerge. In particular, control group prices are (weakly) increasing with d—and thus its empirical counterpart $C2T$. Treatment group prices, meanwhile, are (weakly) decreasing with d (something we admittedly don't find much evidence for in our empirical analysis). Define Δ^{LR} to be the average expected price difference received by informed and uninformed farmers in the long run, i.e. $\Delta^{LR} = \frac{1}{N/2}\sum_{j\in I} P^I(d_j) - \frac{1}{N/2}\sum_{j\in U} P^U(d_j)$. The overall effect is that in the longer run, when traders update their beliefs and bargaining spillovers emerge, the observed price difference between informed and uninformed farmers falls relative to the observed price difference prior to this belief updating, i.e. $\Delta^{LR} < \Delta^{SR}$.

Proposition 4 formally proves this result. In order to do this, we aggregate over the entire distribution of d and we assume two conditions: (i) *Condition 1*: The distribution of d is identical across informed and uninformed farmers—a realistic assumption given our randomization; and (ii) *Condition 2*: after the introduction of the MIS, the likelihood of being informed has weakly increased for all farmers, and $d > d^{LL}$ for some farmers.

Proposition 4. *If Condition 1 and 2 hold, the difference between the average price received by informed and uninformed farmers, conditional on sale, is lower in the longer run as compared to the short run, that is* $\Delta^{LR} < \Delta^{SR}$.

Proof. Under Condition 1 Δ^{LR} can be rewritten as $\Delta^{LR} = \frac{1}{N/2}\sum_{j\in I}\left(P^I(d_j) - P^U(d_j)\right)$. It is sufficient to prove that:

$$\frac{1}{N/2}\sum_{j\in I}\left(P^I(d_j)-P^U(d_j)\right)<P^I(d^0)-P^U(d^0) \tag{8}$$

which is equivalent to show that:

$$\sum_{j\in I}\left\{\left(P^I(d_j)-P^I(d^0)\right)+\left(P^U(d^0)-P^U(d_j)\right)\right\}<0 \tag{9}$$

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

6.5 Further evidence in support of the bargaining model

So far, our empirical findings could also be explained by information sharing between treatment and control farmers. But our model gives us one additional testable prediction which would be difficult to reconcile with a scenario where spillovers are driven exclusively by control farmers getting urban market price information from treatment farmers. In our model, 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). 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.[49] 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 to estimate the following equation:

$$F_{ijt}=\alpha_m+\beta_1 T_j+\beta_2 C2T_j * C_j+\beta_3 C2T_j * T_j+\alpha_s+e_{ijt}, \tag{10}$$

[49] 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.

where α_m are monthly fixed effects, and α_s are strata fixed-effects. The bargaining spillovers model predicts that β_2 is zero: C2T has no impact on timing of sales for control farmers. It also predicts that β_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 V. Consistent with our model of bargaining spillovers, in Year 2, β_2 is estimated to be zero and β_3 is estimated to be positive (significant at the 5% level).

[INSERT TABLE V 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, there is no significant difference for control farmers.

[INSERT FIGURE V HERE]

7 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 investigate the causal mechanisms behind the spillover effects. Our analysis suggests that the spillover benefits are substantially driven by changes in traders' bargaining behaviors caused by the intervention ("bargaining spillovers"), a mechanism which so far has not received much attention in the literature. We find no strong evidence of the usual suspect, that is information sharing between treatment and control farmers.

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, illustrating that indirect (spillover) effects of interventions in agricultural markets can be substantial, can cause bias in standard treatment effect estimates, and are therefore extremely important to take into consideration. After correcting for spillover effects, we find that our intervention increased farmer's prices by 8-9%, generating an increase in annual household income on the order of 170 GHS (US$117) for a typical yam farmer. Given the low cost of information delivery via mobile phone, the intervention delivered these benefits in a highly cost-effective manner, with a direct ROI in excess of 200%[50].

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 Online Appendix A, we show that the alerts had little, if any, impact on where farmers sold their crops. While changes along other 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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[50]The ROI figure is computed by comparing the direct impact on the annual revenues of treated farmers, about 172 GHS, to the total cost of the service in our study, about 74 GHS including training

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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

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 VI

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 VII

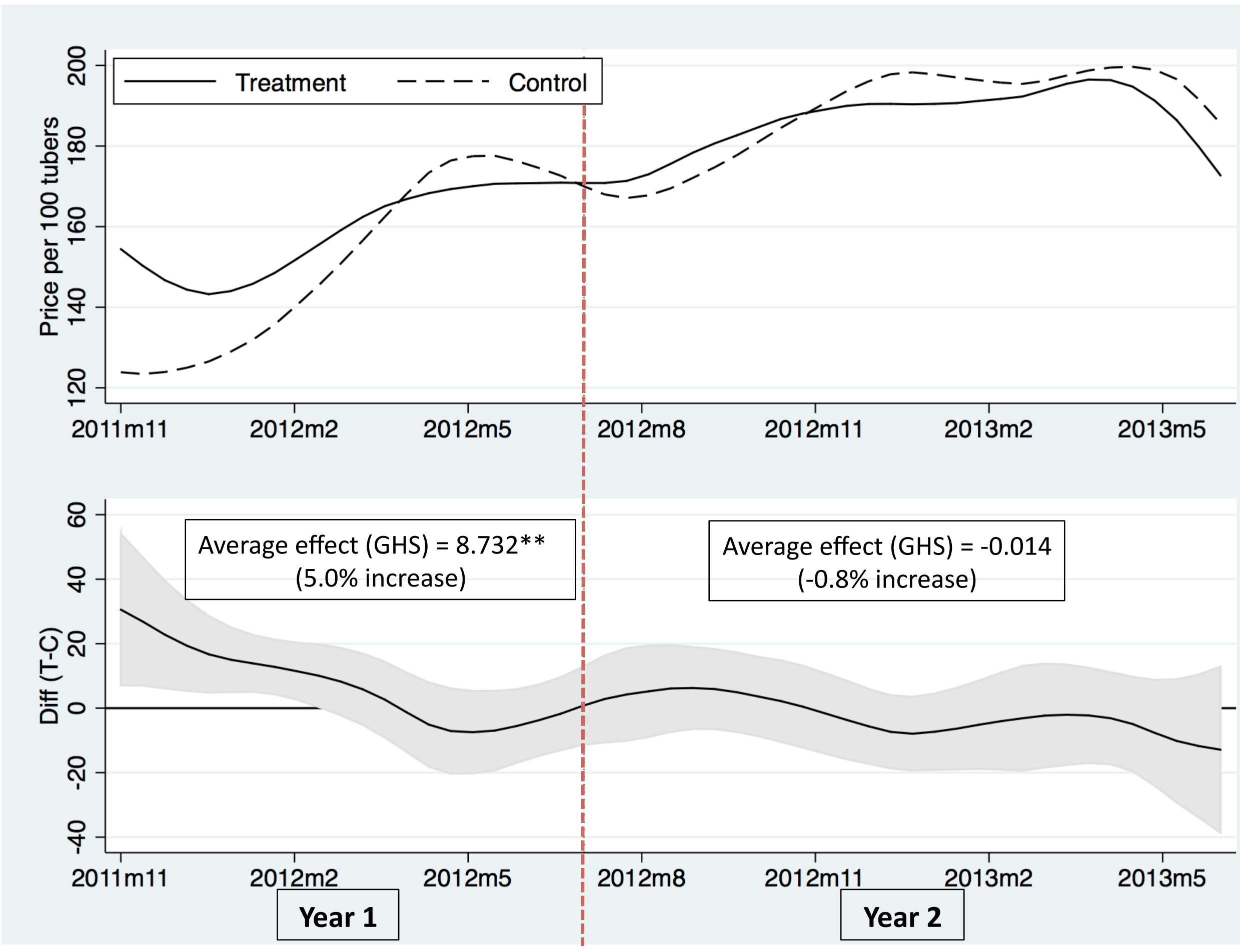
Estimate of spillovers and de-biased treatment effect

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

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

Figure I

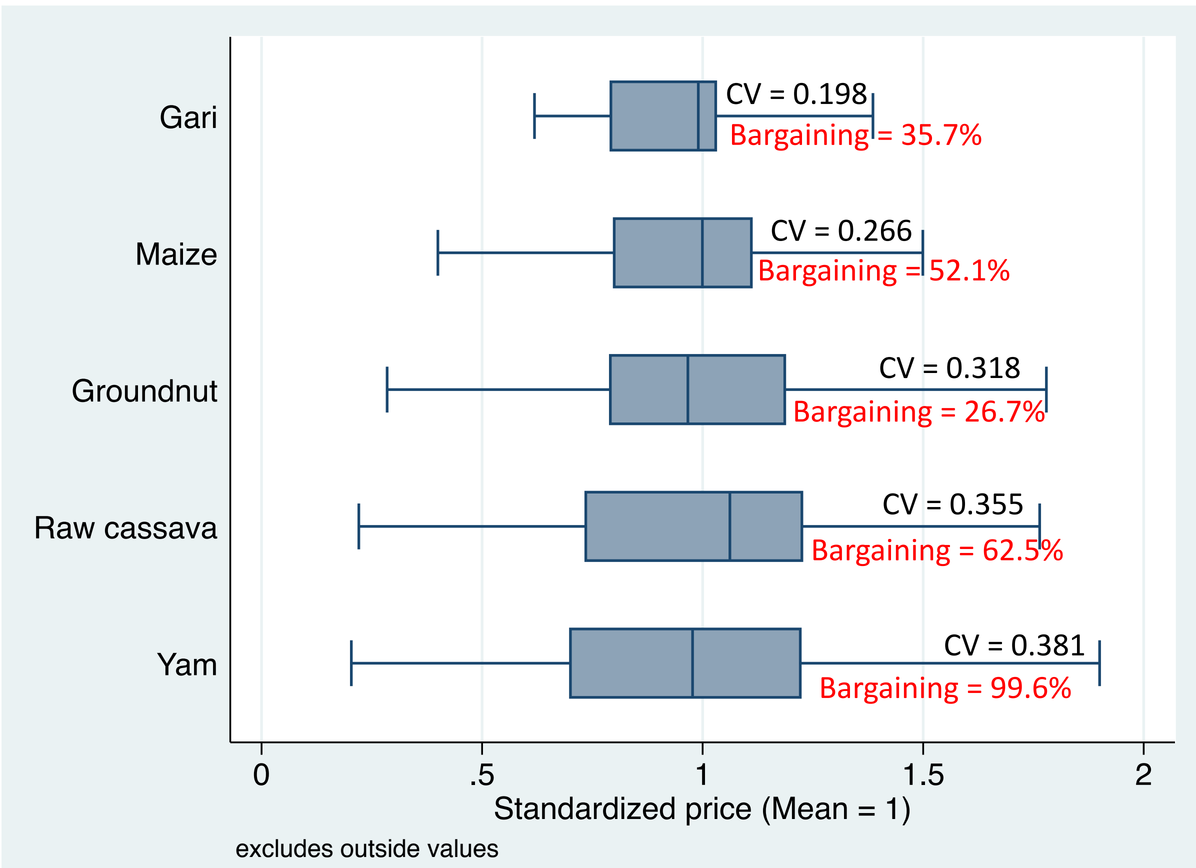
Impact of price alerts on yam prices, assuming no spillovers



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

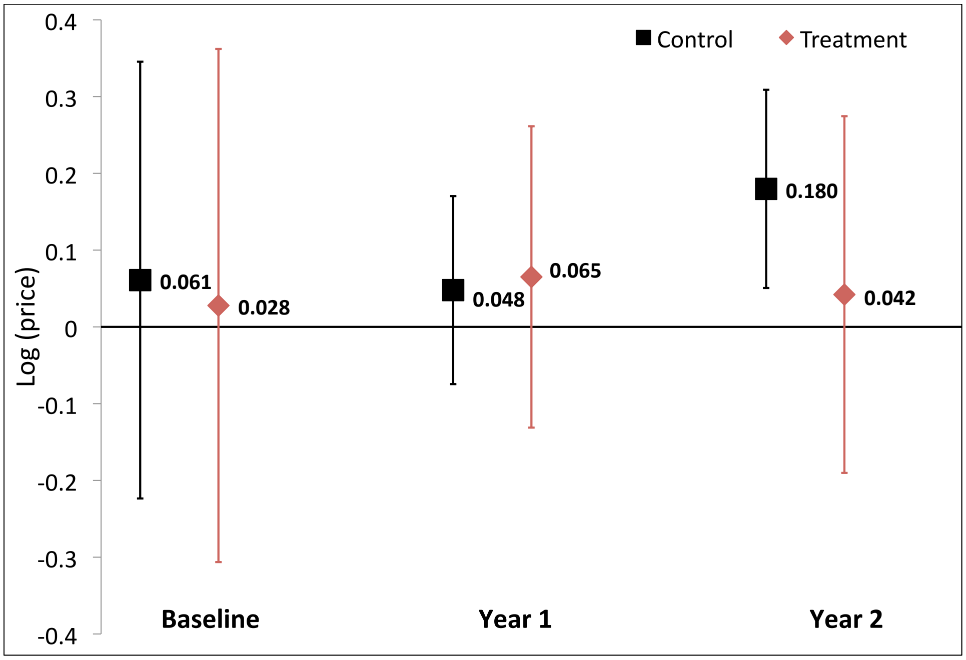


Notes: The figure presents box plots of average annual prices received by farmers in the study in the 2010-11 agricultural season (as recorded in the baseline survey). The figure also reports the mean within-district coefficient of variation (CV) for each crop, and the percent of farmers that report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

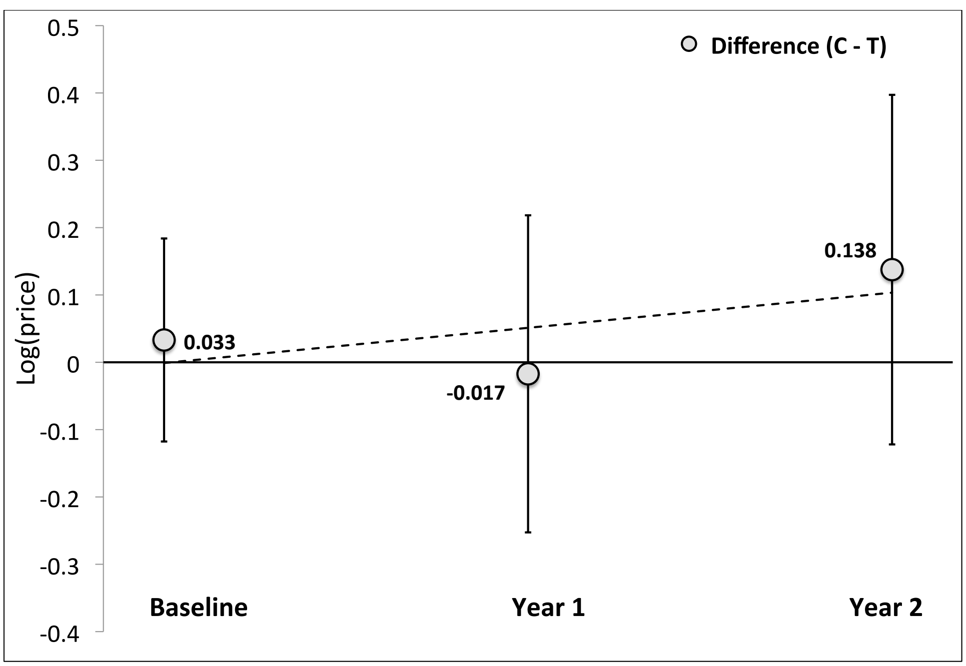
Figure III

Impact of C2T on yam prices over time

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



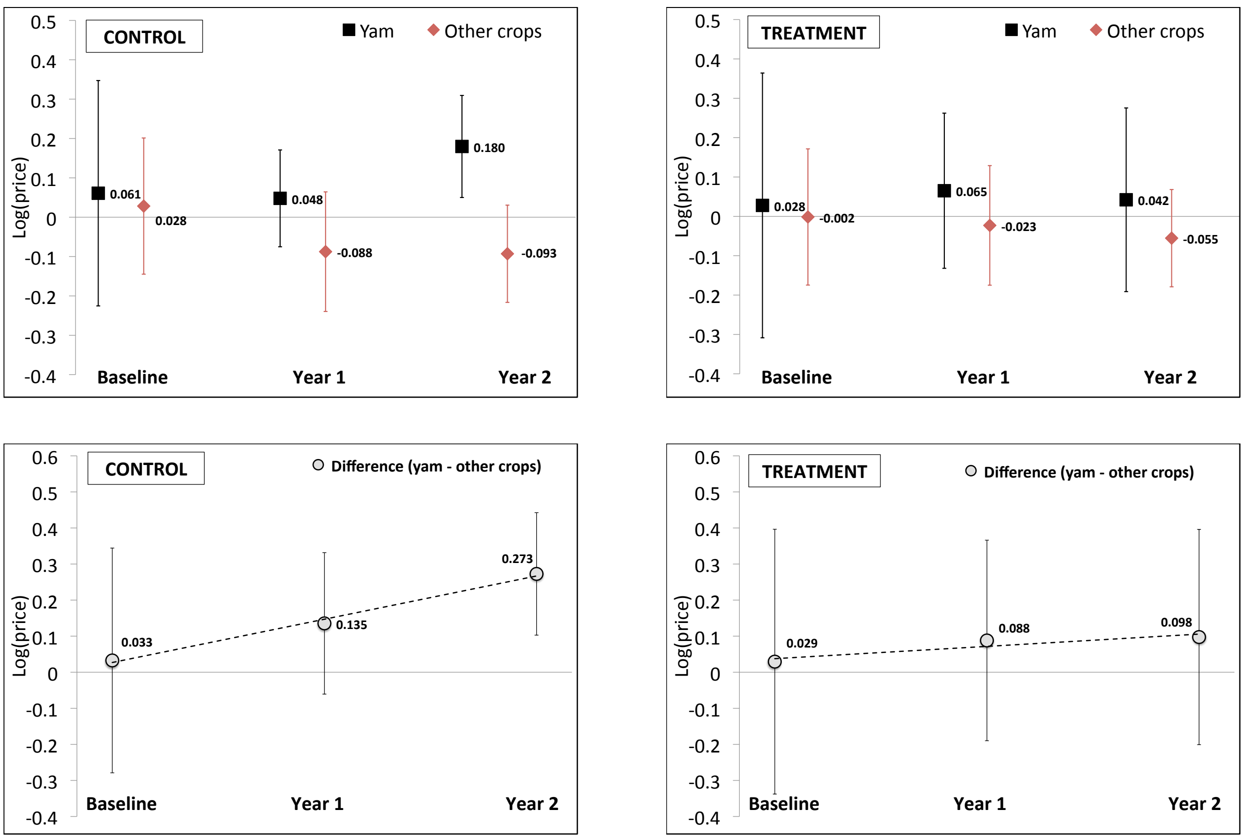
(b) **Difference (control - treatment)**



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

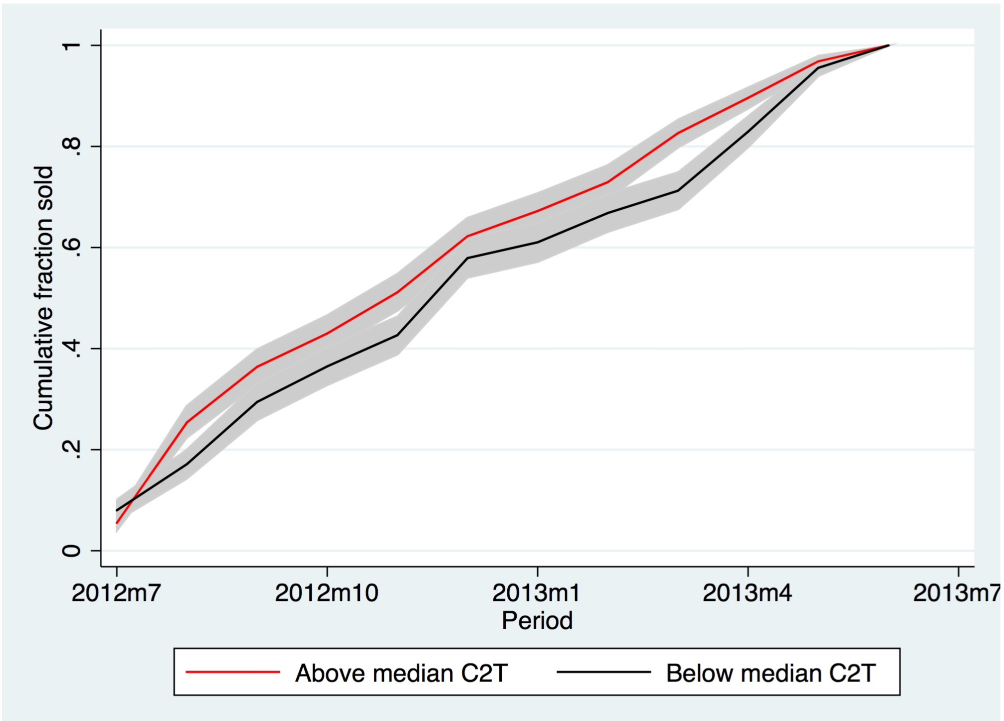


Notes: The top panels plot the impact of C2T on prices for yam and other crops, for control farmers (left-hand side) and treatment farmers (right-hand side). The bottom panels show the difference in the impact of C2T (yam prices vs. other crop prices). Error bars show 95% confidence intervals. Dashed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

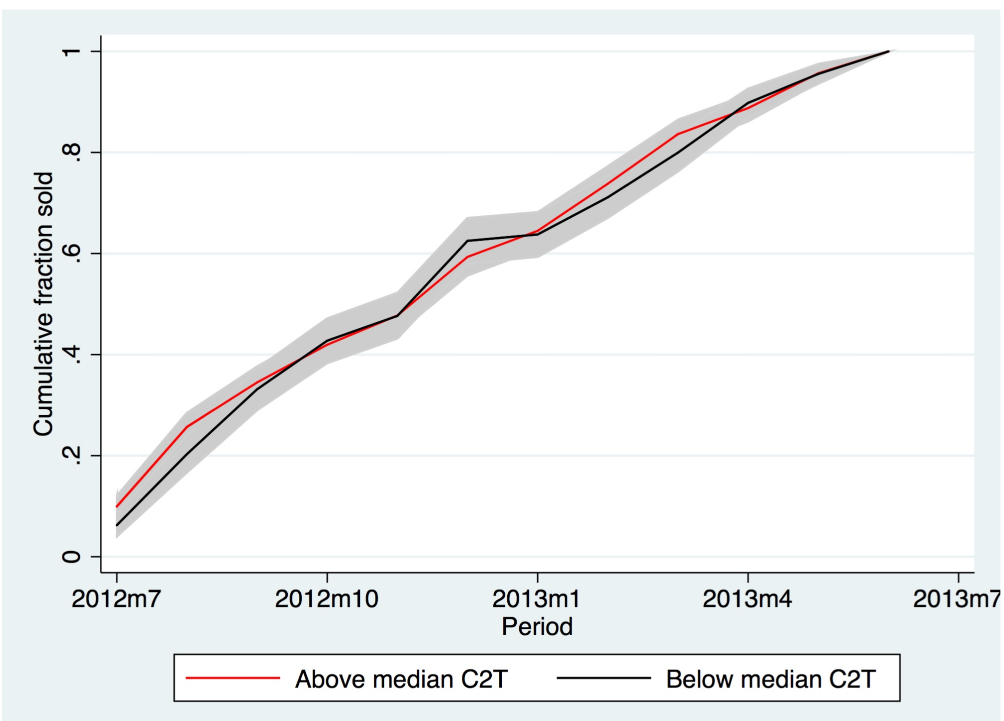
Figure V

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 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 Details of bargaining spillovers model

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

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

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

where

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

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

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

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

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

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

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

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

Proof: (A) Define $\Delta\left(R^I\right) \equiv \Phi E_v[max\{v, R^I\}] - R^I$. Then $\Delta\left(0\right) = \Phi E_v[v] > 0$ and $\Delta\left(v_H\right) = \Phi v_H - v_H < 0$. Since $\Delta\left(R^I\right)$ is continuous in R^I, from the intermediate value theorem we know there exists a value R^{I*} such that $\Delta\left(R^{I*}\right) = 0$, which by definition is a solution to (4). Now we show uniqueness. Note that when $R^I \in [0, v_L)$, $\Delta\left(R^I\right) = \Phi E_v[v] - R^I$, which is strictly decreasing in R^I. To proceed, we note that[51]:

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

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

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

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

Putting this in (4):

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

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

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

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

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

---

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

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

Here we show that, if an equilibrium value for R^{U*} exists, then it must be that $R^{I*}(d) > R^{I*}$ for all d.[52] The argument follows three steps:

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

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

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

Proof: (A) is self-evident. As respondents, informed and uninformed farmers face the same price offer because traders cannot distinguish between the two types. As proposer, the informed farmer can always mimic the uninformed farmer's strategy and achieve payoffs that are at least as high.
(B) Let O^I be the expected value of being the proposer for the informed farmer, and let O^U be as defined in (8) in Appendix B.3. Given the informed farmer's optimal strategy, $O^I = E_v[max\{v, R^{I*}\}]$. Since O^I is always strictly greater than v_L, (B) always holds when the uninformed farmer is playing the corner solution (offering v_L). To see that (B) also holds when the uninformed farmer is playing the interior solution, note that, for all values of $R^U \geq 2v_L - v_H$, $O^U(R^U)$ is strictly increasing in R^U. It follows that, for all d,:

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

where the first inequality follows from $\frac{v_H + R^{I*}}{2} > R^{I*}$ and the second inequality follows from $R^{I*} \geq R^{U*}$ (proved in (A)) and the fact that O^U is strictly increasing.
(C) We prove this by contradiction. Assume that there exists a $\hat{d} \in [0, 1]$ such that $R^{I*} = R^{U*}(\hat{d}) = \bar{R}$. Then the optimal strategy for the trader is to offer a price equal to $min\{v, \bar{R}\}$. It follows that

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

we can write the continuation values of informed and uninformed farmers as:

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

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

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

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

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

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

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

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

where

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

and

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

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

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

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

where:

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

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

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

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

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

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

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

Proof: (A) follows from

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

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

(B)

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

(C)

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

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

(A) Define:

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

Then

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

and

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

where

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

(B) Define:

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

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

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

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

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

we can conclude that

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

which proves (9). Next, when $R^{I*} > v_L$, it can be shown that $M > v_L$[53] so that:

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

When $R^U > \bar{R}^U(d)$, replace the $F(M)$ in (7) with the expression in (10) to conclude, after

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

algebraic simplification, that Z is equal to the value $\tilde{Z}$ defined above. When $R^U \leq \bar{R}^U(d)$, $F(M) = 0$ and thus $Z = R^U$.

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

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

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

and

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

Finally, define:

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

$L(R^U)$ represents the discounted continuation value of the informed farmer at any R^U when the probability of pooling is zero. Since $\frac{d\tilde{O}^U}{dR^U} \in (0,1)$ from Lemma A1, $\frac{dL}{dR^U} \in (0,1)$ as well. Further, $L(0) > 0$ and $L(R^{I*}) < R^{I*}$[54], so there exists a unique fixed point of L, $R^{UL} > 0$, such that $L(R^U) \gtreqless R^U$ as $R^U \lesseqgtr R^{UL}$. This is illustrated graphically below in Figure D1.

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

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

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

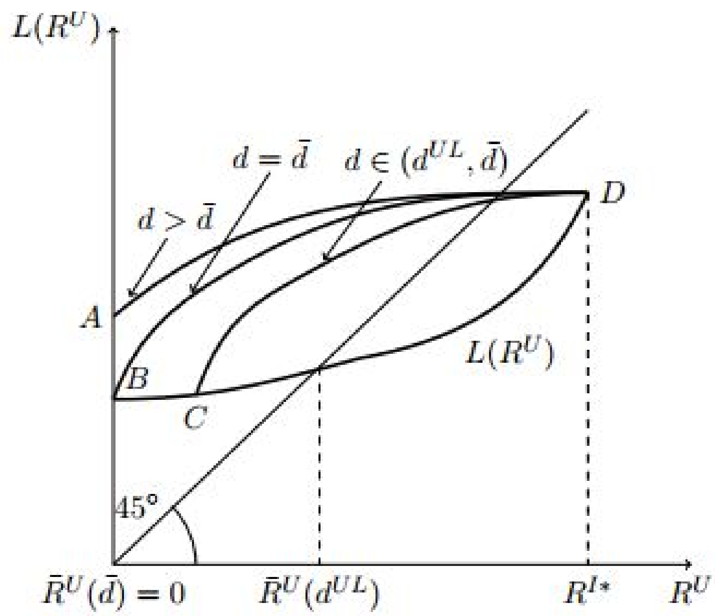
Since $\tilde{O}^U$ and $\tilde{Z}$ are quadratic in R^U so too is $\tilde{Y}$. By our assumption that $2v_L - v_H < 0$, we know that $O^U = \tilde{O}^U$ for all R^U. Thus, in cases where there is a positive probability of pooling,

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

$Y = \tilde{Y}$. In cases where there is zero probability of pooling, $Y = L(R^U)$. To summarize:

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

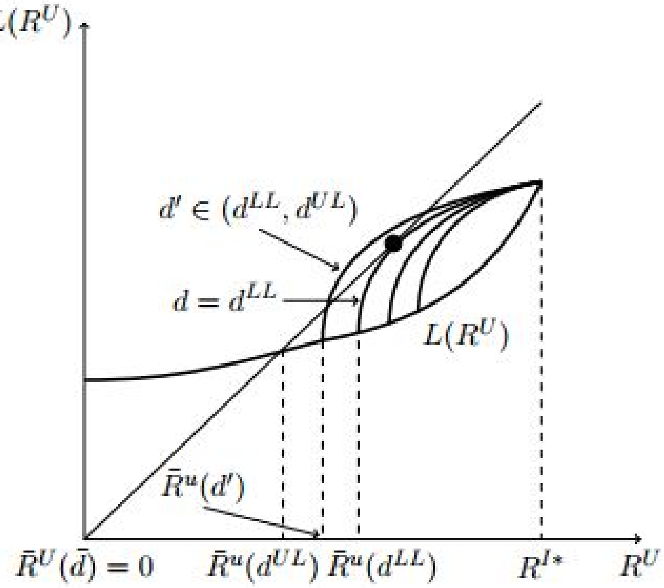
Figure D1: The Y function for $d \in (d^{UL}, 1]$



Consider Figure D1 above. The curve AD represents the Y function for a given $d \in (\bar{d}, 1]$; curve BD represents the Y function when $d = \bar{d}$; and the curve BC along L and CD along $\tilde{Y}$ represents the Y function for a given $d \in \left(d^{UL}, \bar{d}\right)$. Recall also that $\tilde{Y}$ is quadratic so it is either concave as drawn in Figure D1, linear or convex. At each of points A, B, and C we have $\tilde{Y}\left(R^U\right) > R^U$. Also $\tilde{Y}\left(R^{I*}\right) < R^{I*}$. Lemma A3 below implies that in each of these cases Y admits a unique fixed point R^{U*} in $(0, R^{I*})$.

Figure D2 gives examples of Y for $d \in (0, d^{UL}]$. Y is equal to L at all R^U from 0 to up a point $\bar{R}^U(d) > \bar{R}^U\left(d^{UL}\right)$ and thereafter it becomes equal to the function $\tilde{Y}\left(R^U\right)$. The $\tilde{Y}\left(R^U\right)$ function is quadratic in R^U, so it is either concave (as drawn in Figure D2), linear or convex and it shifts

FIGURE D2: The Y function for $d \in (0, d^{UL}]$



up as d gets smaller. Suppose that for some $d < d^{UL}$,

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

One can show that for all d sufficiently small, (11) will hold.[55] Hence $\mathrm{d}^{LL} > 0$. Since $\tilde{Y}\left(R^U\right)$ is increasing in d, this means that (11) is also true for all $d' < d$. Define d^{LL} to be the supremum of all $d < d^{UL}$ such that (11) holds. If (11) holds for all $d < d^{UL}$ then $d^{LL} = d^{UL}$. For all such d values, there is only fixed point at $R^U(d^{UL})$.

For cases when $d^{LL} < d^{UL}$, fix any $d \in (d^{LL}, d^{UL})$ and define R^{LL} as the value of R^U where $\tilde{Y}(d^{LL})$ is tangent to the 45 degree line. Then, using Figure D2 as a guide, it should be clear that $\tilde{Y}\left(\bar{R}^U(d), d\right) = L\left(\bar{R}^U(d)\right) < \bar{R}^U(d)$ and $\tilde{Y}\left(R^{LL}, d\right) > \tilde{Y}\left(R^{LL}, d^{LL}\right) = R^{LL}$ so from Lemma A4 there is one fixed point of $\tilde{Y}$ on $(\bar{R}^U(d), R^{LL})$. Similar arguments show that there is one fixed point on (R^{LL}, R^{I*}). In particular, when $d \in (d^{LL}, d^{UL})$, $\tilde{Y}$ has three fixed points: one at $\bar{R}^U\left(d^{UL}\right)$, one in $(\bar{R}^U(d), R^{LL})$, and one in (R^{LL}, R^{I*}).

Lemma A3. *Let f: $[x_1, x_2] \to [x_1, x_2]$ be continuous and increasing with slope everywhere strictly*

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

less than 1 (i.e., for all $x' < x''$, $f(x'') - f(x'))/(x'' - x') < 1$. *Suppose that* $f(x_1) > x_1$ *and* $f(x_2) < x_2$. *Then* f *admits a unique fixed point on* $[x_1, x_2]$.

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

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

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

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

B.4 Proof of Corollary 1

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

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

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

B.5 Proof of Proposition 3

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

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

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

Since $\frac{v_H + R^{I*}}{2} > R^{I*}$, $P^I(d)$ is decreasing in $\mu^I(d)$. Hence, it is sufficient to prove that $\mu^I(d)$ is increasing in d. The intuition is that the probability that the trader is the proposer in a succesful bargaining round depends positively on the probability of pooling which is increasing in d from Corollary 1. The formal derivation of $\mu^I(d)$ is as follows. When the farmer is the proposer trading occurs with probability $(1 - F(R^{I*}))$. When the trader is the proposer trading occurs only under pooling, and therefore with probability $\pi(d)$. Hence the total probability that a bargaining round is succesful is $w(1 - F(R^{I*})) + (1 - w)\pi(d)$. It follows that $\mu^I(d) = \frac{(1-w)\pi(d)}{(1-F(R^{I*}))w + (1-w)\pi(d)}$ which is increasing in $\pi(d)$, hence in d from Corollary 1, completing the proof.

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

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

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

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

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

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

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

Follows immediately from (A) and (B).