Restoring Trust: Evidence from the Fertilizer Market in Tanzania.

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Abstract:

Many farmers in low income countries believe that fertilizer in their local markets is adulterated. Recent evidence shows that adulteration is rare, and urea fertilizer is high quality. A lack of reliable information about fertilizer quality depresses farmer demand, reducing crop yields and negatively impacting livelihoods. We implemented a randomized controlled trial across 100 market clusters and 148 associated villages in an agricultural region of Tanzania. Fertilizer sellers and farmers in the treatment market clusters are exposed to a low-touch information campaign consisting of posters, pamphlets, and meetings explaining that urea fertilizer tested in those markets was found to be of good quality, with the required amount of nitrogen. We find evidence of substantial concerns among farmers regarding the quality of urea fertilizer at baseline, with 80% of farmers reporting beliefs that fertilizer is of bad quality. The information treatment significantly improves farmer beliefs about fertilizer quality and reduces variance of beliefs across farmers within treatment villages. Treated farmers buy more fertilizer. On the extensive margin, i.e. the farmer's decision whether or not to use fertilizer, we estimate an effect size of nearly 40%. Our design and data collection allow us to estimate effects on the supply side as well, where agro-dealers in treated markets report increased sales but stable prices.

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1. Introduction

The functioning of rural markets is critical to economic growth and agrarian change. In Sub-Saharan Africa, inputs for agricultural production including pesticides, fertilizers and seeds are typically sold in local markets characterized by a small number of retail shops (Fafchamps 2003, Barret et al. 2018). These shops, commonly known as agro-dealers, sell agricultural inputs, but also can serve as credit-providers, information points, network hubs, and buyers of agricultural output (Tadesse and Shively 2013).

The importance of these shops to rural economies and the fact that they tend to have little proximate competition has led to suspicions regarding their operations and practices among consumers and policy-makers. Indeed, economic theory would predict that such circumstances -- few dealers in an often poorly connected rural area -- give rise to local monopolies, extracting economic surplus from a non-coordinated population of poor farmers.

This study focuses on farmer suspicions of the behaviour and incentives of local agro-dealers and whether and how such suspicions can change in response to new information. Prior data collected from farmers in Tanzania suggests that farmers often believe that local dealers tamper with the quality of fertilizers or allow it to degrade (Michelson et al. 2021, Hoel et al. 2021). Newspaper articles about the prevalence of so-called fake fertilizer help support these suspicions. Evidence from numerous studies however indicates that fertilizer, especially urea fertilizer,[1] is of good quality and these suspicious are unfounded (Sanabria et al. 2013, Ashour et al. 2019, Michelson et al. 2021, Hoel et al. 2021).[2]

Evidence suggests that these suspicions nevertheless reduce the demand of farmers for urea fertilizer, whose nitrogen is an essential input for sustained agricultural productivity (Sanchez 2002, McArthur and McCord 2017). Michelson et al. (2021) show that farmer suspicions about fertilizer quality in Tanzania lower the stated willingness-to-pay for fertilizers (as opposed to circumstances where the farmers are confident about quality). Other concurrent studies document similar effects on willingness-to-pay for fertilizer (Hoel et al. 2021) and for other inputs in other African countries (as in Gharib et al. 2021 and Maredia et al. 2018).[3] In those markets where farmers have pessimistic beliefs about quality and associated lower willingness-to-pay, the demand curve for fertilizer could shift inward, decreasing fertilizer demand.

Farmer beliefs about urea fertilizer quality are incorrect. In equilibrium, they do not appear to have converged to the truth of good quality in the marketplace. Our previous research establishes evidence of an information friction in these markets (Michelson et al. 2021). Hoel et al. (2021) shows that beliefs about bad quality urea can persist and farmers can fail to learn about true quality if farmers misattribute a bad yield outcome to bad quality fertilizer rather than to bad weather, incorrect or insufficient application.

[1] In this paper we study urea fertilizer and will henceforward use fertilizer to mean urea, which is 46% nitrogen by weight, widely used by small farmers, and essential for crop growth.

[2] Results of these studies differ from Bold et al. (2017) who find highly variable nutrient content and high average nitrogen deviations of 30% in urea in Uganda. Bold et al. is an outlier in the literature and their results on urea have not been corroborated by any of the other studies done in Uganda (Sanabria et al. 2018B, Ashour et al. 2019) or elsewhere in the region (Sanabria et al. 2018B)

[3] See also Prieto et al. (2019) for evidence on the maize market.

What are the market-level effects of correcting false beliefs? Could a realistic information campaign about good fertilizer quality change beliefs and would belief change lead to changes in demand? How would sellers respond?

We combine a randomized controlled trial at the market level with detailed panel data collection from both farmers and fertilizer sellers in Tanzania's agricultural Morogoro Region. Our main dataset is based on a dealer census: 429 agro-dealers in all 100 market centres in Morogoro Region; and a representative sample of 1,480 farmers from 148 villages within 3-7 kilometres of these market centres. We proceeded with randomization at this market-village cluster level.[4] Randomly assigned treatment markets and their associated villages received pamphlets and posters with the message that all urea tested in that market in a study we ran in 2016 (Michelson et al. 2021) was found to be of good quality, with the required nitrogen content. We also held in-person information meetings about the results of previous urea quality testing in treatment villages. The information disseminated was based on tests of samples purchased using mystery-shoppers in 2015 and 2016 and tested in laboratories in the United States and in Kenya which established that urea quality was excellent across market centres. Control markets received this information treatment 12 months later.

We begin by establishing farmer quality concerns about urea fertilizer, with findings consistent with Michelson et al. (2021), and Hoel et al. (2021) but in a larger, and regionally-representative sample of nearly 1500 farmers across 100 market centres and 148 villages. We find that farmers' concerns about fertilizer quality are substantial. We asked the farmers to imagine the following scenario: "If ten farmers, like you, purchase 1 kg of fertiliser at Market X this week, how many would be bad quality and how many would be good quality?" This question was preceded by a transcript explaining what we meant by "good" and "bad" quality fertiliser. The market X was a market near the farmer's village. Looking at the average number of "bad" bags across the three markets elicited, we note an average of three (out of ten). However, there is a wide variety, about 20% of farmers think there are no issues with fertilizer quality in the elicited markets while the beliefs of all the others follow a roughly normal distribution. We expect our intervention to affect some segment of those approximately 80% of farmers who report concerns at baseline.

We then show that farmer beliefs change in response to the information treatment. Being exposed to the information treatment leads to an increase in the belief that the bags are of good quality by about 7 percent. These beliefs, on their turn, influence the uptake of fertilizer. On the extensive margin, i.e. the farmer's decision whether or not to use fertilizer, we note an effect size of 35 percentage points for urea, representing an effect size of nearly 40%. We do not observe any positive effects on other, non-urea, fertilizers. We also observe a positive relationship between the treatment and the amount of urea used, however, this relationship is not statistically significant. This indicates changes happen mostly on the extensive margin. Using a smaller sample of 30 villages, we find an increase in the use of hybrid maize seed, a well-known complementary investment, but not other agri-chemicals, such as pesticides. These overall changes in behaviour do not appear to impact maize yields or beliefs regarding maize yields, the most common food crop in the region.

[4] The treatment assignment of markets and villages was not cross-randomized. Instead, each village which had only one market nearby (exceptions were made for those villages near multiple markets as we explain in Section 4.2), it would receive the same treatment allocation as the nearby market. Essentially, we "double-down" on the treatment. We considered cross randomization, but decided against this for three reasons. First, the power calculation indicated that we might no longer be able to pick up effects of either treatment. Second, the scope for spillovers in this type of design is substantial, and something we purposefully tried to limit. Third, this double intervention is most realistic from a policy point of view. We return to these points in Section 4.

A unique feature of our study is that we work directly with both sides of the market, sellers and buyers. Our design allows us to estimate the near term impacts of the information intervention and the change in farmer beliefs on local market centres and sellers. As demand increases, we would expect the market to adjust, and perhaps the price to increase and the overall quantity sold to increase as well.

We find no effects on urea (nor other fertilizer) prices in treated markets. Prices are regulated by the government though anecdotal evidence suggests that these set prices are not always adhered to; the topic of prices is therefore somewhat sensitive. Similarly, we find no effect on the likelihood of selling urea fertilizer, or any other fertilizer, over the agricultural season, by treatment status. However, we note 30% effect on the intensive margin on the amount of urea sold (which is almost statistically significant). [5] Consistent with possible positive effects on sales, we find a positive effect on the dealer's willingness-to-pay for such a testing service. We asked the dealer to imagine that a highly regarded local university would provide a regular service of testing fertilizer and giving up to date certificates; how much would they be willing to pay for such a service? The agro-dealers in the control group are willing to pay, on average, 3,300 TZS (1.4 USD) while the agro-dealers in the treatment group are willing to pay, on average, 5,100 TZS (2.2 USD) with the difference between the two groups being almost statistically significant).

The origins of these beliefs and relevant theories of beliefs formation made us optimistic that an information campaign might work to change not only farmer beliefs but also demand. Overall, our results finding a strong effect of a low-touch information campaign on both beliefs and behaviours are in contrast with the previous literature. Prior studies where researchers have provided information to farmers have produced largely disappointing results. Providing farmers with information about a new crop insurance product does not result in much uptake (Jensen et al. 2016), and providing them information about their soils nutrient limitations (in the absence of additional liquidity) does not result in the use of fertilizer (Harou et al. 2021). In effect, in these contexts information does not appear to be the primary constraint. More substantial effects are reported by Bai (2018) who considers the impact on sellers from a certification scheme for watermelons in China. Hsu and Wambugu (2021) provide quality assessment in the maize seed market in Kenya, a context in which evidence suggests the seed quality concerns of farmers are valid (Tjernstrom and Lybbert 2020).[6]

In the next section, we provide the necessary background information on fertilizer use and the fertilizer market in Tanzania. Section 3 outlines the experiment and selection of dealers and farmers for the survey. Section 4 provides information on the data collected. Section 5 presents the analysis and results. Section 6 concludes with a discussion.

[5] Note that this coefficient is based on the sample of dealers which do sell urea in both periods, a certain degree of sample selection might be at play. In addition, we have capped the distribution to exclude high numbers.

[6] Providing information to rural households on migration possibilities (Tjaden and Dunsch 2021), returns to education (Jensen 2010), the benefits of smoke-free cooking and of cleaning drinking water (Bonan et al. 2021; Bennear et al. 2013), testing for HIV/AIDS (Thornton 2012), and food safety at local food stalls (Gianmarco et al. 2020) all came back with positive, but somewhat disappointing results. Generally knowledge increases but behaviour changes little. See also Nygvist et al. (2018) who provide information on antimalarial medicine in Uganda and Jin and Leslie (2009) who give information on restaurant hygiene. An exception is Annan (2020), who assesses the impacts of an information intervention revealing seller's fraud regarding to mobile money payments and documents a significant impact on market activity, with reduced fraud, and increased consumer welfare as a consequence.

2. Background

Morogoro is the third largest region in Tanzania, occupying 8.2% (72,939 sq. km) of the country's mainland area, with a population of 2,218,492 (URT, 2012). According to the 2007/08 Agricultural Census 98% of households in Morogoro grow maize, yet the majority are net buyers of maize and other staple foods. Self- employment in agriculture provides the main income stream to households, and supports nearly all household activities. Smallholder agriculture accounts for 80-90% of the region's economic activity (Mutabazi, 2015). Most families consume what they grow, trade goods for other necessities, and sell their crops or livestock for income (EDI, 2007). Despite the various uses for their agricultural products, smallholder households often fall short of their daily income needs. Most households live at or below the poverty line, and many live in extreme poverty (URT, 2010a). FAO (2014) estimated the number of chronically hungry people in Tanzania have risen from 28.8% in 1992 to 33% in 2013.

Fertilizer use is low in the region. While the government recommends application of 60 kg of urea and 40 kg of DAP for one acre of maize cultivation in the region, current application rates average less than nine kilograms of fertilizer per acre (IFDC 2012). Urea is the most widely stocked and sold fertilizer by retailers in Tanzania (Benson et al., 2012) and also the most commonly used fertilizer in Tanzania. It is 46% nitrogen by weight and among the most important fertilizers for plant development. Other commonly available fertilizers in Tanzania include diammonium phosphate (DAP), calcium ammonium nitrate (CAN), and nitrogen-phosphorous-potassium fertilizer (NPK).

Beliefs about fertilizer quality are among many constraints characterizing agricultural inputs users and markets. Constraints to fertilizer adoption and use among small farmers include insufficient access to credit, uninsured production risk, output market price volatility (Cardell and Michelson, 2021), incomplete information about farmers' plot-level nutrient limitations (Harou et al. 2021, Corral et al 2021). Among these constraints, fertilizer quality and quality beliefs is less studied, only recently receiving attention from researchers and policy makers. Quality beliefs are unlikely to be the the primary constraint to fertilizer adoption and use, but they may contribute to and exacerbate the effects of other constraints.

It is well established that farmers are suspicious of the quality of fertilizer in their local markets (Bold et al. 2017; Ashour et al XX; Sanabria et al. 2013), especially in the Morogoro Region of Tanzania (Hoel et al. 2021; Michelson et al. 2021). Concerns about quality are reinforced in the media (Kasumuni 2016), though work by our research team in 2018 investigating the journalistic sources of a 2016 piece in national newspaper The Citizen that reported that 40% of fertilizer for sale in the country was counterfeit found that the story itself was based on rumors.

Research has also now convincingly established that the agronomic quality of fertilizer in the region, especially urea fertilizer quality, is good (Sanabria 2017A; Sanabria 2017B; Michelson et al. 2021; Ashour et al. 2017). One likely reason that there is little fertilizer tampering (either adulteration or full-scale counterfeiting in which a non-fertilizer substance is sold as fertilizer) is that it is difficult to do profitably: the cost-benefit from this action does not add up on the seller side. Adulteration or counterfeiting may make more economic sense for other agricultural inputs; pesticides for example can be diluted with water and such dilution is unlikely to be visually detected by consumers (see, for example, Haggblade et al. 2021 on pesticides in Mali and Ashour on herbicide in Uganda).

3. Information intervention

The intervention builds on a previous research project conducted in Morogoro Region between 2015 and 2017. That project (Michelson et al. 2021) conducted a census of stores and market centres selling fertilizer, used mystery shoppers to sample the fertilizers, and had the fertilizers tested in both Kenyan and United States labs. The urea sampled in that study was found to be of excellent quality, in all 100 local market centres and stores.

As with the previous research project, this project was conducted in close collaboration with Sokoine University of Agriculture (SUA). SUA is a public University based in Morogoro, Tanzania. The university is an established and well-respected agricultural university in Sub-Saharan Africa, with extensive experience in agricultural technology development, such as hybrid seeds, and extension. We found working with a local, well respected partner, essential to the success of the project, as it instilled confidence and trust not just in a our research activities, but perhaps more importantly, in the information interventions we rolled out.

Prior to finalising the intervention design, we conducted two focus group interviews with 40 farmers in the region in two non-sample villages in the Singida region in November 2018. We inquired about farmer concerns regarding fertiliser quality, confirming our results as published in Michelson et al. (2020). We also asked if we had information to share about fertiliser quality, how best should we share this. Farmers noted the importance of large colourful posters in the market, and information at the point of purchase -- agro-dealers' shop counters and windows. At the time, we were considering various communication methods, including media and phone-based methods. However, our focus group interviews and our baseline survey results confirmed that farmers in this region rely mostly on face-to-face provided information, and generally do not get their information from the radio or via the phone. The focus group interviews and baseline survey also indicated that organising village meetings would require involving the local government extension agent as this individual is largely perceived to be a trusted person.

Out intervention consisted of two components: A market-level intervention and a village-level intervention. Both interventions took place in the period December 2018-January 2019. We sequenced the intervention and baseline interviews as to avoid any immediate spill-overs between markets, and between markets and villages. To this end, we first completed the baseline interviews at the control markets, followed by the interviews and the intervention at the treatment markets. Then, we completed the baseline interviews at the control villages, followed by the interviews and the intervention at the treatment villages.

It should be noted that our enumerator and intervention team were graduates and students from SUA. As such, they were familiar with the research protocols established, and fluent in the local language, Kiswahili. All interviews and the information related to the market and village interventions were conducted in this local language, Kiswahili.

To further facilitate the separation of intervention and interviews, we had two teams on the ground. One team conducted the interviews and one team conducted the intervention, with the interviewing team always arriving and finishing their activities prior to the intervention team. We completed the interviews prior to the interventions because we needed to elicit pre-intervention agro-dealers' and farmers' beliefs and behaviour. The timeline of our intervention, with respect to the sampling and randomization, is outlined in Figure 1 below.

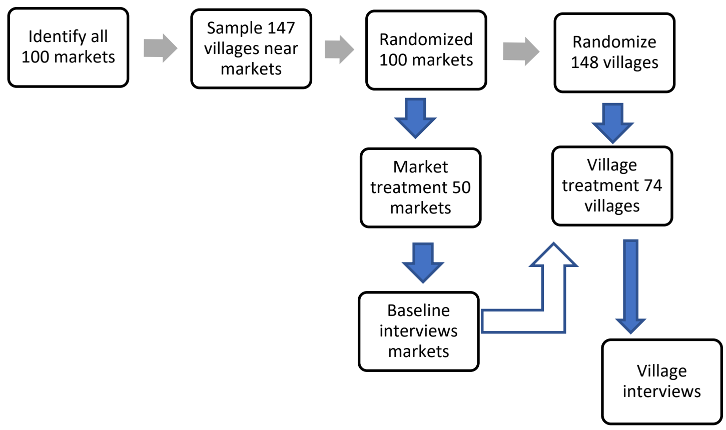

Figure 1: Timeline of the study intervention in December 2018 – January 2019

3.1. Market intervention

When the team arrived at the markets in late 2018, they first reported to the government authorities and presented them with a letter of approval to conduct research in the location, as required by Tanzanian regulations. Then, they scouted the market, identifying all possible agro-dealers in the location as per protocol described in the next section (4). Once the interviews were completed, the companion team visited the agro-dealer and informed him or her of the tests we had completed earlier, as reported in Michelson et al. (2021). The transcript used was as follows:

> "We are from Sokoine University of Agriculture (SUA). We have some important information for you. In 2016 urea samples were taken from this market for testing. Results show that fertilizer tested contained 46% Nitrogen which is required. We have come with signs to be displayed in your shop and we will also display the same around this market/village. We are also requesting to distribute the pamphlets to customers/farmers who come to your shop. The fertilizer testing will also be done in June 2019."

Then, we shared a poster with the agro-dealer and a set of pamphlets (see the pictures in the appendix materials). For each market, we had 8 posters and 100 pamphlets to be distributed among the agro-dealers. If any posters were left over (which was mostly the case), see the distribution of agro-dealers per market in Table 1, we hung these at prominent locations the market centre. We did not approach any customers in the markets, but if approached, shared the pamphlet with them and indicated our purpose also following a standard script, and shared a pamphlet with them. This happened quite often, as business continued as usual while we were in the shop.

Table 1: Descriptive statistics of markets and dealers interviewed at baseline

	Control markets	Treatment markets	P-value t-test
Number of sellers/market			
Not selling at the time of interviews	0.91	1.39	0.26
Selling at the time of interviews	1.50	2.95	0.05
Number of markets	46	43	

While at baseline, in the winter of 2018-19, we conducted this intervention only among the 50 randomly selected treatment markets, we rolled out the same treatment to the control markets after the completion of the endline interviews in the winter of 2019-2020.

a. Village intervention

The village intervention proceeded in the same manner as the market intervention. After introductions of the research team to the local village head and extension officer, again, a step which is required by the Tanzanian authorities, we proceeded to interview the 10 randomly selected households (we return to this selection in Section (4)). These households were contacted in advance, and appointments had been made for their interviews. After the ten interviews were completed, we invited all farmers in the village to a common location, such as outside the village office. It is important to note that at this point, all farmers in the village were invited, not just the farmers whom we had interviewed prior to this point.

The supervisors of the enumerator team conducted the intervention. They were assisted by the enumerators who distributed pamphlets to the attendees at the end of the meeting. We followed the script below. Essentially, we informed the attendees about the test we had conducted prior. As we will explain in the next section, section (4), each village has been sampled within a certain radius of a selected market. Thus, each village had what will term an associated market. Also, we will explain in Section (4) in detail, the village intervention linked up to the market intervention, with village treatments referring and building on the local market's intervention. So for mostly reasons of statistical power, the village treatment followed the market treatment (in almost all cases). This implied that within the treatment villages, we could notify the farmers of our tests conducted in this associated treatment market. So the village was only informed of the associated market's status (if this associated market was a treatment market, which was mostly the case).[7]

Note that if the farmers form beliefs in a sophisticated manner, updating their beliefs about all markets, our strategy of informing farmers about one market only should not be of relevance. The choice to inform markets only about market was hence made for simplicity purposes – it avoided a situation in which we would read out all the treatment markets – many of which the farmers would have likely not heard.

The script used was the following:

> "Fertilizer is one of the important inputs in agricultural production. We have so different brands and types of fertilizer. Which types of fertilizer do you use? [Ask responses]. Fertilizers are for basal and for topdressing. Fertilizers, including urea, have nutrient

[7] In the exceptional case that the associated market of a treatment village was a control market, the attendees were informed of the quality of fertiliser in the nearest treatment market (see also randomization section).

standards that ensure that the fertilizer will preserve or improve soil fertility and help the crops to grow. For example, in urea, the most important element is Nitrogen and samples of urea should contain 46% nitrogen. Tests were conducted by the International Institute of Tropical Agriculture and Sokoine University of Agriculture (SUA) in 2016 in collaboration with researchers from the United States. The World Agroforestry Center (ICRAF)Laboratory in Nairobi and Thornton Laboratory in the United States performed the testing. The shops did not know that the fertilizer purchased for testing was for a test and did not influence the results in any way. All the urea tested in from market [INSERT associated market name] in 2016 contained 46% Nitrogen. This means it met national and international product standards. The research found NO evidence of adulterated urea fertilizer. [Allow for questions from the attendees] We are here today to give you this important information and we have the pamphlets for you."

Thus, at the end of our script, the supervisors answered questions. While we had restricted the script to only one market, we had decided to answer truthfully if farmers asked about other markets in their location. For instance, if the farmers asked about whether the quality of a nearby treatment markets, we answered in the affirmative, but if the farmers inquired about the control markets, we referred to the ongoing nature of our study. [8]

During the meeting with farmers, different questions were asked by both farmers and village leaders. Some farmers wanted to some fertilizer brands seem to be good in adding fertility to the soil compared to other brands. The Urea manufactured YARA company was mentioned to stay longer in the soil than urea from other companies. Other farmers claimed to applied fertilizer and still get low yield. From the village government offices, the main questions were on the supply side of the fertilizer. Village leader complained that the government subsidy program was not efficient due to its bureaucracy. They recommend that subsidy fertilizer should directly be given to farmers and not through agents. Another concern from the village leaders was how the fertilizer quality and prices can be controlled especially in their areas.

During the survey, one of the notable achievements was the positive response on the side of the recipients (farmers) on the intervention. Farmers noted to feel honoured having been given the opportunity to hear the feedback and each provided with a brochure, something which they commented "had never happened before" in other research projects. Hence, the large majority of farmers appreciated us for our efforts to make sure that farmers were kept up to date with the research findings. The information provided about fertilizer quality was perceived to be timely and valuable to both farmers and agro-dealers (we will provide statistics on these in Section (4)). A few farmers were less satisfied, however, as they had their hoped for free fertiliser bags.

At the end of the village treatment, we left pamphlets with the villagers as well; around 135 pamphlets per village. It should be noted that unlike in the market treatment, due to financial reasons, we did not follow up with a village treatment in the control villages after the endline data collection in the winter of 2019-2020.

[8] In a subset of the villages, a mistake was made, and the supervisors informed the attendees of all markets, meaning they did not indicate any specific market, but simply stated that all markets in the region had been tested. While this was a correct statement, this complication will no longer allow us to do by-market comparisons well; and we will have to take into account this implementation error in the analysis.

4. Sample, randomization and data collected

4.1. Sample

We first selected the markets, then the agro-dealers. Based on the market selection, we select villages, and finally farmers.

We start with a census of agro-dealers in the Morogoro region of Tanzania. This census was identified as follows. We started with the census data from Michelson et al. (2021) – which had identified all sellers of fertilizer as of 2016. This census contained the locations of 100 markets, and named 226 agro-dealers.

Our first task when we re-visited these 100 markets in late 2018, was to update this census list. We expected a certain degree of market churning, and starting with an up-to-date listing was hence important for this project. We instructed our research team to locate all shops selling agricultural inputs for farmers, open at the time of the visit. As we will discuss later, this method resulted in a slightly larger set of shops than we had intended, including shops who had never sold, and had no intention of ever stocking or selling fertilisers. We remedied our approach at the endline which took place in early 2020 where we redefined the eligible set of shops as those who were currently selling fertiliser, had sold fertiliser the previous year, or were planning to sell fertiliser in the future. In the analysis, we limit our main regressions to those shops who sold fertiliser at either base or endline, and/or planned to sell fertiliser in the future, or had sold fertiliser in the past (prior to our baseline).

Table 2 gives the sample numbers at base and endline. We have only 231 agro-dealers whom were interviewed in both rounds. Then we have indications of a substantial amount of market churning. Data collected from the field indicates that this is due to new shops opening, others closing, as well as some shops opening only for certain months of the year (during the main agricultural season early in the year), and, finally, some shops relocating. In addition, our definition of agro-dealers was not fully comparable across the rounds, but, this, as we indicate in the appendix, did not contribute substantially to the mismatch in numbers one can note in Table 2.

Table 2: Number of agro-dealers interviewed at base and endline

	Interviewed at baseline	Not interviewed at baseline
Interviewed at endline	231	131
Not interviewed at endline	67	NA

The dataset from Michelson et al. (2021) included the GPS location of each market. Using ARGGIS, we created a detailed map of the region. Figure3 reproduces the map. As you can see, most markets, indicated as dots, are located among the main roads. Essentially, markets are connected via these main roads, serving the hinterland of villages behind them.

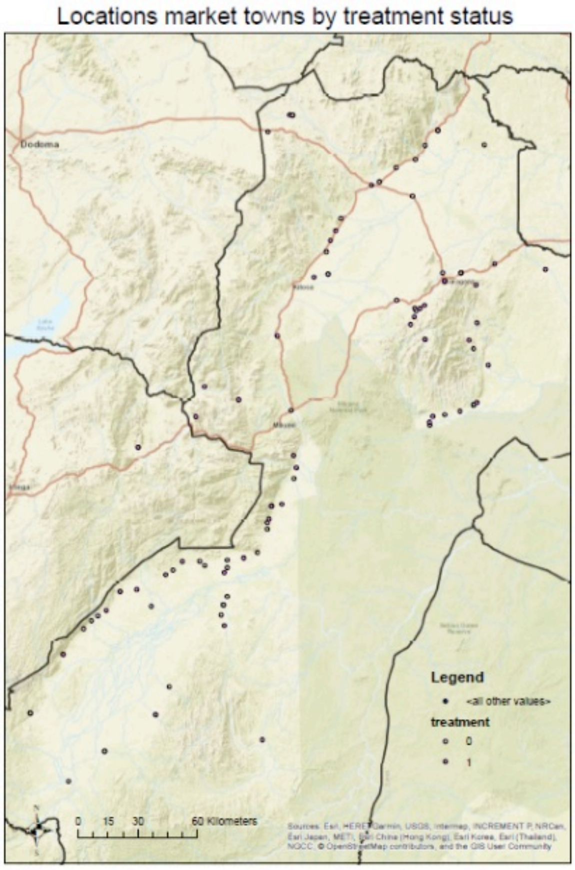

Figure 2: Map of market locations (dots)

We then drew circles of various radii across the markets – an example is given in Figure 3. We then elicited the help of government extension officers to locate all villages within the 3 to 7 km ring. The 3 km minimum boundary ensured that farmers who were located within the market boundaries themselves were excluded, avoiding a situation in which the market treatment and village treatment duplicate each other in the same location. The 7 km upper boundary ensured that the link between the village and the associated market was meaningful, meaning it would be feasible for the villagers to visit the market.

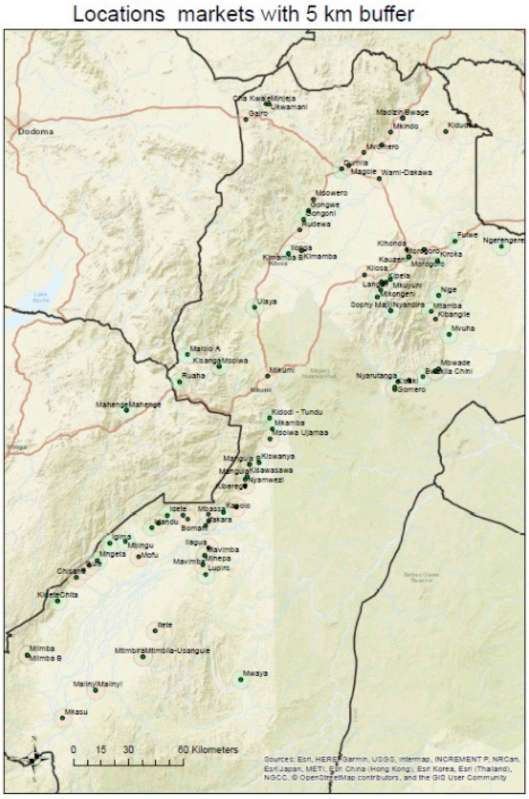

Figure 3: Map of market locations with 5 km buffer zone

With a budget for 150 villages, we randomly selected 148 villages from this full list of villages (we had aimed to select 150 villages, but not all markets had sufficient villages within the 3-7 km ring). As we have 100 markets, this implied that half of the markets had two accompanying villages while the other half had one associated village only (this process was stratified by treatment/control status of the market, and randomized by market).[9] So some markets are linked with two villages, while others only one. As noted earlier, we refer to this linkage as the villages and their "associated" markets. Figure 4 shows the end result of this process. We note that in practice, the villages selected do not always fall within the intended ring radius.

[9] In practice, as the rings of some markets overlap, some villages could have been 'selected' by more than one market. To avoid this possibility, we developed a Stata program such that villages which were 'taken' by one market could no longer be 'taken' by another market. As we randomized the sequencing in terms of which market's villages was selected first, we expect no particular bias to have occurred as a result of this process.

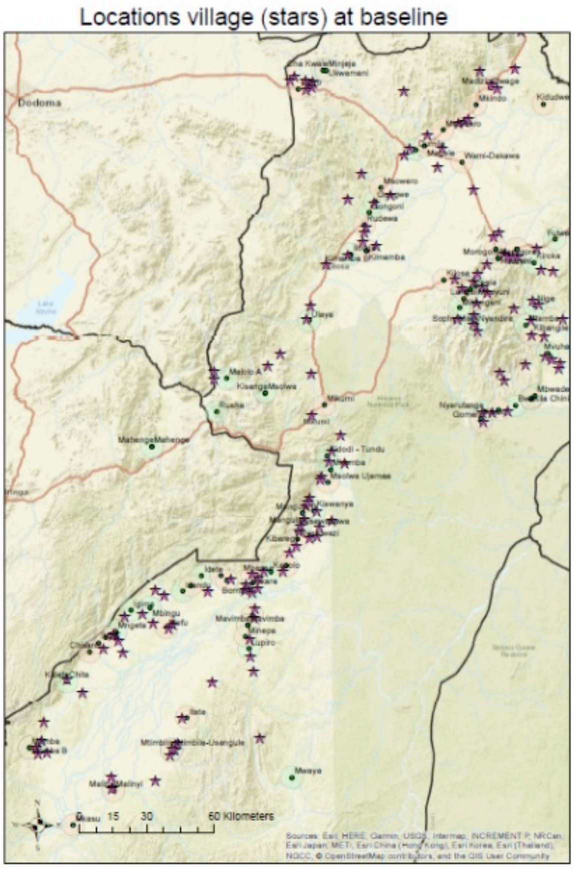

Figure 4: Map of the village locations (stars)

The sample of households consisted of ten randomly selected households from a household census list obtained from the same government agricultural extension officers in advance of the baseline survey in the selected sample villages.

4.2. Randomization

We first randomized the markets, and then randomized the villages.

In late 2019, we randomized half of the markets into the treatment group and the other half into a control group. So we have, 50 treatment markets and 50 control markets. The treatment markets received the market intervention treatment immediately after the baseline interviews in December 2018 and January 2019, while the control markets, as mentioned in Section (3), received the same treatment after the endline data collection in 2020. Overall, our treatment resulted in a well-balanced treatment and control group. We further discuss balance across agro-dealers by treatment status in the Section (5).

Each village was also assigned a treatment status, with 74 villages (out of 147) in the treatment group. If the village was within the 7 km radius of only one market it was assigned the treatment status of this market, which necessarily would have been the "associated" market. If the village was within a 7 km radius of more than one market, and these markets had the same treatment status, the village was assigned the same treatment status as the markets.

It is important to note that the treatment assignment of markets and villages was not cross-randomized. Instead, for each village which had only one market nearby, it would receive the same treatment allocation, essentially we are "doubling-down" on the treatment.

While this method does not allow one to consider the cross-treatment effect of village and market treatments, we perceived this method in which we doubled-down on the treatment to be simpler, and more credible: villagers who were informed about the treatment could see the posters in the market about which we informed them. The design also avoids some of the immediate spill-overs one would expect from treated markets to control villages (even though, as we will see later, we were not entirely able to avoid all such spill-overs).[10] It addition, it should be noted that from a budgetary and sample size perspective (there were only 100 markets in the area), a power calculation indicate that using a cross-randomized design, we might no longer be able to pick up effects of either treatment.

4.3. Data collected

We collected data at from both agro-dealers and farmers before, during, and after the intervention. Figure 5 gives an overview of the survey timeline.

We started in December 2018 with an in-person baseline survey among all 298 agro-dealers and 1479 farmers (aimed at 10 farmers in each of the 148 villages). After the baseline survey was completed, we started a weekly phone survey, dialling up all agro-dealers weekly until the end of July 2019.

In September and October 2019, we conducted an endline survey on the phone among all farmers and all agro-dealers. We had attrition in both samples, as we discuss in the next section, but more significantly so among the agro-dealers. Due to this significant attrition, especially among the agro-dealers, we obtained additional funding and returned to all markets in-person in January 2020 for another endline interview; this time in person. We also selected 30 villages, randomly, by village treatment status, for an in-person visit as well (although only 29 were reached due to weather conditions).[11] In addition to these, we also collected observational data at the market level each time we visited. Finally, in December 2020 we called a (stratified, and randomly drawn) subset of 38 farmers and 20 agro-dealers with some additional qualitative questions.

[10] Matters became more complex in case a village was located within the 7 km radius of more than one market, and these markets were of different treatment status. In this case, the status of the village was allocated as per a Binomial distribution which followed the same probabilities as the nearby market. For instance, if 1 out of 3 nearby markets were treatment, the village was assigned as a treatment village with probability $p = 1/3$.

[11] The goal of these in-person farmer endline visits was to shed some light on how our village treatment was perceived by the farmers, to understand why it was effective, or why it was not effective in some cases. In addition, we wanted to understand how the treatment affected the farmers outside of the immediate impact on fertiliser quality beliefs and fertiliser use. In effect, farmers might recalibrate the various inputs they use, and also adjust the use, of, for instance, hybrid seeds. And as a result of these changes, farmers might alter their expectations for the farm as a whole, and obtain higher crop yields.

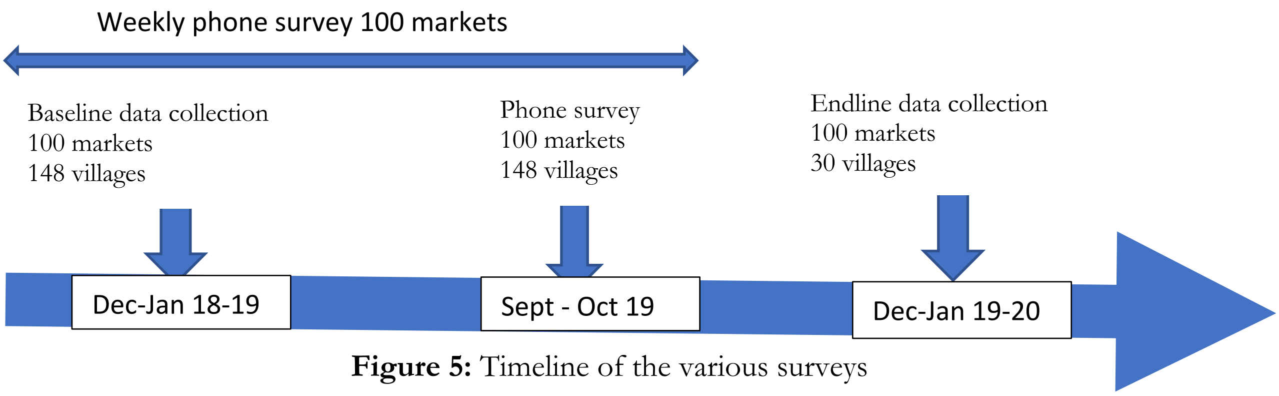

Figure 5: Timeline of the various surveys

Before we detail the content of these interviews, a few notes on the process. It should be noted that, as we visited the same markets we had visited in 2016, agro-dealers were somewhat more cooperative than otherwise expected. As noted earlier, the Tanzanian authorities, through its Tanzanian Fertilizer Regulatory Authority, had introduced a new set of rules and regulations during the same time of our project. Hence, perhaps understandably, some agro-dealers suspected us to be affiliated with the government. We had made it a point to arrive in cars with the logo of Sokoine University, and to remind agro-dealers of our previous work with him. Hence, while we still had cases of agro-dealers closing their shop and leaving when they spotted our arrival, these were rare, and all in all we were able to interview almost all agro-dealers at baseline. The interviews took place in the shop, and were frequently interrupted by customers. We had trained the enumerators to adopt a relaxed and flexible attitude, and to work around the business' needs and take time to pause the interviews as need be. If the shop-owner was not available, another knowledgeable member of staff was interviewed. For the phone interviews later on, the agro-dealers received 5000 TZ (equivalent to 2 USD) per call. The need for some form of payment was obvious as agro-dealers, and even farmers, commonly ignored calls from unknown numbers.

For the farmers' interviews, we selected the main decision-maker regarding agriculture in the household. We defined a household in a traditional manner as individuals eating from the same kitchen on a daily basis for the last 6 months (excluding newborns who are not subject to the 6 months criteria). We made sure to invite the respondent, and all other interested household members, to the intervention event later on in the village. The interview took place either in the house of the respondent or in a common village location. Care was taken to ensure confidentiality, so in the case of a common location, enumerators were well spaced. The farmers too, received a small payment of 5000 TZS for the phone interviews in September 2019. Despite this incentive, attrition is still substantial in these phone interviews. We suspect two factors were driving this high attrition. First, cell phone reach is still limited in many areas of Tanzania, with no cell phone towers within miles of some villages. Second, at the time of our phone survey, the government of Tanzania had introduced a new regulation[12] to require all phone owners to register their SIM card. This resulted in several households no longer owning phones, or changing phone numbers.

Prior to training recruitment of enumerators and supervisor was done. A total of 17 enumerators and 4 supervisors were hired. Three types of training were conducted to enumerators during baseline and endline. The purpose of intervention training was done to introduce the research project to the team to be able to answer questions when asked by respondents (farmers and agro-dealers). During this training, enumerators were emphasized to follow the research ethics and

[12]https://www.tcra.go.tz/document/The%20Electronic%20and%20Postal%20Communications%20(SIM%20Card%20Registration)%20Regulations,%202020

protocol; how to distribute pamphlets and posters and to interview agro-dealers. Another training was for farmers' and dealers' survey in which enumerators were introduced to the questions on how to ask and filling in the questionnaire. Lastly, training was done to 4 supervisors on research ethics, protocols and distribution of roles. It should be noted that while we used pen and paper surveys at baseline, we collected data via tablets at endline. The latter greatly reduced the need for data cleaning, as checks were built into the software.

Agro-dealer survey

We collected baseline data on business locations, shop and owner's characteristics, asset ownership and asset rentals, stock facilities and current stocks, supplier chains and characteristics, and sales. We also conducted a series of in-person observations, detailing visible inventory, posted certifications, employers and customers at the time of interview. In the endline in person interviews we recapped information on business location, shop and ownership characteristics, assets ownership and asset rentals for those business which were new in the market, or not interviewed at baseline for other reasons (we return to the issue of attrition and new business entry below). We also collected another round of data on stock facilities and current stocks as well as sales. We also inquired about the perception of the market treatment, and repeated our in-person observations. The phone interviews focussed on sales only.

Sales: Quantities and Prices

We considered six different inorganic fertilisers: Urea, NPK, DAP, Minjinju, CAN and SA. For each type, we mark whether or the dealer has them in stock at the time of the interview, and whether or not the dealer ever sold this type of fertiliser. At baseline, we also note the manufacturers/brands of the fertilizer stocked. We then indicate the total amount of fertiliser sold, in kg or metric ton, in the 2018 year for each type of fertiliser. The phone endline survey inquires about the start and end date of fertiliser sales (for those businesses only selling during the season), and the amount sold of urea, NPK, and DAP in kg (to date, which is from December 2018 till September 2019). The endline in person survey also notes, for each type, whether or the dealer has them in stock at the time of the interview, and whether or not the dealer ever sold this type of fertiliser. We then note down the total quantity sold in the 2019 year, again in kg or metric ton, and the current price at the time of the interview for a 50 kg bag.

In order to obtain additional detail on the dynamic effects of the market treatment, we also collected weekly data. The weekly data inquires about the number of weekly customers, the weekly sales of urea, NPK and DAP, and the price that week in the market for urea, NPK and DAP (in kg).

Due to the stringent enforcement of the fertiliser maximum prices during our project year, in 2019, prices were a sensitive topic. It was for this reason that we avoided asking for prices at baseline, and during the weekly data, we only inquired about the "going rate in the market", attempting to make the question less personal. In the endline phone survey we inquired about this government maximum price for urea, DAP and NPK in bags of 50 kg, and recall, in the endline in person survey, after having established a close relationship with most dealers, we finally asked about their own sales price of the various types of fertiliser. Note that the weekly data used a different unit than the endline surveys – kg instead of 50 kg. We expect this to matter. Fertiliser is typically sold wholesale in bags of 50 kg. Government regulation prohibits sales in smaller quantities, but this regulation is routinely violated. Effectively, most small farmers only purchase a few kg at a time, and hence, most agro-dealers split up the bags of 50 kg into smaller bags. Previous research however pointed at the presence of wholesale discounts for those who purchase larger amounts.

WTP for intervention

At the endline in person survey we inquired about the willingness-to-pay for fertiliser quality certificates of Sokoine Agricultural University. We asked this question among agro-dealers in both treatment markets as well as control markets. While the formulation of the question was identical across the two types of dealers, those who were not familiar with our posters and pamphlets were shown a copy. We asked: "Imagine that Sokoini University could come and test your urea fertilizer and establish that the quality of the urea fertilizer meets official government regulations. Then, once this is done, the university would provide you with these types of pamphlets and posters. What is the highest price you would be willing to pay for doing this type of test, today?" Note that we did not use a formal BDM mechanism. However, we made sure to emphasize that we were looking for the highest price they were willing to pay, and not what they thought the cost was of such an endeavour.

Farmer survey

We collected baseline data on respondent characteristics, such as age, sex, education, and the degree of risk aversion. We also collected information on land ownership. Then, we collected information on which markets the respondent visited in the past twelve months prior to the interview, and information on mineral fertiliser purchases. We return to the latter below. We also collected information on the use of organic fertiliser and the baseline beliefs on fertiliser quality. We also return to these beliefs below.

During the September phone survey, we asked the farmer whether they had visited any of three pre-selected markets, and whether they received information, either through the village treatment, or directly in the market through the market treatment. These three pre-selected markets were the nearest three markets to the village, and correspond to the markets used in the beliefs elicitation exercise. However, an error was made during this exercise, and we had to repeat this question once more during the endline in person survey. The in person endline survey was, recall, only conducted in 29 villages. The goal of this survey was to gain a better understanding of the village treatment, and also the overall changes in the decisions made after exposure to village, and possibly, market treatments. We hence inquired in detail about the sources of information, the perceptions of the village treatment, and the maize production techniques used, as well as maize yield. Recall that maize is the most common crop in the region, widely consumed by households, and cultivated by 85 percent of the farmers in our sample at the in-person endline survey. We also asked, once more, about all markets the farmer had visited in the previous long-rains season, and whether or not the farmer had been exposed to any information treatments at that market in the form of posters and pamphlets.

Fertiliser purchases

All fertiliser purchase decisions and follow-up questions refer to activity during the long rains season, which runs from February till June each year. At baseline, we asked the farmer how much fertiliser he or she bought for each fertiliser type, and where the fertiliser was bought, allowing for multiple markets per fertiliser. In effect, the data elicited the purchases in two ways. First, market by market, and within each market, fertiliser by fertiliser. Second, fertiliser by fertiliser, and within each fertiliser, by crop. If the respondent stated to have used fertiliser, we followed up this inquiry by asking about the total acreage cultivated and the total acreage fertilised. We also asked about the crops which were planted.

The phone survey in September also inquired about the purchases of fertiliser during the long rain season (this time referring to the 2019 season). Due to time limitations during a phone call, this question was asked fertiliser by fertiliser, but for only a single market. We did inquire about the price paid (per kg) as well. At the endline in-person interview, we asked the farmer about fertiliser purchase plans for the coming season.

Beliefs about fertiliser quality

Beliefs elicitation is now common in agricultural economics. We build on the earlier work of Grisley and Kelologg (1983), Lybbert et al. (2007), Maertens (2017) and the overview study of Delavande et al. (2011) to elicit the beliefs regarding fertiliser quality during the baseline in-person interviews. We ask the question: "if ten farmers, like you, purchase 1 kg of fertiliser at Market X this week, how many would be bad quality and how many would be good quality?" This question was preceded by a transcript explaining what we meant by "good" and "bad" quality fertiliser. This type of formulation, as opposed to a probabilistic statement, did well in pre-testing, as farmers commonly purchase 1 kg, which they would then judge to be either of good quality or of bad quality. It is the same question used in Hoel et al. (2021) to elicit fertilizer beliefs in Tanzania. The Market X in the statement refers to three pre-selected markets. These are the three nearest markets to the village, and hence also includes the what-we-termed "associated" market. We repeated this question during the endline phone interviews.

Beliefs about maize yields

At the endline in person survey, we inquired about the beliefs of maize yields. We used a standard method in which we asked the respondent to make a density distribution using ten equal-sized blocks, each representing 10% probability. After inquiring about the minimum and maximum per acre yield in a unit of choice, we presented the farmer with three equal sized boxes, showing the range between this minimum and maximum. We then asked the farmer to distribute the ten blocks in the boxes, tracing a density.

In the winter of 2020-21, we conducted a further 58 interviews via the phone, due to covid limitations at that point in time. We interviewed 38 farmers, randomly selected from both treatment and control villages, stratified by village status and whether or not they were using fertiliser at baseline. We also interviewed 20 agro-dealers, again stratified by market status. The interviews with the agro-dealers focussed on their perceived constraints and limitations, and in the case of the treatment markets, the perceived impact of our treatment. The interviews with the farmers looked at market visitations, beliefs formation, social learning, and the impact of our village treatment. We also conducted two interviews with government official, to gain a better understanding of the regulatory context.

5. Descriptive statistics

5.1. Farmer descriptive statistics

Table 3 introduces our sample of farmers. We present data on our analysis sample – this is the baseline-endline panel data, where the endline was conducted via the phone,[13] and consists of 995 farmers (out of the 1,479 farmers interviewed at baseline). While this represents significant attrition, it is expected that in a phone survey that not everyone is reached as some villages are out of cell tower reach, some farmers have no phone credit, and others have changed or switched of

[13] The endline in-person survey was restricted to 30 villages. We use this in-person endline to validate the phone survey rather than to establish treatment effects.

their phones. This attrition is not correlated with the treatment status, and as we can see from Table 3 below, the remaining analysis sample remains balanced among across observable characteristics, including demographics, and baseline market visiting and purchasing behaviour.

Table 3 - Column (1) presents statistics on the full sample, Column (2) on the control villages, and Column (3) on the treatment villages. Column (4) presents the p-value of a t-statistic testing the differences between the means of the control and treatment villages. The large majority of the farmers are male. The average age is about 45 years, and their household includes 5.5 members. Farmers own, on average, about 7 acres of land, and have about 15 years of experience with farming. Farmers consider themselves (compared to others) fairly risk neutral. All in all, the sample is balanced across the treatment and control villages, although the control villages are slightly more risk-loving. We note that we control for these characteristics in our analysis.

Table 3: Descriptive statistics farmers at baseline

	Full	Control	Treatment	p-value
Sex (0 = male, 1 = female)	0.4	0.42 (0.02)	0.37 (0.02)	0.15
Age (years)	45	44.79 (0.54)	45.67 (0.54)	0.25
Household members	5.55	5.55 (0.11)	5.54 (0.111)	0.96
Acreage land owned (acre)	6.90	7.33 (0.48)	6.48 (0.34)	0.15
Experience*	16	15.83 (0.51)	16.61 (0.52)	0.28
Risk aversion**	3.17	3.06 (0.96)	3.28 (0.06)	0.02

Notes: Table 3 presents the results of a baseline balance check. Column (1) presents the average and standard deviation of the full sample. Column (2) of the control villages, Column (3) of the treatment villages. Column (4) presents the results of a t-test with unequal variances testing the differences between the treatment and control groups. The sample contains all farmers who were present at both baseline in person interview, and endline call interview. . N = 995 (control = 490; treatment = 505).

*experience refers to the number of years the farmer has cultivated at this location

** risk aversion refers to the categorical answer to the question 'compared to others, how much risk do you take'. Answers are coded from 1 = much fewer, to 5 = much more.

Farmers beliefs are, on average, not as pessimistic as previous anecdotal, and less representative, research had suggested. When visiting the local markets, on average, three out of ten bags are expected to be of poor quality. Recall that we asked the farmers, for three selected nearby markets, to estimate the number of bags which would be of poor quality out of ten bags purchased. This number represents the average of this average beliefs across markets.

However, this number disguises a great deal of individual variation. We present the distribution in Figure 6 and note that there is a wide variety of beliefs: about 20% of farmers think there are no issues with fertilizer quality in the elicited markets while the beliefs of all the others follow a roughly normal distribution.

Table 3: Descriptive statistics of farmers at baseline (cont.)

Fertilizer purchases				
	Full	Control	Treatment	p-value
Fertiliser purchased in last 12 months (yes/no)	0.4	0.38 (0.22)	0.42 (0.02)	0.17
Urea purchased in last 12 months (yes=1/no=0)	0.37	0.35 (0.02)	0.39 (0.02)	0.28
Urea purchased (kg)	41.68	41.93 (7.91)	41.44 (4.93)	0.95
NPK purchased in last 12 months (yes=1/no=0)	0.02	0.02 (0.00)	0.01 (0.00)	0.94
NPK purchased (kg)	1.77	1.07 (0.43)	2.45 (1.55)	0.39
DAP purchased in last 12 months (yes=1/no=0)	0.12	0.12 (0.01)	0.12 (0.01)	0.91
DAP purchased (kg)	9.70	11.74 (3.94)	7.73 (2.22)	0.37
CAN purchased in last 12 months (yes=1/no=0)	0.06	0.05 (0.00)	0.08 (0.01)	0.02
CAN purchased (kg)	4.68	2.68 (0.66)	6.42 (2.26)	0.11
Fertilizer beliefs				
Beliefs – number of bags with poor quality fertilizer (out of ten)	3.04	3.06 (0.11)	3.02 (0.10)	0.79
Market behaviour				
Number of markets visited in past 12 months	1.36	1.34 (0.04)	1.30 (0.42)	0.63
Number of markets where fertilizers was purchased in past 12 months****	0.46	0.45 (0.3)	0.48 (0.03)	0.44

Notes: Table 3 presents the results of a baseline balance check. Column (1) presents the average and standard deviation of the full sample. Column (2) of the control villages, Column (3) of the treatment villages. Column (4) presents the results of a t-test with unequal variances testing the differences between the treatment and control groups. The sample contains all farmers who were present at both baseline in person interview, and endline call interview. N = 995 (control = 490; treatment = 505).

*** Belief refer to the number of bags with poor quality fertiliser the farmer expects out of a total of 10 bags purchased, averaged across nearby markets

**** With the number of markets set at 0 if no fertilizers were bought.

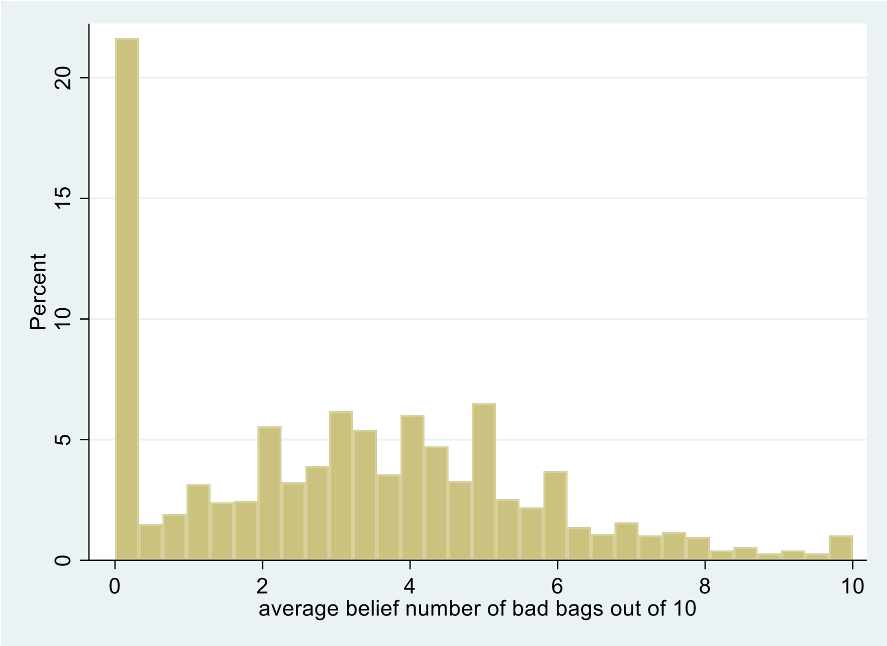

Figure 6: Distribution of the beliefs regarding fertilizer quality at baseline (poor quality bags out of ten)

Figure 6 refers to the number of bags with poor quality fertiliser the farmer expects out of a total of 10 bags purchased, averaged across nearby markets

When regressing these baseline beliefs of farmers on farmer, village and nearby market level characteristics, we note that the factor risk aversion and amount of land owned are the main determinants, with people who are most risk averse and who have more land to have more optimistic beliefs. Slightly larger local markets, and having purchased at that market before, also positively correlates with this optimistic belief (although the latter sign reverses when controlling for village fixed effects).

This reversal points at an interesting role of village-level factors. It is notable that only 16% of the variation in concerns is at the village level, meaning, even within villages, people vary a lot in terms of their concerns. Inspecting the raw data, it appears that there is little agreement within each village as to which one is the better market nearby. In effect, the only time we observe such agreement is when the village is quite isolated and only one market is nearby.

Along the same lines, there are no villages where everyone has no concern. This is a bit surprisingly, as one might have thought that, through repeated interaction within the village with other farmers, a village would have come to some kind of joint understanding of how serious this problem is. Instead, what we see is a varied concerns across farmers within a given village. When we asked the farmers at baseline how the formed their beliefs about fertilizer quality they mentioned both individual and group experiences: about half of farmers referred to the yield results on their won farm, while the other half referred to yield results on others' farms. like their own results on the farm, or their close contact's results. Interestingly, when splitting the sample as to whether or not the farmer has ever tried fertilizer, those who have largely rely on own experiences to form beliefs, while those who do not rely on the experiences of other farmers.

There are a few reasons which might be underlying this lack of joint, i.e., village-level, understanding. First, this issue of fertilizer quality might simply not be important enough, within the bigger picture of everything thing else that is going on in their lives. Secondly, it is well established that advice taking and beliefs formation is not an easy process, and people tend to be overly confident of their own prior opinion even in the face of new, contradicting incoming advice – we return to this discussion in the conclusion.

We were aware of this literature when we designed the treatment, and made sure to ensure the treatment was provided by a credible source of information in an official manner. One of the authors is from Sokoine Agricultural University, the most well-known agricultural university in Tanzania with a well established reputation for agricultural extension. In addition, our university researchers worked together with local government extension agents and set up a version of the frequently used village presentations. We had pamphlets and posters to illustrate our point, and allowed sufficient time for questions and discussions.

In terms of fertiliser use, our baseline sample could be broadly divided in three categories. About 40 percent of the baseline farmers have never used mineral fertiliser. Another about 20 percent have used it, but did not use it in the season prior to the baseline survey. Then, only about 40 percent of baseline farmers used mineral fertiliser in the last season. This implies that when we look at the impact of the treatments, we will have to distinguish between the internal and the external margin effects.

This is particularly true when considering the different types of fertilizer. As is clear from Table 3, urea is the most commonly used fertiliser, with 37 percent of the sample purchasing urea in the past 12 months prior to the interview. The other types of fertilisers, such as NPK, DAP, CAN and others are significantly less common. There are no statistically differences between the control and treatment groups (with the exception of CAN purchases).

Farmers report using fertilizers on a wide variety of crops, but primarily on maize and paddy rice. Conditional on using fertiliser, 40% of farmers use it on all of their land. But this does not mean the correct doses are used. Figure 7 gives the distribution of the kg of fertiliser used (in total, across all fertiliser) per acre of land. One can see that the bulk of the farmers are located between 0 and 20 kg/acre, which is still well below the recommend amounts.

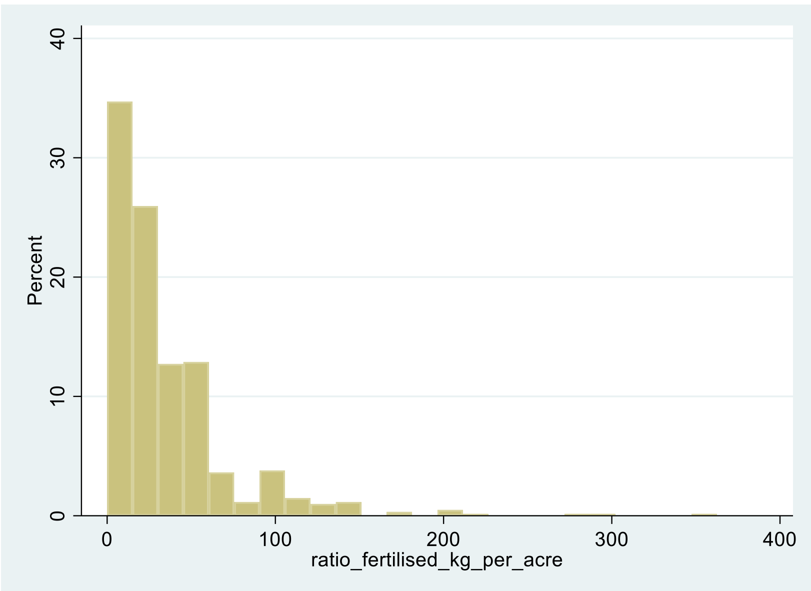

Figure 7: Kg/acre fertilizer applied, conditional on using fertilizer

Baseline statistics indicate that farmers visit between one and two markets for any reason in the past twelve months prior to the baseline interview, but generally purchase fertilizer only at one market. It is notable that farmers report visiting markets a handful of times each year and not more frequently; not, for example, on a monthly basis. It is also notable that the market where farmers report purchasing their fertilizer is generally not the one that they visit the most. So one could imagine a scenario in which one passes by a market on a regular basis, but travels a bit further afield to purchase fertilizer.

5.2. Agro-dealer descriptive statistics

In Table 4 we present the descriptive statistics of the agro-dealer. The sample we consider for our analysis is the balanced sample of agro-dealers for which we have information at both base and endline. Of the 298 agro-dealers interviewed at baseline, we lose 22% through attrition. Much of this is due to businesses closing, although some of it is also because we altered the definition of agro-dealer from base to endline, and because we could not always interview someone (in effect, one using a more consistent definition, this attrition is much lower and stands at 17%.) It should be noted that this endline attrition does not correlate with the market treatment in place. We report the results of the following regression in Appendix Table A3, and note that there is no systemic relationship between not being interviewed at endline and the treatment.

$$present\ at\ endline_{j,m} = \beta_0 + \beta_1 T_m + \varepsilon_{j,m}$$

So Table 4 relies on the balanced sample only, including only sellers who were interviewed at both base and endline. Table 4 - Column (1) presents the full sample, Column (2) the control market data, Column (3) the treatment market data. To check for baseline balance, we first conduct a t-test with unequal variance for the following baseline characteristics for the balanced sample: ownership characteristics, characteristics of the shop, sales amounts of fertilizers (DAP, CAN, NPK, urea, SA, and Minjingu) stock of fertilizers at the time of the interview (DAP, CAN, NPK, urea, SA, and Minjingu), number of customers at the time of the interview, number of years the shop has been open, and the number of weeks open/year, as well as locational characteristics. The result of this test is reported in Column (4).

Overall, sellers have a good average level of education and are mostly male. Few shops have the two required government certificates for selling fertilizer. Not only is it the case that shops enter and exit markets at a high rate, but most stores are only open during a few months of the year, and this is particular the case for shops in the treatment group (though the difference is not statistically significant). Average years selling fertilizer is between 4 and 4.5, an indication of the market churning. Conditional on the shops being open, not all fertilizers are stocked and sold. Our baseline interviews which took place in December, January and February -- the months just before the long rains planting – and almost half of the stores did not have any urea in stock.

Table 4: Descriptive statistics of agro-dealers at baseline

	Full	Control	Treatment	p-value
Sex owner (1 = female; 0 = male)	0.26	0.26 (0.05)	0.25 (0.03)	0.85
Age owner	42.72	43 (1.32)	42 (1.05)	0.57
Education owner*	2.20	2.53 (0.18)	2.12 (0.15)	0.33
TFRA license (1 = yes ; 0 = no)	0.49	0.46 (0.06)	0.51 (0.04)	0.44
CNFRA tagmark member (1 = yes ; 0 = no)	0.37	0.36 (0.05)	0.38 (0.04)	0.80
Asset index**	2.82	2.8 (1.56)	2.83 (0.11)	0.86
Years selling fertilizer	4.17	4.5 (4.96)	3.9 (4.33)	0.37
Selling fertilizer every month (1 = yes ; 0 = no)	0.60	0.43 (0.50)	0.28 (0.48)	0.36
Number of customers present during baseline interview	2,78	2.4 (3.18)	3 (4.09)	0.22

Notes: Table 4 presents the results of a baseline balance test. Column (1) presents the average and standard deviation of the full sample. Column (2) of the control markets, Column (3) of the treatment markets. Column (4) presents the results of a t-test with unequal variances testing the differences between the treatment and control groups. The sample contains all agro-dealers who were present at both baseline in person interview, and endline call interview. N = 231 (control = 82 ; treatment = 149).
*The education variable is coded as follows (0 = primary; 1 = secondary ; 2 = trade school ; 3 = diploma ; 4 = BA and related ; 5 = Ms and related ; 6 = PhD). **The asset index is the sum of ownership of the following assets: mobile phone, smart phone, computer, pickup truck, motor bike, car and generator.

Looking back over the 2019 calendar year, most agro-dealers sold urea (about 70%). Significantly fewer sold the other fertilizers. NPK stands at 40%, SA at about 30%, DAP and CAN at 50% and Minjingu at a mere 1%. The average total amount of fertilizer sold is about 20 ton in 2019, of which about half represent sales in urea.

Overall, the sample of agro-dealers is well balanced. While there is an imbalance in the number of fertilizer sellers in treatment and control markets in the balanced sample, partially due to a larger number of dealers in the treatment markets (see Table 1), the randomization was successful in balancing the two groups of sellers across these observable characteristics.

Table 4: Descriptive statistics of agro-dealers at baseline (cont.)

	Full	Control	Treatment	p-value
Sales				
Sell urea in 2019 (1 = yes ; 0 = no)	0.67	0.65 (0.47)	0.69 (0.46)	0.61
Quantity urea sold in 2019 (kg)	11,537	1,2050 (2,750)	1,1265 (2,6421)	0.86
Sell NPK in 2019 (1 = yes ; 0 = no)	0.41	0.39 (0.49)	0.42 (0.49)	0.65
Quantity NPK sold in 2019 (kg)	5,461	4,748 (8,558)	5,794 (12,828)	0.64
Sell DAP in 2019 (1 = yes ; 0 = no)	0.51	0.50 (0.50)	0.52 (0.50)	0.73
Quantity DAP sold in 2019 (kg)	5,287	4,907 (10,255)	5,487 (15,793)	0.81
Sell Minjingu (1 = yes ; 0 = no)	0.05	0.01 (0.11)	0.07 (0.26)	0.01
Quantity Minjingu sold in 2019 (kg)	3,937	NA	NA	NA
Sell CAN (1 = yes ; 0 = no)	0.60	0.59 (0.49)	0.61 (0.48)	0.84
Quantity CAN sold in 2019 (kg)	4,773	2,626 (4,736)	5,933 (15,506)	0.06
Sell SA (1 = yes ; 0 = no)	0.40	0.33 (0.47)	0.44 (0.49)	0.09
Quantity SA sold in 2019 (kg)	3,091	1,544 (4,967)	3,705 (11,258)	0.21
Total amount of fertilizer sold in 2019 (kg)	26,278	21,618 (43,430)	28,744 (75,794)	0.45
Stocks and capacity				
Urea in stock at time of the interview (1 = yes ; 0 = no)	0.48	0.48 (0.50)	0.47 (0.50)	0.87
Total current stock of fertilizer (kg)	664	476 (938)	558 (2,611)	0.31
Total current capacity of fertilizer (kg)	17,082	16,301 (45,923)	17,493 (67,635)	0.89

Notes: Table 4 presents the results of a baseline balance test. Column (1) presents the average and standard deviation of the full sample. Column (2) of the control markets, Column (3) of the treatment markets. Column (4) presents the results of a t-test with unequal variances testing the differences between the treatment and control groups. The sample contains all agro-dealers who were present at both baseline in person interview, and endline call interview. N = 231 (control = 82 ; treatment = 149). The sales amounts of the individual fertilizer types are conditional on any sales.

6. Analysis and results

6.1 Farmer analysis

We start with the farmer-level analysis, and with our main variable of interest, the beliefs regarding the quality of fertilizer. Given the nature of our intervention, this is the main mechanism of a possible change in fertilizer use.

Information collected in our endline in person survey documents the importance, and strength, of our village information treatment. First, almost everyone in the treatment villages whom we interviewed at endline in person had attended our informational meeting, and reported having found the information both credible and useful. As per farmers' account, the information received during the village meeting was shared, but mostly within the village. We also note that most farmers do not get information from print media, TV/Radio or the internet. The bulk of the information about agricultural matters still comes from in person discussions with other farmers. This confirms what we had learned from the baseline data collection regarding how the beliefs regarding fertiliser quality are formed and updated: through own experiences and experiences of one's immediate peers.

We defined three measures of beliefs based on the question: "If 10 farmers, like you, purchase 1 kg of fertilizer at MARKET X this week, how many farmers would purchase a good quality bag and how many farmers would purchase a bad quality bag?" As we asked this question across three markets, we have three answers for this question. We consider the number of farmers who purchase good quality bags, for easy interpretation of the coefficient, and compute the average, minimum and maximum for each farmer across the three markets.

Our basic empirical specification is a difference-in-difference specification, making use of the panel data of the in-person baseline survey with the endline phone survey. We use subscript *i* to refer to the farmer. We also introduce subscript *v* which refers to village. We use time subscript *t*. Standard errors are clustered at the village level.

$$beliefs_{i,v,t} = \beta_0 + \beta_1(INFO_v * AFTER_t) + \beta_2 INFO_v + \beta_3 AFTER_t + \gamma X_{i,v} + \varepsilon_{i,v,t} \quad (1)$$

Where INFO refers to whether or not the village was selected as an information treatment village, and AFTER refers to the base/endline data. Control variables $X_{i,v}$ include baseline measures of sex, age, education, land owned, farm experience, number of household members, and number of markets visited. We test the hypothesis $\beta_1 = 0$. Table 5 presents the results of specification (1).

We recognise that while spill-over between villages is likely limited due to geographic isolation, villagers in the control group might have been affected by the market treatment in nearby villages. We return to this matter later, and for now, emphasize that the effects estimated the next set of tables should be viewed as a lower bound.

Table 5: The effects of the village treatment on the fertiliser beliefs of farmers

	(1) Average belief (out of ten)	(2) Minimum belief (out of ten)	(3) Maximum belief (out of ten)
Village treatment * After	0.522**	0.419*	0.591*
	(0.255)	(0.247)	(0.301)
After	0.531***	0.328*	0.717***
	(0.176)	(0.178)	(0.201)
Village treatment	-0.00453	-0.0456	0.103
	(0.180)	(0.172)	(0.214)
Constant	5.822***	7.112***	4.676***
	(0.610)	(0.573)	(0.710)
Baseline controls	Yes	Yes	Yes
N	1,907	1,907	1,907

Notes: Table 5 presents the results of an OLS regression following the difference-in-difference specification (1) of the average, minimum and maximum belief of farmers using a difference in difference specification with baseline control variables. Control variables include baseline measures of sex, age, education, land owned, farm experience, number of household members, and number of markets visited. Errors are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1

The effects reported in Table 5 are statistically significant, and strong across the board. The village treatment increases the average fertilizer beliefs by 0.5, which represents an effect size of about 7 percent. Both minimum and maximum have increased as well.

While this table 5 presents average effects, there are two dimensions of heterogeneity that are of interest to us: between farmers within villages; and between markets within farmers. Indeed, our discussion in Section (4) highlighted that there are farmers with no concerns at all at baseline, and farmers with strong concerns.

To address the later type of heterogeneity, we can further exploit the panel of beliefs data, which, as we mentioned before, we had obtained at the market level at baseline and during the phone interviews at endline.[14] Recall that in the village intervention, information was provided on the nearest treatment market. However, this was not always adhered to correctly, and in our analysis below, we use the actual differential information provision to assess differential beliefs updating, by market, within farmer.[15] Figure 8 presents the difference increase in beliefs for the treatment villages, depending on whether the farmer reported having received information on the respective market in the village treatment or not.

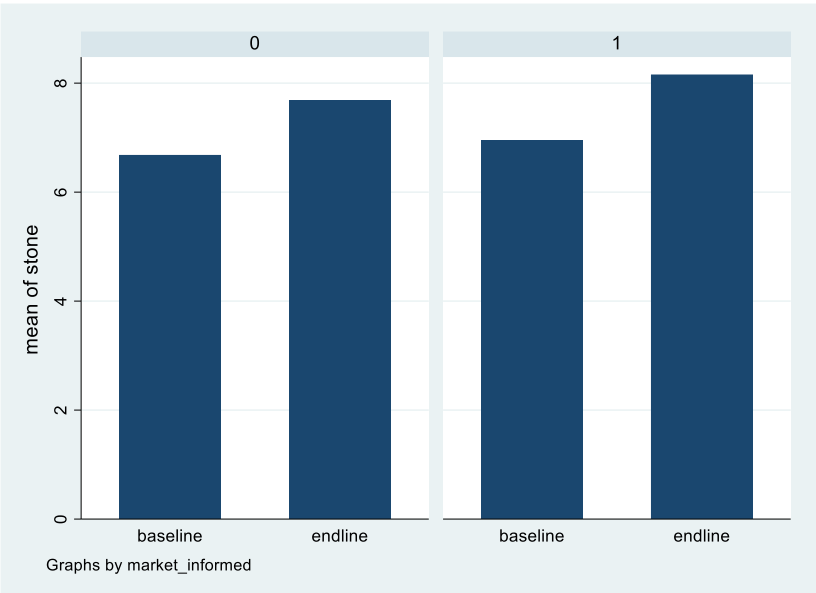

Figure 8: The average number of good bags (out of ten) as believed by the farmers in the treatment group by whether they received information about the market

From Figure 8 we note that the beliefs go up in both cases, but slightly more so in the case of the market for which the farmer received information. Note that this effect should not be interpreted as causal. The market they were receiving information about was not randomly selected, but

[14] Note that this is not a balanced panel, in the sense that not all markets were asked about in both rounds (even though it should have been).

[15] Using information from the in-person endline survey, we can get an estimate of the degree to which the enumerators deviated from the script during the village intervention meetings. Recall that in the treatment villages, they should have received info on one market only, the nearest treatment market. In reality, each farmer received info on, on average, 1.38 markets (instead of 1), and only 50% got exactly info on just one. Furthermore, only for 35% of the sample was the market that should have been included in this set. So it seems that enumerators gave info on more markets than they should. However, about 80% of markets that information was provided on, were treatment marketing, limiting concerns of spill-overs between treatment and control groups.

identified as the nearest treatment market. In addition, in not all villages information was given on this exact market, and in many villages, the enumerators ended up providing information on one than more treatment markets, possibly at the insistence of the farmers.

We proceed with the impact of the village treatment program on the use of fertiliser using an amended, but similar specification to specification (1):

$$use_{i,v,t} = \beta_0 + \beta_1(INFO_v * AFTER_t) + \beta_2 INFO_v + \beta_3 AFTER_t + \gamma X_{i,v} + \varepsilon_{i,v,t} \qquad (2)$$

Note that we have two measures for fertilizer use (for each fertilizer type): A binary measure as to whether or not the farmer purchased fertilizer in the long-rains growing season, and a continuous measure, in kg.

To consider the effect on fertilizer prices, recall that we do not have baseline information. Hence, we use the endline data to estimate:

$$price_{i,v} = \beta_0 + \beta_1 INFO_v + \gamma X_{i,v} + \varepsilon_{i,v} \qquad (3)$$

In both specifications (2) and (3), we test the hypothesis: $\beta_1 = 0$. We also use specification (3) to consider a measure of fertiliser density which we only have at endline: kg of fertiliser use per cultivated acre.

Table 6: Effect of the village treatment on fertiliser use (binary measure)

	(1) Urea	(2) DAP	(3) CAN	(4) Minjingu	(5) NPK	(6) SA
Village treatment * After	0.116***	0.0193	0.220	0.00856	0.00437	-0.0164*
	(0.0338)	(0.0268)	(0.183)	(0.00954)	(0.00913)	(0.00965)
After	-0.0545**	-0.0419**	-0.443***	-0.0168**	-0.0126*	-0.00210
	(0.0262)	(0.0182)	(0.129)	(0.00762)	(0.00641)	(0.00558)
Village treatment	0.0288	0.00386	0.248	-0.0109	-0.000635	0.00798
	(0.0482)	(0.0279)	(0.188)	(0.00913)	(0.00965)	(0.0139)
Baseline controls	Yes	Yes	Yes	Yes	Yes	Yes
Constant	-0.0792	-0.0277	-1.756***	-0.0301	0.0147	0.0438
	(0.0997)	(0.0561)	(0.428)	(0.0218)	(0.0147)	(0.0501)
N	1,928	1,928	1,922	1,928	1,928	1,928

Notes: Table 6 presents the results of an OLS regression of the binary fertilizer use variable of interest using a difference-in-difference specification (2) with baseline control variables. Control variables include baseline measures of sex, age, education, land owned, farm experience, number of household members, and number of markets visited. Errors are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1.

In Table 6 we find that the village treatment increases the (binary) use of urea fertiliser. After the village treatment, farmers are 11 percentage points more likely to use urea. From an average baseline use of 40% among the analysis sample, this is a significant effect. We note a negative significant result on the fertiliser SA. However, with a baseline use of less than 3% we believe this effect is not as important in magnitude (even though some switching between fertilisers might have occurred).

In Table 7, we present the results of the same specification (2), but this time using the continuous measures, where those farmers who are not using are set a value of zero. We still have a sizable coefficient, in the order of a 20% effect size. But this is no longer statistically significant, with a P value of around 0.25.

Table 7: Effect of the village treatment on fertilizer use (continuous measure)

	(1) Urea (kg)	(2) DAP (kg)	(3) CAN (kg)	(4) Minjingu (kg)	(5) NPK (kg)	(6) SA (kg)
Village treatment * After	8.949	0.845	-0.899	0.573	-1.040	-1.271
	(8.348)	(4.625)	(2.511)	(0.759)	(1.752)	(1.112)
After	-9.384	-2.147	-1.237**	-1.306**	-0.836*	-0.107
	(5.820)	(3.841)	(0.594)	(0.629)	(0.448)	(0.324)
Village treatment	3.171	-1.088	3.183	-0.640	1.276	0.727
	(11.30)	(3.125)	(2.334)	(0.714)	(1.520)	(1.226)
Baseline controls	Yes	Yes	Yes	Yes	Yes	Yes
Constant	-63.23***	-5.949	-2.059	-0.777	-0.902	0.900
	(19.64)	(7.726)	(4.156)	(1.390)	(2.057)	(2.341)
N	1,928	1,928	1,928	1,928	1,928	1,928

Notes: Table 7 presents the results of an OLS regression of the continuous fertilizer use variable of interest using a difference-in-difference specification (2) with baseline control variables. Control variables include baseline measures of sex, age, education, land owned, farm experience, number of household members, and number of markets visited. Errors are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1

It is notable that the village treatment, which provided information on urea fertiliser only, did not appear to spill over onto fertilizer use of the other fertilisers. There are several possible reasons why this could be the case. First, as we noted in Section (4), the other fertilisers are less widely used, and hence any effect would be harder to detect. Second, perhaps the beliefs regarding the other fertilisers were not as pessimistic at baseline. While we cannot verify this, as we only collected beliefs information for urea, the bulk of the narrative in the media on fertiliser centres around urea. As farmers might suspect agro-dealers from altering the product, it would be odd if this altering only happened for one type of fertiliser, unless farmers believe that it's easier or more profitable to do for only that type. Knowing the underlying reason would allow us to figure out whether and in what circumstances these beliefs and changes in beliefs are likely to 'spill-over' from one type of fertiliser to another type.

Table 8 presents the results of specification (3). Columns (1) through (3) present the prices farmers report paying per kg at endline. Note that we do not list all the fertilizers. This is because these data are only known conditional on having purchased fertilizer (one can observe the drop off in observations when moving from urea to the other fertilizers). We note no effect on the prices paid. Figure 9 presents the distribution of the urea price (TS/kg) across treatment and control villages and confirms this lack of effects. Note that in this case, unlike in the market treatment, there are two reasons why we might not expect an effect of the treatment. The first reason being is that a change in prices would imply a change in the equilibrium at the local market. As we are looking at village treatments, and a small number of villages compared to the universe of villages in this region, we would not necessarily expect that an increase in demand pushes the price up. Secondly, as we noted earlier in the market treatment discussion, prices are regulated by the government.

Column (4) in Table 8 presents the effect on fertilizer per acre conditional on use (including those who do not use, set at zero), and notes an effect of almost 6, which corresponds to an effect size of 54%. As about 40% of baseline farmers who used fertilizer at baseline already use it on all their land, this effect is likely driven by the ones who were not using at baseline.

Table 8: Effect of the village treatment on prices (TS/kg) and fertilizer use (kg/acre)

	(1) Price Urea (TS/kg)	(2) Price DAP (TS/kg)	(3) Price CAN (TS/kg)	(4) Fertiliser kg/acre
Village treatment	-7.839	-61.32	-132.5	5.579**
	(50.15)	(111.1)	(130.6)	(2.510)
Baseline controls	Yes	Yes	Yes	Yes
Constant	1,617***	2,301***	2,126***	-9.044
	(112.0)	(571.5)	(377.7)	(5.620)
N	364	90	37	959

Notes: Table 8 presents the results of an OLS regression using specification (3) of the price of fertilizer and the fertilizer use on the village treatment with baseline control variables. Errors are clustered at the village level. Control variables include baseline measures of sex, age, education, land owned, farm experience, number of household members, and number of markets visited. *** p<0.01, ** p<0.05, * p<0.1

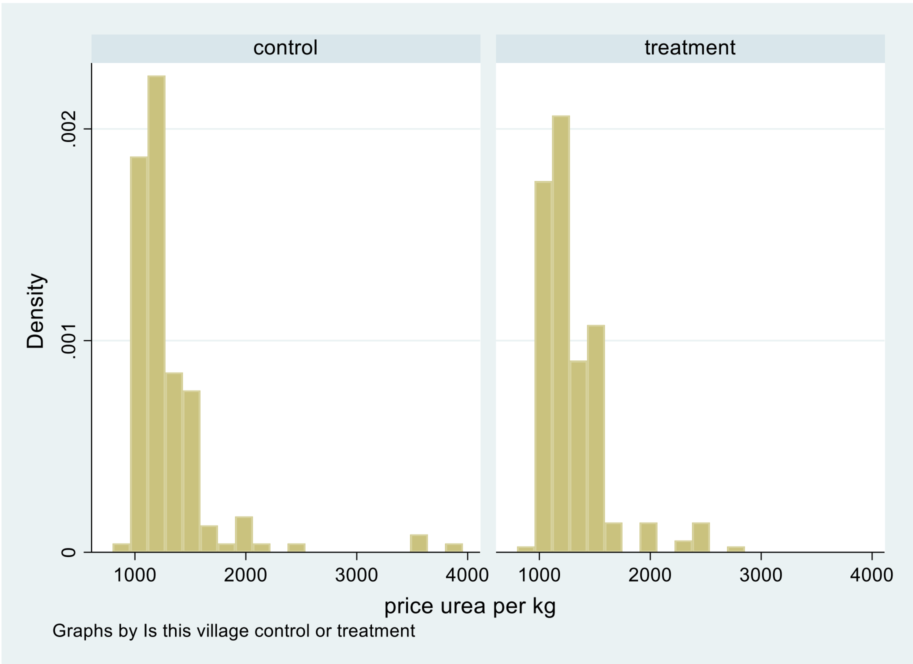

Figure 9: Distribution of the price of urea at endline (TS/kg)

In Table 9 we provide more detail as to these changes in input use and beliefs using the endline in-person survey.[16] As many of these variables were not elicited at baseline, we use a specification akin to specification (3), again, on the balanced panel only. Columns (1) refers to maize acreage, Columns (2) and (3) refer to input use of agro-chemicals and hybrid seeds. Columns (4) and (5)

[16] However, with a smaller sample, both balance and attrition become more of an issue. In terms of balance, risk aversion now appears to be comparable across treatment and control villages. But, importantly, the control villages within this sample start off with significantly worse beliefs in terms of fertiliser quality; and this being the main channel of treatment effect, might imply that, to some extent, we might be under-estimating the village treatment effect. Finally, attrition is again significant, at 27% despite the fact that we visited the villages in person. This is partially due to the fact that one village was not reached at all. Attrition appears not correlated with the village treatment status. [We cannot do panel data for these data though as we did not collect these data in panel. I did add these baseline beliefs now as an additional control in the impact analysis in this sample, but we'll have to reason through this as to whether this is enough.]

present effects on plans to use urea in the following long-rain season. Column (6) present yield effects, and Columns (7) and (8) refer to the yield beliefs regarding maize. Recall that these were elicited using a visual method feature a density distribution with three possible categories, and blocks representing each 10%.

Table 9: The impact of the village treatment on farmer's behaviour and outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Maize acres in previous season	Use of agro-chemicals on maize in previous season (1=yes; 0=no)	Use of hybrid maize variety in previous season (1=yes; 0=no)	Planning to use urea next season (1=yes ; 0 = no)	How much urea do you plan to use next season (kg)	Harvest maize in kg previous season	Yield beliefs maize (kg) - average	Yield beliefs maize (kg) - variance
Village treatment	-0.100	-0.0667	0.187*	0.0267	-13.22	4.020	88.45	114.8
	(0.327)	(0.0907)	(0.108)	(0.0747)	(26.93)	(180.1)	(178.3)	(259.1)
Baseline controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	0.821	0.580**	0.0795	0.597***	56.75	562.3	656.8	929.5
	(0.885)	(0.231)	(0.282)	(0.207)	(38.66)	(418.9)	(471.5)	(757.8)
N	207	177	177	207	207	165	193	197

Notes: Table 9 presents the results of an OLS regression of various outcome variables using specification (3) on the village treatment with baseline control variables. Control variables include baseline measures of sex, age, education, land owned, farm experience, number of household members, and number of markets visited. Note that the baseline controls this time also include the baseline beliefs due to the baseline belief imbalance at baseline within this analysis sample. Errors are clustered at the village level. Sample only includes the balanced sample between baseline line and endline in person survey. *** p<0.01, ** p<0.05, * p<0.1

Table 9 suggest an interesting set of patterns. Farmers in treatment villages were more likely to use hybrid maize, but not more likely to have expanded maize acreage or use more agro-chemicals in the previous season (the 2019 year) (Columns 1 through 3). This is perhaps expected, it is well known that there are production complementarities between hybrid seeds and the use of fertilizers.[17]

Considering Columns (4) and (5), we would caution against the interpretation of the variable 'do you plan to use urea' (which refers to the following season, 2020). We noted that this variable was suspiciously high across the board, and even across fertilizers, in this survey, which makes us suspect we are not captured actual plans here, but perhaps rather, the farmers aiming to please the

[17] It should be noted that when we split the sample by urea use at endline, we see a negative effect of the treatment for those farmers who use urea at endline. While at first sight, this might suggest some degree of financial competition, are less funds available for agro-chemicals, perhaps? However, closer inspection reveals that this is not a causal effect, but rather driven by sample selection. Instead of the uptake of fertiliser leaving less funds for other chemicals, there is a particular type of people, people who don't usually use external inputs who now give fertiliser a try, but this does not spill over to other inputs, so it does not cross their mind, or they decide to not adopt any complementary inputs. So what we have a correlation, not a causal relationship with the treatment.

enumerators. Although, there is another interpretation as well, for which we have some evidence. It is possible that by the time of the endline survey even the control villages were treated indirectly by witnessing posters at markets. When we cross tabulate whether or not they plan to purchase urea it is notable that almost everyone in the control villages who has seen the posters plans to purchase urea despite not having had the village treatment. Similarly, the overwhelming majority of those who do not plan to purchase urea within the control villages have not seen the pamphlets.

We note null effects when it comes to yields regarding the 2019 season and yield expectations. Barring the small sample, there are three other factors that might be driving the lack of effects. First, the effects on yield may take time, and one might not see them, to the extent that we will pick them up, the first year one adopts fertilizer, especially if one does not quite use fertilizer the correct way yet. Second, the beliefs and yields were elicited in total, not per acre, so changes in the acreages (which we do not see in Column 1 though) could complicate the interpretation of this null effect. Third, and most importantly, we focus our analysis of yields and yields beliefs on maize only, as this is the most important food crop. However, we know from the baseline data that fertilizer is being used on all crops.

We conclude with a discussion of market behavior. Recall that we collected data as to which markets farmers visited at baseline, and from which markets farmer purchased fertilizer at both baseline and the phone endline survey. In addition, from the in-person endline survey, we know which markets they visited and whether they spotted the posters and pamphlets. We use this information to test how farmers altered their shopping behaviour based on treatment, perhaps moving from untreated to treatment markets, or buying more only if their local market is a treatment market, or if they received information about a local market.

Table 10 presents the effect of the village treatment on this farmer market behaviour.[18] The results in Column (1) indicate that the village treatment increases the number of markets where a farmer purchases fertilizer, and Column (3) indicates that the treatment increases the likelihood one buys at the local, i.e. associated, market. However, this effect is almost entirely driven by farmers who did not purchase fertiliser before, and essentially are going from zero to one. Conditional on buying (as in Column 2 below), the village treatment effect is even negatively correlated with purchasing behaviour.

[18] There are two limitations to this analysis. First, during the phone endline survey, the enumerators mistakenly only asked for one possible market per fertiliser type, so the number of markets purchased from could be an underestimate at endline, which might imply an underestimate of the effects on Table 9. Another limitation is that farmers were allowed to mention markets by name. While our census of markets was expected to limit the cases where we had no market ID, in practise there was no unsubstantial number of markets where we could not assign any ID. These markets are dropped in the analysis as well. [Quantify this further]

Table 10: The effect of the village treatment on farmers' market behaviour

	(1)	(2)	(3)	(4)
	Number markets bought	Buy urea at a treatment market (yes/no)	Buy at local market (yes/no)	Switch markets (yes/no)
Village treatment * After	0.105**	-0.505**	0.368*	-0.279
	(0.0459)	(0.226)	(0.198)	(0.210)
After	-0.128***	0.393***	-0.521***	
	(0.0386)	(0.151)	(0.159)	
Village treatment	0.0129	1.502***	-0.0432	
	(0.0470)	(0.242)	(0.217)	-0.279
Baseline controls	Yes	Yes	Yes	Yes
Constant	0.984***	0.841	-1.317	-3.662***
	(0.131)	(0.883)	(1.046)	(0.682)
N	1,928	598	783	205

Notes: Table 9 presents the results of an OLS regression on the market behavior of farmers using difference-in-difference specification (2) for Columns (1) through (3), and specification (3) for Column (4), with baseline control variables. Control variables include baseline measures of sex, age, education, land owned, farm experience, number of household members, and number of markets visited. Note that the baseline controls this time also include the baseline beliefs due to the baseline belief imbalance at baseline within this analysis sample. Errors are clustered at the village level. Sample only includes the balanced sample between baseline line and endline in person survey. *** p<0.01, ** p<0.05, * p<0.1

6.2. Agro-dealer analysis

A market-level response can consist of two elements: A response in quantities and a response in prices. We start with the former, and then turn to the latter.

Recall that our analysis sample consists of all sellers present in both base and endline survey. Denote the agro-dealer by subscript j, market by m, and time by t. To test for the impacts on sales we run the following difference-in-difference specification:

$$sales_{j,m,t} = \beta_0 + \beta_1(INFO_m * AFTER_t) + \beta_2 INFO_m + \beta_3 AFTER_t + \gamma X_j + \varepsilon_{j,mt} \quad (4)$$

Where the variable sales represents a range of binary and continuous measures of sales including whether the seller sold urea, had urea in stock at the time of the survey, the amount of urea sold and the total amount of all fertilizer sold in a given year. T_m refers to the market treatment and = 1 if the market was treated, and = 0 if the market was not treated. The variable AFTER refers to the data collection round and = 1 if this was the endline, collected in 2020 but referring to the 2019 year, or = 0 if this was the baseline, collected in 2019 but referring to the 2018 year. We test the hypotheses whether $\beta_1 = 0$. The control variables X_j include the gender, age and education level of the owner, the number of years the business has been selling fertiliser, and has been on that location, the total capacity of the business, the asset index for both owned and rented in assets, and whether or not the business has an TFRA license and is an CNFA member.

Table 11 presents the results of specification (4). We detect no effects of the treatment on the extensive margin: either the likelihood of having urea currently in stock or selling urea in any given year. This results extends to other fertilizers as well (not reported). It seems that there might be an effect on the amount of urea sold. The coefficient of 3,853 represents an about 30% effect size, but is not statistically significant.

Table 11: Effect of the market treatment on agro-dealers' sales

	(1) Sold Urea (1 = yes ; 0 = no)	(2) Urea currently in stock (1=yes ; 0 = no)	(3) Quantity sold urea (kg/year)	(4) Total fertilizer sold (kg/year)
Market treatment * After	-0.130	-0.131	3,853	12,2218
	(0.264)	(0.255)	(3,062)	(2,3078)
Market treatment	0.0910	-0.0283	-1,247	7,125
	(0.179)	(0.173)	(2,462)	(9538)
After	0.407*	-0.587***	-2,338	7,514
	(0.212)	(0.203)	(2,513)	(1,629)
Constant	0.408***	-0.0306	7,774***	2,1618***
	(0.143)	(0.139)	(2,160)	(5,885)
Baseline controls	Yes	Yes	Yes	Yes
N	464	464	299	388

Notes: Table 11 presents the results of regression mapping sales of urea and total sales into the treatment variable using a difference-in-difference specification (4). The sample consists of the balanced panel only. Column (1) and (2) present the coefficients of a Probit regression, while Columns (3) and (4) present the coefficients of an OLS regression. Column (3) is conditional on non-zero sales of urea, and excludes outliers > 95%. Control variables include…….. Robust standard errors are reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.1.

One of the reasons of these lack of effects, barring any issues with attrition, might be that urea sales does not represent a normal distribution, but tends to have a long right-tail. In Figure 10 we plot the kernel density of the quantity of urea sold at endline, this time keeping the zero observations, but capping at 50,000 kg/year. One can note that the treatment group distribution has a significantly longer tail than the control group, indicative of possible effects among a sub-set of agro-dealers only.

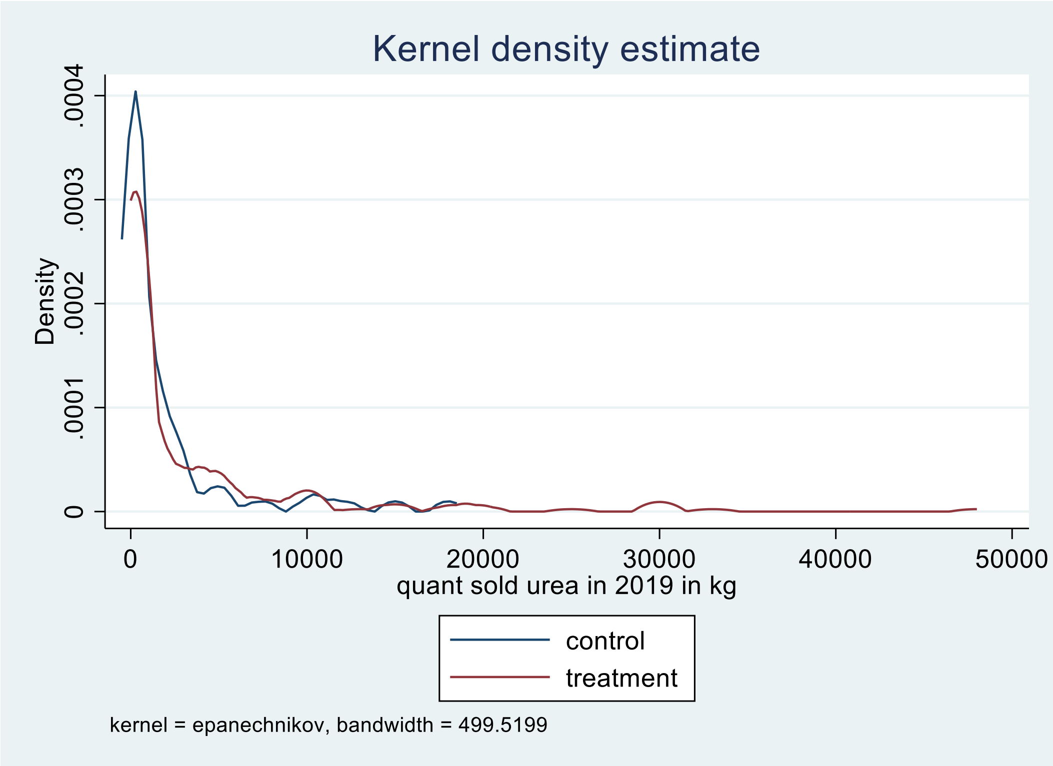

Figure 10: Kernel density amount of urea sold kg/year by market treatment status at endline, agro-dealer data

Recall that we do not have price data at baseline, but do have price data at endline: We asked all agro-dealers to report on current fertilizer prices, in 50 kg (bag), whether or not any fertilizer were sold that week. Hence we drop the t subscript in regression specification (5). We continue to use the same balanced sample:

$$price_{j,m} = \beta_0 + \beta_1 T_m + \gamma X_{j,m} + \varepsilon_{j,m} \quad (5)$$

Prices were asked in terms of bags of 50 kg at endline, referring, for ease of reference for the agro-dealer, at the time of the interview in January-February 2020. Using these prices, one can also construct a measure of revenues, defined as the multiplication of these current prices with fertilizer sold in the previous year, which is 2019 at endline. In addition, one can use the same regression specification (5) to consider the effects on another variable which was only available at endline, the elicited WTP for fertilizer information.

Table 12 presents the results, again using the balanced panel only. In Column (1), we note a significant effect on the WTP to pay for information. One should keep in mind that a sub-set of agro-dealers noted they would not be willing to pay anything for such a scheme. These sellers indicated that they were not the decision-maker or that it's the government who should guarantee the quality of fertilisers. Despite the inclusion of these zero offers, we note a sizable effect, 18,311 TZS, which represents an effect size of 30 percent (although the p-value is 0.2, and hence not statistically significant). It is notable that even among the control group, those who were not exposed to the market treatment, the WTP is substantial, with an average WTP of about 20 USD for the sample as whole.

In Table 12 - Column (2) we present the effect on the ago-dealers' revenues. We note no statistical significant effects. The lack of power likely plays a role here, as our qualitative surveys did indicate effects among at least a sub-set of the agro-dealers in this realm. In Column (3), we note no statistical significant impact on the price of urea sold. While this estimate also suffers from issues related to power, it is consistent with what we noted in the farmer-level analysis.

Table 12: Effect of the market treatment on prices and revenues of agro-dealers

	(1) WTP for information	(3) Revenue (TZS)	(2) Urea price sold (TZS/50 kg)
Market treatment	18,311	6.998e+06	105.9
	(13,973)	(8.189e+06)	(1,061)
Constant	32,521***	1.147e+07*	57,835***
	(7,950)	(6.132e+06)	(863.4)
Baseline controls	Yes	Yes	Yes
N	364	165	216

Notes: Table 12 presents the results of an OLS regression mapping WTP for information, revenues and the price of urea into the market treatment variable using specification (5) with control variables. Control variables include…..The sample consists of the balanced panel only. Robust standard errors are reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.1.

In effect, considering the price series elicited at endline, we note an Intra-Cluster Coefficient of 0.46 for the price of urea, implying that the majority of the variation is at the market-level. This is consistent with what we heard during our qualitative interviews with the government officials. The government sets maximum prices for the main fertilizers, among others urea, on a location-week specific basis. Given that we interviewed all agro-dealers within a short span of time, and they reside close to each other in the market, we would expect little variation across them, especially as this maximum price appears to be binding.

We have one other measure of prices. We elicited details on sales also during our weekly agro-dealer survey. The strength of these weekly data lies in the frequency, and possibly accuracy, as this time, we elicited the prices per kg rather than per 50 kg. This more accurately reflects the amounts the farmers purchase but might introduce another source of measurement error related to the ban on sales of small bags which was introduced in the year of our study. In addition, there is significant attrition (only 70 dealers provided information for each week of this survey, and we see significant sample selection in this, in the sense that the dealers in the control group are, on average, selling more than the ones in the treatment group). However, taking this selection into account, we do note a more stable pattern in prices in the treatment markets compared to the control markets (see Figure 11. It should be noted that the average price difference between the two samples, with the prices in the treatment markets being about 5 % higher than the one in the control markets, is not statistically significant.

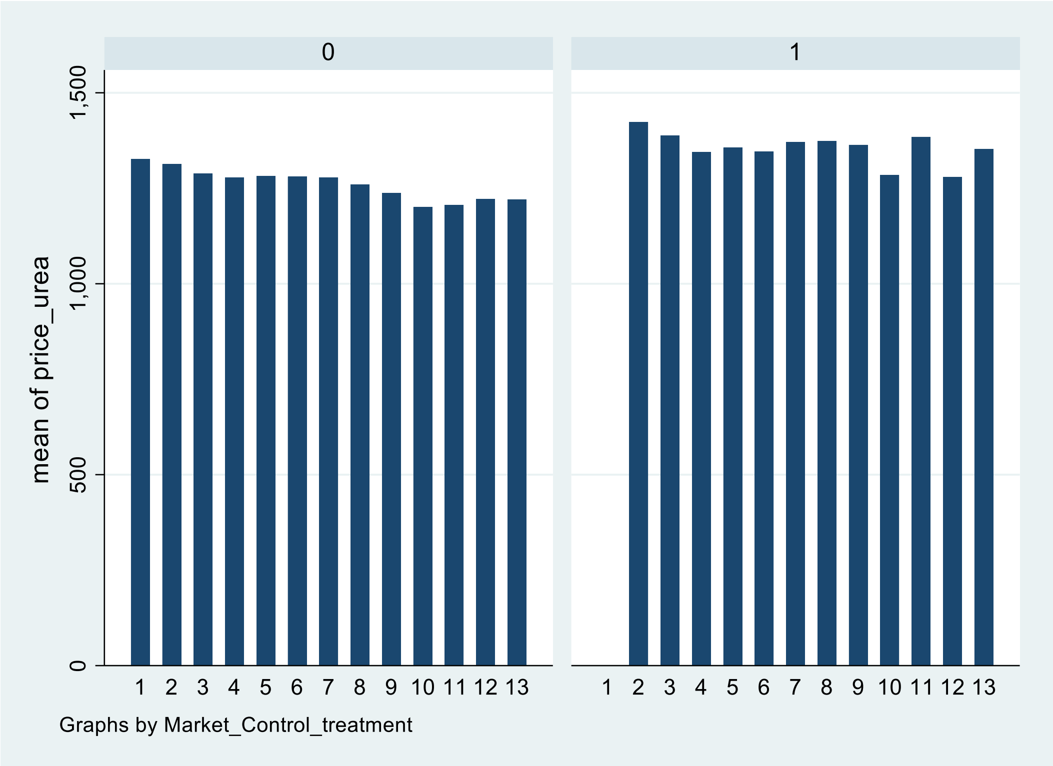

Figure 11: Average price for urea per kg in the control and treatment markets over time since the implementation of the program

We can further investigate these weekly trends using the following regression specification (6), where *t* now refers to the week number, and I represents the various weekly indicators elicited.

$$I_{j,t,m} = \beta_0 + \beta_1 T_m + \beta_2 t + \beta_3 t T_m + \varepsilon_{j,m,t} \qquad (6)$$

Table 13 presents the results. While we note no statistically significant correlations when it comes to the urea prices, we note a positive, and statistically significant, correlation with the various sales variables, including the number of customers purchasing fertilizers, the overall fertilizer sales that week, and the fertilizers sales of urea, DAP and NPK. Note also the negative coefficient on the Market Treatment variable, indicative of the sample selection issue mentioned earlier.

Table 13: Correlation between market treatment, week, and sales indicators of the agro-dealers

	(1) Urea price (TS/kg)	(2) NPK price (TS/kg)	(3) DAP price (TS/kg)	(4) # Customers	(5) # Customers fertilizer	(6) Fert sold (per week, kg)	(7) Urea sold (per week, kg)	(8) DAP sold (per week, kg)	(9) NPK sold (per week, kg)
Market treatment	66.35	106.0	-17.53	-3.663	-19.73***	-75.37*	-60.56*	-37.34**	-18.91*
	(51.75)	(88.42)	(65.94)	(33.59)	(6.770)	(43.96)	(32.16)	(14.77)	(9.817)
Week	-9.634***	6.493	-2.410	6.306	-1.093**	1.900	0.676	-2.033	-2.309***
	(2.680)	(7.396)	(4.769)	(4.456)	(0.485)	(3.802)	(3.243)	(1.332)	(0.861)
Market treatment * Week	3.890	-2.667	13.86*	1.739	3.152***	12.51**	8.079*	3.960**	3.184***
	(6.509)	(10.42)	(8.328)	(6.186)	(1.186)	(6.158)	(4.413)	(1.971)	(1.091)
Constant	1,328***	1,426***	1,498***	106.3***	26.64***	170.9***	125.7***	69.03***	41.08***
	(21.21)	(65.75)	(36.85)	(24.46)	(5.000)	(28.96)	(23.37)	(9.692)	(7.733)
N	506	301	371	510	910	385	362	171	113

Notes: Table 13 presents the results of an OLS regression mapping measures elicited during the weekly survey into the market treatment and the week of elicitation. The sample consists of the agro-dealers whom responded to all weeks of the survey. Robust standard errors are reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.1.

7. Conclusion

Work by Michelson et al. (2021), Hoel et al. (2021), and Bold et al. (2017) establish widespread suspicions among farmers about urea fertilizer quality in Sub-Saharan Africa. On average across these studies, farmers expect that one out of every three bags of urea in their local market is of bad quality. Hoel et al. (2021) show in experimental settings that Tanzanian farmers are willing to pay less for urea of unverified quality, but that they respond and revise their willingness-to-pay in response to information that guarantees the good quality of the fertilizer.

What's puzzling about this widespread perception of uncertain fertilizer quality is that it does not seem to reflect the truth. Urea in the region is found to be consistently of good quality (Michelson et al. 2021; Hoel et al. 2021; Ashour et al. 2019; Sanabria et al. 2018a; Sanabria et al. 2018b; Sanabria et al. 2013). Critically, farmer beliefs have not converged to the truth. Hoel et al. (2021) develop a Bayesian learning model to show that misattribution and ambiguity make it difficult to learn true quality over time. They also establish evidence of spatial correlation in incorrect beliefs.

We implemented a randomized controlled trial in 100 markets in Morogoro Region, Tanzania, to test the effects of an information campaign about urea fertilizer quality. We distributed pamphlets and posters in randomly selected markets and villages, with the message that all urea tested in a study we ran in 2016 (Michelson et al. 2021) was found to be good. At baseline we found that approximately 20% of farmers believed all urea was of good quality. Among the rest, the average farmer believed about 30% of the bags for sale were of bad quality, with wide variation.

The information treatment significantly improved farmer beliefs about fertilizer quality six months after the intervention. Treatment also increased urea usage at the extensive margin, but not the intensive margin, suggesting that the primary effect of distrust in this community was to stop many farmers from using commercial fertilizer altogether. The intervention, which centred around urea, the most commonly used fertiliser, did not seem to spill over much to the other fertilisers. While farmers increased their use of urea, they did not increase their use of DAP, CAN, NPK, etc. Is this because there are no incorrect beliefs, or the fertiliser, even if good, is considered of less use, we don't know, or more perhaps more worryingly, the beliefs formation process may be fragmented/segmented across products.

At the agro-dealer level, we find a significant increase in the amount of urea fertilizer sold in treatment markets – almost a doubling of quantity sold in treated markets from 12,000 kg to more than 20,000. We find no effect on the price.

The COVID pandemic has spurred research on misinformation, and the role of the social media in its spread. We know from this research that incorrect beliefs can be motivated (in the sense of having value in and by themselves) and mixed up with issues of identify (as in Zimmerman 2020), and, perhaps as a result, display what has been termed `belief stickiness', meaning a situation in which beliefs do not respond adequately to new information presented (as in as in Falk and Zimmerman 2018). Beliefs can be so disassociated from reality that psychologists have used the terms `wishful/magical thinking' or `doom scenarios' (as in Engelmann et al. 2019; Risen 2016). These disassociated beliefs thrive in what has been termed the 'echo chambers' of social media, meaning social media groups which present a separate, disconnected, entity (see, among others, Chiou and Tucker 2018), and among communities who have been marginalised historically (Larson 2020). This implies that combatting misinformation is more likely to be successful when the population concerned is less marginalised, and the consequences of this incorrect information (from the point of view of the subject) can be substantial.

Through qualitative interviews in the area, we established that the incorrect beliefs of the Tanzanian farmers are not by any means motivated, or mixed up with their identity (barring the farmers' general distrusts of tradesmen). This is perhaps expected, as farmers in Tanzania represent the majority of the rural population, and are by no means marginalised, especially not politically. The decisions they make in terms of fertilizers can have substantial consequences, and affect yields and profits. Furthermore, farmers do not engage with social media, and while they do report reading the news and listening to the radio, the bulk of the information gathering happens from other farmers through in-person conversations, and through the government extension agents who are a regular presence in the community. Hence the scope for the so-called `echo chambers' is limited.

As such, we had reason to believe that farmers might not display belief stickiness when being confronted with credible information from our research team.[19] A key aspect of our intervention is that the information is provided by a trustworthy party to a group of individuals. We provide the information treatment to the whole village, the likely social network for each farmer. This avoids some of the pitfalls with social learning as disconnected individuals might not receive the information first-hand (as in Eyster and Rabin 2014, BenYishay and Mobarak 2019 and Maertens 2017).

It is interesting to think about why this information gap was there in the first place, where and how initial beliefs about bad fertilizer quality emerged. Without the ability to benchmark what a good fertiliser would look like and would yield, it can be difficult to estimate urea quality. The agronomic quality of fertilizer is determined by its nutrient content and cannot be directly observed, nor learned outside of a laboratory. Farmers in our sample rely on their own experience and the experience of other farmers to estimate fertilizer quality. If they have had a series of poor experiences, over-weighting them might result in the pessimistic beliefs we observe (Hoel et al. 2021).[20]

[19] De Brauw and Kramer (2018) finds significant response to their reputational interventions among input dealers in Bangladesh.
[20] Evidence for both overweighting and underweighting has been found; recent lab evidence seems to emphasize that individuals tend to overweigh good memories over bad memories in some cases (Soo Hong et al. 2020) but for farmers it can also be the other way around (as in Lybbert et al. 2007).

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