Making Markets: Experiments in Agricultural Input Market Formation

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Abstract

Making markets is central to theories of development. In a randomized controlled trial, we vary the characteristics of agricultural input markets to test whether time-inconsistent preferences, hard or soft commitments, and liquidity are constraints to market formation. The results show that markets organized earlier raise market sales and increase input adoption. Simply providing market access did not have any effect on demand, but liquidity in later spot markets increased demand to similar levels as markets organized earlier. We conclude that market timing is a substitute for liquidity in input market organization and that input demand is relatively inelastic to commitment levels.

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JEL Codes

Q12 Micro Analysis of Farm Firms, Farm Households, and Farm Input Markets
L10 General, Market Structure, Firm Strategy, and Market Performance
G21 Banks, Depository Institutions, Micro Finance Institutions, Mortgages

1 Introduction

Making markets is central to theories of economic development. For agricultural markets in low income countries, the technology adoption literature has extensively considered why farmers do not take-up seemingly profitable agricultural inputs or production technologies (Schultz (1964), Foster and Rosenzweig (2010), Magruder (2018), de Janvry and Sadoulet (2020), Suri and Udry (2022)) with significant attention to demand constraints. In much of this literature, a maintained assumption is that supply exists should farmers be willing to pay. Despite the idea that farmer constraints inhibit market formation, the supply side of the market is often also constrained. Input dealers lack information about existing demand in remote areas, face high transportation and coordination costs in reaching new markets, and are often liquidity constrained (Aggarwal et al. (2022), Asante et al. (2021), Macours (2019)). Markets fail to form due to demand and supply constraints, yet we have few empirical studies on market organization that create agricultural input markets relative to the extensive literature on demand side constraints [1] , government-led fertilizer subsidies, [2] [3]or market-based interventions where we observe behaviors of both demand and supply side actors (Magruder (2018),Dillon et al. (ming)).

Our experimental design addresses how agricultural input markets [4] form in rural Mali by making markets, specifically Village Input Fairs (VIFs). We exogenously vary the input market's organization to better understand which mechanisms inhibit agricultural input market formation, focusing on market timing, deposit levels in forward contracts as a form of market commitment, and liquidity in the form of credit to farmers. VIFs are an one day commercial events that bring ag-input dealers, financial institutions and farmers together to buy and sell agricultural inputs within a village. In our experimental design, VIFs are organized as spot markets during the planting season when farmers apply fertilizer, or during the post-harvest period when the previous agricultural season's production activities have finished and farmers may have more liquidity from harvest sales. In spot markets during the planting season, markets clear directly after the terms of the transaction are agreed. In post-harvest markets, a forward contract organizes the

[1]An exception is Aggarwal et al. (2024) discussed below.

[2]We specifically focus on agricultural input markets that supply fertilizer, seed or phytosanitary products. In the recent literature on agricultural output markets, a focus on intermediaries and farmer behavior is modeled to estimate welfare effects for farmers and/or price effects. See for example, Bergquist and Dinerstein (2020) or Casaburi and Reed (2022).

[3]Recent papers also focus on other agricultural input markets besides fertilizer, seed and phytosanitary products such as credit and insurance (Udry and Kolavalli (2019)), land rental (Acampora et al. (2025)), mechanization (Caunedo and Kala (2022)), irrigation (Jones et al. (2022)), or animal power (Brudevold-Newman et al. (2023)). See Michael Carter and Yang (2021) for estimates of a government-led subsidy program's impact on farmer yields.

[4]While agricultural production is characterized by multiple potential inputs including land, credit, water, and capital, we refer to fertilizer, seed and pytosanitary products as the agricultural inputs of interest for the rest of the paper.

market transaction, the terms of which are formalized in a purchase order. Post-harvest markets clear during the planting season when inputs are delivered to farmers in their villages and transaction balances are paid. In post-harvest treatment groups, we vary the deposit amounts of forward contracts between 10 or 50 percent of the purchase order. Deposit requirements are, in effect, commitment mechanisms, with a large body of literature focusing on either "hard" or "soft" commitment (Bryan et al. (2010)). The last variation in VIF organization are credit offers made through a partnership with a rural microfinance organization. Variations in market timing, deposit amounts and credit offers allow us to disentangle the effects of behavioural mechanisms, such as time-inconsistent preferences (Duflo et al. (2011)), commitment levels, and liquidity constraints on market demand. We estimate the relative effects of different market characteristics on farmer adoption and demand, crop and labor choices, production and marketed surplus to better understand how to make markets.

We summarize our results in four parts. First, we present compliance measures from a supply and demand perspective. In experiments such as Duflo et al. (2011) or Liverpool-Tasie et al. (2024), one agricultural input company implements the intervention. In this experiment, a consortium of ag-input dealers organized by the national input dealers association (UNRIA) organized the VIFs. Working with multiple ag-input dealers creates potential variation in implementation quality across VIFs, but more closely approximates the real world in which ag-input dealers do business. We define measures of supply-side compliance that relate to ag-input dealers VIF-organizing effectiveness and village-specific implementation outcomes which are necessary conditions for farmer take-up. We find that ag-input dealers are successful in marketing the VIFs with high levels of farmer awareness, but that village take-up (villages where a VIF was organized, and purchases were made) varies from 45-80 percent in post-harvest market fairs with forward contracts, to 100 percent in spot market fairs organized during the planting season. With respect to demand side compliance, farmer unconditional take-up varied from 18-24 percent of participants in VIFs organized with forward contracts and a 53 percent take-up rate in the spot market VIFs. The higher compliance in spot markets at planting time is associated with lower individual farmer demand. Farmers have lower liquidity during the planting season and a shorter time horizon to make complementary investments in seed, land preparation, water-control, or farm capital acquisition, which may lower demand. Alternatively, the salience of the planting season may also increase market participation, even if transactions are smaller.

Second, we estimate the effect of VIFs on the competitiveness of market prices and farmer trust in the VIF transactions. One potential concern in creating village markets is that ag-input dealers may price discriminate. We do not find evidence of price differences among the agricultural inputs frequently sold during the VIFs in comparison to the control group. Next, given the novelty of introducing forward contracts in a rural context and offering forward contracts linked to credit, we wanted to assess farmers' trust in VIF markets. Specifically, we ask whether farmers had trust in VIF transactions, namely that transactions would be completed as promised with the products (and quality) promised. We find farmers had high levels of trust in the VIF market transactions when spot markets were organized with or without credit. Farmer's also had high levels of trust in the forward contract treatments with 10 percent deposits, but we did not observe increased trust in forward contracts with harder commitments of 50 percent.

Third, we estimate the intention-to-treat effects for input demand. We find that VIFs with forward contracts or spot markets with credit increased farmer total input demand relative to the control group, but more consequentially also relative to simply providing farmers with market access in a planting season spot market. VIFs with forward contracts increased the total value of fertilizer used by farmers by 39,951 - 47,443 FCFA (23 - 28 USD) relative to the control group (24 - 28 percent relative to the baseline control mean). Adding credit options to the commitment contracts increases farmer input demand, but is not statistically different than effect sizes for VIFs organized with only commitment contracts. During the planting season with credit, total fertilizer demand increased by 38,548 FCFA (22.5 USD) (23 percent relative to the baseline control mean), an effect size which we are not able to reject as different than the post-harvest forward market VIFs. These results suggest that the effect of earlier post-harvest market timing and later planting season liquidity are effectively substitute mechanisms in creating input markets.

Fourth, while most households in our sample used agricultural inputs, organizing village input fairs addressed a key last mile problem in input adoption for households who had not used inputs in the previous season. Forward market VIFs organized in the post-harvest season increased fertilizer use between 9-14 percentage points. A spot market VIF organized at planting with credit increased fertilizer use by 12 percentage points. Both treatment effects are comparable to the baseline control mean of households who did not use agricultural inputs in the previous season.

Fifth, given the first stage demand effects from VIFs, we estimate the intention-to-treat and treatment on the treated effects on crop choice, labor, production, and marketed surplus. Despite new farmers' adoption of fertilizer due to VIF treatments and overall increases in farmer input demand, we do not observe changes in the farmers' crop choice portfolio, but do estimate moderate changes in household and hired labor demand. We do not measure statistically significant effects of VIFs on production value or marketed surplus at the village level (ITT results).

However, we estimate treatment on the treated effects among farmers who purchased inputs at the VIFs and find statistically significant and large effects on those farmers who participated in the fairs. Among farmers who purchased inputs at VIFs, we find a 52 percentage point (pp) increase in production value and a 60 pp increase in marketed surplus value relative to the control group mean for this subsample. TOT estimates on labor indicate a large increase in household labor and a higher demand for hired labor during the weeding season. Farmers who purchased inputs during the VIF were more likely to cultivate higher value legumes and cereals (peanuts and rice) relative to subsistence cereals (millet). Creating VIFs after one agricultural season did not produce agricultural production increases or broader agricultural transformation at the village level, but did increase production and agricultural income among those who purchased inputs during the fair.

Lastly, we assess the robustness of our results to potential concerns about multiple hypothesis testing. We track the number of hypotheses tested in our analysis and demonstrate that for our main results, we far exceed the number of expected statistically significant results among the hypothesis tests that drive our main results, alleviating concerns that treatment effects are due to multiple treatments or outcomes.

Our study contributes to the literature on farmers' constraints and the effect of market organization (contract design) on farmers' input demand, mechanisms explaining farmers' technology adoption and use, and the welfare and substitution effects of market creation on farm households and production. While these results are not a definitive response to why markets are missing in low income countries, our more modest question allows us to test which mechanisms make markets, perhaps the more relevant real-world policy question.

First, contract design to create agricultural markets is an underappreciated mechanisms when we vary the timing of agricultural input market timing. Much of the literature has taken a demand-side approach to relieve farmers constraints, particularly to promote farmer learning through agricultural extension, subsidize inputs, or reduce farmer risk with agricultural insurance Suri and Udry (2022). Recent attention to scaling has integrated supply side and market perspectives which are often taken as exogenous constraints in a farm household model. By focusing on the experimental variation of contracts in real transactions, the behavior of supply-side actors such as ag-input dealers is directly observed, but also best approximates with whom demand-side actors (farmers) actually interact. Our results illustrate a model where liquidity constraints can be addressed by the timing when markets are organized using forward contracts. Prices are competitive in the VIFs we observe. Taken together with demand and supply side compliance, these results illustrate that changes in the timing of markets also require alternative contract types. The contract attributes such as the level of commitment can itself influence farmer adoption and demand.

Second, we estimate the tradeoffs between commitment and liquidity on market design, illustrating that they are in fact substitutes. ITT effects on farmer input demand are statistically similar whether spot markets are organized during the planting season with credit or commitment contracts without credit are offered to farmers in the post-harvest period. This empirical result underscores a larger literature on farmer seasonal liquidity (Rui Albuquerque (2024)) and how market creation might be tied to seasonal liquidity to improve farmer investment in agricultural inputs. Our results are consistent with Aggarwal et al. (2024) which find that creating spot markets alone does not improve input demand or farmer welfare in Malawi.

Third, we find several explanations of our production results consistent with theories of agricultural transformation. While we did not expect VIFs to create agricultural transformation in a single agricultural season, the large literature on market access (J-PAL and CEGA (2024)) finds mixed results on the effects of market access on production. We find among farmers who participated in VIFs, substantial increases in production and marketed surplus, though no evidence in intention to treat effects on these second-order outcomes. We interpret these results as consistent with several stylized facts. First, a large literature tests for separability of household production and consumption decisions (Benjamin (1992), LaFave and Thomas (2016)) and estimates the consequences of misallocation due to market 'missingness' (Adamapoulous (1992), Gollin and Udry (2021), Dillon and Barrett (2017)). A key result of the separability literature is that resolving one missing market problem does not necessarily lead to welfare improvements if other missing markets exist. Second, it is not uncommon in the agricultural technology adoption literature to find increased adoption without yield or profitability (Beaman et al. (2013) Cole and Fernando (2021), Udry and Kolavalli (2019)). We interpret our results as consistent with both agricultural economic history (Pingali (2007)) and these more recent empirical studies which show that increased input demand is a necessary but not sufficient condition for agricultural transformation.

2 Literature

We motivate the farmer's problem in agricultural input markets with the canonical non-separable agricultural household model. Households maximize utility subject to budget, time and production constraints for a given production technology. The separability literature establishes that an implication of such a model is the non-separability of household consumption and production decisions when markets are incomplete. When markets are complete, households act as price takers and maximize utility recursively. Maximizing profits and taking profits as given in a second stage, the consumption bundle is chosen to maximize utility. The empirical literature has broadly failed to reject the null hypothesis of separability and the existence of complete markets (Benjamin (1992), LaFave and Thomas (2016)).[5]

Examples from the economic history of markets provide context for the Village Input Fair innovation and economic institutions that have emerged to create markets (North (1977)). One day fairs, trade shows, farmer's weekly markets, or annual/harvest festivals are examples of agricultural spot markets. They provide an opportunity for social interaction, the demonstration of skills and crafts, and for the exchange of goods. Fairs were a fixture of the Roman Empire, and the Romans introduced markets and fairs into northern Europe to encourage trade across the Empire. When the Western Roman Empire disintegrated in the late 5th century, virtually all organized commerce in Europe ceased until the late 7th century as these market institutions were abandoned (de Ligt (1994)). Milgrom et al. (1990) assessed how the medieval Champagne County fairs in Northeastern France filled the gap left open by weak market institutions to facilitate trade growth. In Kenya, Ensminger (1996) analyzes the transformation of markets among Orma pastoralists over several decades.

In discussions about markets, the empirical literature has focused on the definition of market access J-PAL and CEGA (2024). Chamberlain and Jayne (2013) examines correlations in measures of market access and changes over time in market access in rural Kenya. Common definitions of market access have often focused on proximity to roads, but Chamberlain and Jayne (2013) argue that distance to roads are poorly correlated with other more direct measures of farmers market access. They find evidence that supply-side actors behavior is most correlated with changes in more direct measures of market participation.

From a demand side perspective, the technology adoption literature has primarily focused on why farmers do not take-up seemingly profitable technologies. In rural Kenya, Duflo et al. (2011) study farmer demand when spot input markets are organized during the planting season in comparison to full prepayment of inputs during the post-harvest period. The rationale for organizing markets with forward contracts is that farmers often have high liquidity during the post-harvest season when crops are sold even if they do not need inputs until later at planting

[5] Recent work on labor market constraints have also highlighted how rationing (Breza et al. (2021)) or transportation frictions (Bryan et al. (2014)) lead to incomplete labor markets.

time. The authors interpret the behavioral effect of a post-harvest market as a mechanism to induce commitment to a profitable investment. Duflo et al. (2011) also provide a theoretical model incorporating time-inconsistent preferences in the context of agricultural input decisions. Offers to purchase inputs using a hard commitment mechanism (full payment at commitment) with guaranteed delivery during the planting season were found to induce much higher take-up of fertilizer than subsidized or market-priced offers of fertilizer purchase during the planting season. The experimental design uses market timing to test the hypothesis that farmers may have time inconsistent preferences leading to suboptimal investment decisions. These models, taken with standard models of credit constraints (Deaton (1992), Bardhan and Udry (1999)), do not provide unambiguous predictions of whether time-inconsistent preferences may most affect agricultural input demand, particularly when farmers are credit constrained, leaving ambiguity about which constraint (time consistency or credit) would be binding. As the demand for a commitment mechanism may be driven by multiple constraints, our study is motivated by the empirical question of estimating the relative effects of different market structures on farmer input demand.

In Burkina Faso, Dillon et al. (2018) build on the experimental design of Duflo et al. (2011) by including a soft commitment mechanism for sorghum farmers. Farmers commit early, in the post-harvest season, by signing a purchase order with an agricultural input dealer during an input fair, and depositing 5 percent of the total order price. Full payment is made upon delivery of the fertilizer during the planting season. Take-up rates were particularly high among those farmers who received soft commitment offers early in the post-harvest season, rather than late at planting, even when the transactions in the planting season were subsidized. These take-up rates are similar to those predicted by Duflo et al. (2011) and may be more effective than full-commitment devices (full payment at commitment) as demonstrated in other contexts (Karlan and Linden (2014)). Similarly, Casaburi and Willis (2018) documents that offering agricultural insurance contracts during the post-harvest season significantly increases insurance uptake compared to offering the same product at planting time. Axmann et al. (2019) provides evidence for the case of certified hybrid seed whose take-up is highest during the harvest period.

On the supply side of input markets, the literature has focused on the effects of asymmetric information in matching agricultural input sellers with potential buyers. First, information about potential buyers and sellers is often unavailable, despite the rise of information technology and communication (Aker (2010)). Moreover, there is evidence that agricultural input markets are characterized by information asymmetries regarding the quality of goods, the reputation of agents, and various commitment problems caused by weak institutions (Michelson et al. (2021), Fafchamps (2020), Aker and Fafchamps (2015), Bird and Fafchamps (2004)). Taken together, these factors reduce competition and negatively affect the incentives of dealers to expand markets in rural areas.

In addition, unreliable transportation infrastructure affects not only market access for farmers, but also the marketing costs faced by input dealers. When markets are sparse and population density low, as is the case in many African countries, transportation costs are a barrier to moving goods from cities to farms and vice versa (Aggarwal et al. (2022), Casaburi et al. (2012), Fafchamps and Gabre-Madhin (2006)). Finally, private agents lack sufficient information about demand in remote areas. Farmers' demand for inputs varies within and between villages, potentially increasing input dealers' uncertainty about where to market their products (Macours (2019), Lybbert et al. (2018)). This demand heterogeneity is exacerbated by how agricultural inputs respond to the characteristics of different soils and by the need to obtain complementary factors such as equipment, labor, or water to ensure a profitable input investment (Harou et al. (2022), Corral et al. (2020)). This related literature motivates our work on agricultural input market development, with a particular emphasis on village-level interventions that address constraints for farmers and ag-input dealers.

3 Experimental Design

3.1 Input Markets in Mali

Agriculture in Mali is primarily composed of smallholder farmers where the production system is labor-intensive, and farmers have limited access to agricultural inputs like capital, water, labor, seeds, fertilizers, or insecticides. Seventy-two percent of the population cannot afford a healthy diet (Ritchie (2021)), while 62 percent of people work in agriculture — a share that has declined by 10 percentage points in the last twenty years (Roser (2023)).

Our experiment was based in four regions: Sikasso, Koulikoro, Kangaba, and Bananba. Arable land is widely available in these regions, but the nutrient content of the soil is generally very low (Dembele et al. (2016), Vanlauwe et al. (2000)) and agricultural input use, considering improved seeds, fertilizers, and insecticides, is low compared to international standards (Ritchie et al. (2022)). Farmers mainly grow millet, rice, and cotton in these regions. At baseline, households cultivated 8.3 hectares across six plots. Production is coordinated within the household where all members contribute labor to larger household plots used for cereal production. Most adult household members in rural areas also maintain individual plots with men often producing a cash crop and women producing vegetables and condiments in a secondary season.

Input markets are largely missing in rural areas. 70 percent of farmers in our baseline survey reported that they did not have access to an ag-input dealer in their village in prior agricultural season. Farmers primarily travel to secondary towns to procure fertilizer, but missing markets reduce access for poorer farmers and women who have more limited mobility. These reports are consistent with other Sahelian zones in West Africa such as northern Ghana where 80 percent of farmers travelled to local towns to purchase inputs (Osei et al. (2022)).

Why don't ag-input dealers expand their businesses to create these rural markets? One barrier to input market formation is the cost of transporting inputs to rural markets, which accounts for around one-third of the total price (USAID (2018)). Storage facilities for fertilizer and other inputs are inadequate, increasing the risk of input quality deterioration and higher coordination costs. A second barrier is the high correlation between weather uncertainty and planting timing. Smallholder farmers without access to water control plant after the first rainfall to ensure seed germination. This creates high seasonal demand which must be satisfied throughout the rural sector in a few weeks. Small-scale ag-input dealers have procurement difficulties working in highly informal supply chains to meet demand. Lastly, both farmers and input dealers do not have high levels of confidence in bilateral transactions. In our qualititive work, both actors will recount stories of unrealized market transactions that resulted in losses of time and money. Overall, this market environment results in a fragmented private input supply sector, consisting mostly of small-scale secondary input dealers (USAID (2018)) that are connected to a few large dealers in the capital, Bamako.

Ag-input dealers in our rural study areas are local dealers who primarily supply farmers from their shops in secondary towns. These ag-input dealers are at the base of the ag-input supply chain. They often employ one or two seasonal employees, and procure their stocks from wholesale ag-input dealers in large cities. Dealers who participated in VIFs supplied commonly used fertilizers in Mali such as urea, DAP and NPK. These fertilizers are used in the production of cereals, tubers, leafy greens and other vegetables, and cash crops such as cotton. Urea is added to add nitrogen to soils, while DAP is primarily composed of phosphorous with a smaller amount of nitrogen. NPK is composed of lower amounts of nitrogen, phosphorous and potassium. Most mineral fertilizer use in Mali is concentrated in the cotton sector and a national centralized rice irrigation scheme (Office du Niger) rather than in subsistence agriculture (Dissa et al. (2022)). Access to inputs for cotton and irrigated rice farmers is heavily influenced by national agricultural policy and the publicly owned cotton company (CMDT).

For the average smallholder farmer, fertilizer prices in Mali are relatively high compared to other parts of Africa (Sanga et al. (2021)). In Mali, the average retail prices of urea, DAP, and NPK for the year 2018, when we conducted the experiment, are 0.52 \$/Kg for urea, 0.65 \$/Kg for DAP, 0.47 \$/Kg for NPK.[6] The primary data that we collected in the study area during the study period indicate that these commonly used fertilizers were priced between the local currency equivalent of 0.40 and 0.50 \$/Kg, while industrially produced organic fertilizers were priced at \$0.27/Kg. Herbicides and insecticides were priced at \$7.81 and \$8.19/liter, respectively, while commercially improved seeds were priced at \$1.20/Kg.

We highlight two Malian private sector actors which help shape input markets in our study area. The first is UNRIA, which has a network of at least 950 agricultural input dealers in the target regions, represents the main national association of dealers, and plays an active role in agricultural policy. UNRIA mobilized input dealers to supply the VIFs in our experiment. A second private sector actor is Soro Yiriwaso, a microfinance institution based in Mali that has a wide suite of agricultural savings and credit products targeted primarily to rural areas. Although Soro Yiriwaso has been operating in Mali for more than two decades, access to agricultural credit is low due to high demand and relatively few microfinance organizations like Soro Yiriwaso with agricultural lending products.

3.2 Study Design

The experimental design focuses on the market characteristics of Village Input Fairs. These commercial events are not common in rural Mali. When set up by extension services or by the input dealers of UNRIA, they are almost exclusively organized at the time of planting and often in secondary towns or cities as agricultural expositions, rather than in the village context, to sell inputs directly to small-scale farmers. In contrast, VIFs are markets created directly in rural villages that build on the self-interest of three actors: input dealers, farmers, and microfinance institutions. In all VIFs, input dealers organize a one-day fair where farmers can purchase inputs after product description and marketing presentations by the input dealers. Farmers are able to purchase any inputs in any quantities they demand, and no pricing policy is enforced. All actors make decisions to sell, purchase or offer credit independently and in their own interest.

Variations in the VIF treatment are defined by i) the timing at which the input fair is organized, either during the post-harvest season organized with a forward contract or at planting time organized as a spot market; ii) the level of deposit required to place an order with a forward

[6] Data on fertilizer retail prices in eighteen African countries are collected and made publicly available by the Africafertilizers project, powered by IFDC – https://africafertilizer.org.

contract, with a 'soft' 10 percent or 'hard' 50 percent deposit, and iii) the possibility to obtain credit during the fair. The combination of i) and ii) yields three possible ways of organizing the VIFs. Either the VIF is organized during the post-harvest season (with a 10 percent or 50 percent deposit requirement), or the fair is organized at planting season without a deposit amount because farmers pay in full at the spot market. Figure 1 describes the 3 x 2 experimental design where treatment is assigned at the village level. The timing and level of commitment is one level of randomization (post-harvest VIFs with 10 percent deposits, post-harvest VIFs with 50 percent deposits, and planting season VIFS) which are cross randomized with or without credit availability. We selected 140 villages in the defined study area using the National Census available from the Statistical Office in Mali. Each treatment group and a control group were randomly assigned 20 villages.

In the markets where 10 percent deposit forward contracts are offered, the microfinance institution facilitates the interaction between farmers and input dealers by accompanying the supplier in organizing the Village Input Fair. Farmers are offered the opportunity to purchase agricultural inputs with the possibility of financing their purchase with a loan. The interested farmer places the order and pay a 10 percent deposit on the day of the fair. The balance of the purchase could either be paid by the farmer or financed by a loan that would be activated upon delivery of the inputs by the dealer at the beginning of the planting season. The microfinance institution deposits the purchase order in a blocked account. The funds in the blocked account are transferred in full to the input dealer at the time of delivery, along with the balance payment - paid by the farmer or financed by the loan. At the time of delivery, it is possible for a farmer to default on the purchase. In this case, the microfinance institution transfers the order deposit to the input dealer. In reality, we did not observe any farmers who completely defaulted. In cases where farmers had difficulty making the final payment at delivery, farmers renegotiated their order quantity, but again this was a small number of cases. The same VIF organization was used in another treatment arm, but without the credit option, which was not offered to farmers in this case.

In the VIFs organized with 50 percent deposit forward contracts, the input fair takes place immediately after the harvest of the previous agricultural season, but in this case the farmer places input orders and pays a higher rate, equal to a 50 percent deposit, on the day of the Village Input Fair. The balance could either be paid by the farmer or financed by a loan. The same VIF organization was used in another treatment arm, but without the credit option, which was not offered to farmers. We emphasize that the deposit requirement is a mandatory feature of the VIFs organized as a market in the post-harvest season (groups 1, 2, 3, and 4 in Figure 1). In contrast, three treatment arms (groups 3, 4, and 5) include the possibility of a credit option that is entirely voluntary. Farmers can apply for credit, but the microfinance organization treats each farmer as a client and determines eligibility only after credit screening. In all fairs organized with forward contracts, input delivery occurs on a specified date just before the planting season in bulk for all farmers. Bulk delivery ensures lower costs for ag-input dealers and allows farmers to verify input quality before accepting delivery.

Finally, two VIF treatment arms are organized as spot markets at the beginning of the planting season, in groups 5 and 6. In the spot market VIFs with credit, the purchase value is either paid directly by the farmer or becomes a loan. In the latter case, the microfinance institution makes the payment directly to the input dealer after signing the loan contract with the farmer. The same VIF organization is used in treatment group 6, but without the credit option, which in this case is not offered to farmers. Treatment group 6 is essentially a planting season spot market.

A common feature of all village input fairs is demand aggregation and third party verification of transactions. Demand aggregation is of particular interest to ag dealers who would not normally be motivated to supply individual farmers in rural villages. Third party verification is of interest to both input dealers and farmers. Input dealers are assured that farmers will pay at delivery and that farmers may want to renege will face social pressure of having to renege in front of other community members. Such social pressure also works to the advantage of farmers should an input dealer not deliver promised inputs at an expected quality. The input dealer risks losing their reputation with an entire village rather than a single farmer.

4 Empirical Strategy

4.1 Econometric Analysis

We estimate two intention-to-treat (ITT) effects using market and household agricultural data. Data collected during the study included a household baseline, market outcome data collection during the Village Input Fairs, credit information, and a follow-up household survey administered after the agricultural season that followed the intervention. We observe input allocation decisions for household plots (baseline in 2017 and follow-up in 2018) and we obtain one observation of household agricultural production, crop choice and labor post-treatment in 2018.

We begin our analysis by comparing demand and supply side compliance, market price, and farmer trust in market transactions conducted during the VIF by treatment group. The market outcomes that we tracked are village participation in input fairs, the percentage of villagers that conclude a transaction conditional on participating in the fair, market prices and trust levels in the market transaction. Village-level summary statistics and the results of this analysis are reported in Table 1 and 2.

$$y_m = \alpha + \Sigma_{k=1}^{6} \beta^k T_{km} + \epsilon_m \tag{1}$$

We estimate the market level treatment effect, β^k, in equation 1 on the outcomes of interest y_m - where m defines the markets organized in the Village Input Fairs.

At the household level, our primary outcome of interest is unconditional input demand. We estimate each of these ITTs relative to the control group, in equation 2. Though most farm households use some type of agricultural input at baseline, we also estimate the effect of VIF market structures on adoption.

In the pooled difference in differences specification (Bertrand et al. (2004)), we estimate the effects on input allocation decisions for household h in treatment group k in season t relative to the control group. Standard errors are clustered at the village level, the unit at which treatment was assigned.

$$y_{ht} = \alpha + \Sigma_{k=1}^{6} \delta^k T_{kh} + \gamma Year + \Sigma_{k=1}^{6} \beta^k (Year \times T_k) + \epsilon_{ht} \tag{2}$$

The coefficient β^k in equation 2 is the difference in difference estimate, which measures the change in outcomes over time of the farmers receiving the treatment k relative to the change in outcomes of farmers in the control arm. In this specification, we estimate ITT effects on agricultural input extensive and intensive margins. The results of this analysis are reported on Tables 3 and 4.

For outcomes including agricultural production, agricultural sales, crop diversity, and labor, we estimate the intention-to-treat effects using differences within the same agricultural season, as our baseline data did not include detailed plot level agricultural outcomes. Standard errors are clustered at the village level, the unit at which treatment was assigned.

$$y_h = \alpha + \Sigma_{k=1}^{6} \beta^k T_{kh} + \epsilon_h \tag{3}$$

In this case, the coefficient β^k in equation 3 estimates the ex-post difference between the outcome mean in treatment arms with respect to the mean in the control arm. Results from this analysis are reported in Tables 5, 6, and Table 7.

All estimates rely on the independence between treatment and household covariates. In particular, equation 3 relies on this assumption as we are estimating treatment effects in levels rather than in differences. To assess the validity of the independence assumption, we use agricultural, household, and asset variables collected in the baseline census to assess balance across the six experimental groups and the control. These results are presented in Appendix Tables A1-A2. The market access and credit group in the planting period are imbalanced relative to the control group for the plot area and household size variables. The two treatment groups have smaller land size and household size relative to the control leading to potential underestimation of effect size in this group if land and labor availability are more highly correlated with fertilizer take-up and use. Assets ownership is also less prevalent in several of the treatment groups for beds and video cassette recorders (VCRs) relative to the control group, although bikes are more frequent in the treatment groups. Asset values conditional on ownership are generally similar across the groups. Baseline covariate imbalance likely lead to a moderate underestimation of the intention to treat effects in our sample.

For our second-stage results on production, labor and crop choice outcomes, we also estimate treatment on the treated effects by instrumenting the endogenous variable purchases during the VIFs with the treatment variable. Though not causal estimates of the effect of village input fairs, take-up likely affects the statistical power with which we might be able to measure such second order effects. For this reason, we estimate these second order treatment on the treated to illustrate how, conditional on take-up, farmers's complementary decisions about labor and crop choice and production respond to increased market access.

4.2 Testable Hypotheses

We outline four testable hypotheses from our experimental design and data structure which provide insights on market making mechanisms.

4.2.1 Hypothesis 1: Input markets are competitive.

We test whether the spatial variation in prices faced by households are observationally equivalent between treatment groups and in markets organized during different time periods. Forty-six independent ag-input dealers participated in our experiment and were assigned to participate in multiple Village Input Fairs. As independent ag-input dealers, they could set prices in their VIF market. We test the null hypothesis that farmer input prices do not vary across treatment and control groups in equation 4:

$$H_o : E(p_h \mid k = 1) - E(p_h \mid k = 2) - ... - E(p_h \mid k = 7) = 0 \tag{4}$$

Rejection of the null hypotheses indicates that input dealers have market power, while failure to reject the null is consistent with competitiveness in input markets. We test prices in markets organized during the post-harvest and planting seasons, as well as markets organized within these periods.

4.2.2 Hypothesis 2: Farmers input demand is driven by time-inconsistent preferences rather than liquidity.

We measure farmer stated preferences by asking farmers to choose between a current and future monetary transfer similar to elicitation methods described by Cohen et al. (2020) or Falk et al. (2023). Descriptively, we find 87 percent farmers were either impatient or time-inconsistent and 13 percent of farmers were patient.

Given the high proportion of time-inconsistent farmers in our sample, we can test directly whether time-inconsistent preferences or liquidity have stronger effects on farmer input demand. That is, we compare input demand from VIFs organized with a forward contract treatment without credit to the spot market treatment with credit. All else being equal, we can measure the revealed preferences of a farmer faced with a forward contract that includes a commitment mechanism and compare them to input demand expressed in a spot market where liquidity constraints are alleviated by credit offers.

$$H_o : \beta^1 - \beta^5 = 0 \tag{5}$$

$$H_o : \beta^2 - \beta^5 = 0 \tag{6}$$

4.2.3 Hypothesis 3: Within seasonal liquidity affects market demand.

We estimate the marginal effects of forward contract VIFs or spot market VIFs by comparing the demand for 10 percent deposit forward contracts (with and without credit), 50 percent deposit forward contracts (with and without credit), and spot market contracts with and without credit. Testing this hypothesis provides an estimate of the marginal effect of within season liquidity.

$$H_o : \beta^1 - \beta^3 = 0 \tag{7}$$

$$H_o : \beta^2 - \beta^4 = 0 \tag{8}$$

$$H_o : \beta^5 - \beta^6 = 0 \tag{9}$$

4.2.4 Hypothesis 4: 'Soft' commitment increases demand relative to 'hard' commitment mechanisms

The behavioral economics literature recognizes that commitment mechanisms can vary in intensity, leading to differences in behavioral responses Karlan and Linden (2014). In our experimental design, we vary the level of commitment in forward contracts based on the amount of deposit a farmer must pay up front to secure the order. Harder commitments reduce future liquidity which may reduce input demand in the current period. Farmers may value this commitment mechanism if they have time inconsistent preferences. Whether the value of the commitment mechanism depends on the deposit amount or the option to order inputs and be committed to paying for them in the future period is an empirical question. This is a directly testable hypothesis in our experimental design by comparing demand levels between VIFs with 10 percent deposits and VIFs with 50 percent deposits.

$$H_o : \beta^1 - \beta^2 = 0 \tag{10}$$

5 Results

We report five sets of results on: supply-side implementation and demand-side take-up, the ITT effects on market outcomes, the ITT effects on household input demand, the ITT effects on adoption, and the ITT on other inputs include crop choice and labor, and lastly ITT and TOT effects on production and agricultural sales.

5.1 Compliance: Supply-side implementation and Demand-side Take-up

Table 1 presents compliance statistics for the Village Input Fairs by treatment group. One concern with supply-side interventions implemented by private actors is the possibility that low compliance *on the supply side* may affect farmer uptake. Because we work with a consortium of ag-input dealers, implementation quality might vary by site independent of farmer's willingness to purchase inputs in VIFs. To quantify this supply side dimension of compliance, we identify variables directly tied to supply-side implementation quality. We find that households in treatment villages were aware of the input fair (Table 1, column 1), but not all villages participated in the fairs. We characterize village take-up heterogeneity as a combination of demand- and supply-side compliance, as the ag-input dealers primary responsibility was to organize the VIFs.

Village-level take-up rates are 100 percent in the planting season when markets are organized as spot markets, but range from 45 percent to 80 percent in the post-harvest season when markets are organized in the post-harvest period with forward contracts. In villages where the VIF was organized with a 50 percent commitment and credit offered, several village leaders unilaterally declared their unwillingness to engage the entire village on such terms, which they considered too high and risky for their farmers. In qualitative work in treatment villages and with agricultural input dealers, farmers reported some reluctance to use commitment contracts. These markets required more trust between input dealers and farmers and were more novel contracts compared to spot markets. We observe higher compliance with soft commitments (10 percent deposit requirements) than harder commitments (50 percent deposit requirements). Despite differences in village take-up across treatments, unconditional farmer compliance ranged from 18 to 24 percent of farmers participating in the post-harvest season markets with forward contracts and 53 percent in the planting season, spot market VIFs.

Among farmers who ordered agricultural inputs with forward contracts, an open compliance question was whether farmers would pay their order balances upon delivery. Farmers rarely reneged on their commitment contracts, but did renegotiate quantity ordered, particularly when financing with credit was not possible. Table 1, column 4, shows the percentage of the farmer's final order value paid to the ag-input dealer. In spot markets, there is no concern about revising or reneging on orders with a 100 percent order fulfillment rate. In the 10 and 50 percent commitment groups without credit, 70 and 82 percent of the order value was actually purchased. In the same groups with credit, 95 and 98 percent of the order value was actually purchased at delivery.

5.2 Market

Table 2 presents results on market prices and farmer self-reported trust in their VIF market transactions. In the bottom panel of Table 2 and each subsequent table, we formally test for differences between treatment groups. We report the groups for which we reject the null hypothesis that the treatment effects between groups i and j are equal. All p-values for the joint tests are reported in the Appendix Table B1.

A primary supply side concern is whether organizing VIFs with only a few (on average 2-3 input dealers) creates an opportunity for price discrimination as stated in Hypothesis 1. There are several reasons why VIF prices may vary from prices in other fertilizer markets. We test whether VIF prices in the post-harvest or planting seasons differ from input prices found in the control group. We find little variation in the price of fertilizers among the post-harvest treatment groups for the three major fertilizer categories that are usually traded in Village Input Fairs (e.g. Urea, DAP, and NPK). For NPK, where fairs were organized as spot markets at the planting period, we find higher prices with higher standard deviations than the control group, suggesting more price variation in planting season markets. We do not find a statistically significant difference in VIF prices relative to the control group. We also do not find systematic differences between VIF treatments in market prices. The integration of credit into VIF markets could have caused input dealers to offer higher prices, but we do not find evidence of this behavioral response by input dealers. Broadly, we conclude from this analysis that prices were competitive in VIF markets.

We also asked farmers about their trust in input market transactions in their respective groups. Anecdotally, both farmers and ag-input dealers can be skeptical of the trustworthiness of the other, particularly in bilateral transactions where contract enforcement is weak. In treatments with the highest participation, we find higher reported levels of trust which are statistically different than the control group, ranging between a 4 and 6 percentage point increase in trust levels. While these effect sizes are not large, a common concern among farmers is the quality of agricultural inputs. While we can not independently validate the quality of inputs as in Harou et al. (2022), we find that farmers self-report high levels of trust in treatment villages indicative of comparable input quality in VIFs as in other input markets. VIFs introduce third-party verification when inputs are delivered and community observability of transactions in VIF marketplaces which may also have reinforced trust in VIF transactions.

Although ag-input dealer profitability from participating in VIFs is heterogeneous, our administrative data indicates that VIFs generated, on average, XOF 275,450 (equal to USD 525.31) in revenue. Ag-input dealers report in our follow-up surveys that they regard VIFs as a profitable investment as it increases their sales and customer base.

5.3 Input demand

Table 3 presents the effects of VIF treatments on unconditional input demand, our main outcome of interest, which we estimate using a difference-in-differences specification. We specify input demand as the total input value of fertilizer and pesticide used during the agricultural season (Column 1). We also provide ITT estimates for disaggregated effects on fertilizers and pesticides as independent categories (Columns 2 and 3) as well as specific fertilizers: urea, DAP and NPK (Columns 4, 5, and 6). A few input demand patterns emerge from the table. First, by comparing Columns 1 and 2, total input value and total fertilizer value, treatment effects are primarily driven by farmer fertilizer demand rather than pesticide demand. Second, providing market access through a spot market in the planting season (treatment Group 6) has no statistically significant effect on input demand relative to the control group. This result is consistent with Aggarwal et al. (2024) who created spot markets during the planting season and similarly found no effects. We are also able to reject the hypothesis that total input demand in treatments 2-5 are statistically similar to the market access group (Column 1). We conclude from these results that commitment and liquidity are important market organization mechanisms independent of creating the market itself. Third, total fertilizer demand (Column 2) is primarily driven by DAP input demand for all treatments. The effect sizes range from 23 to 28 percent of baseline mean total fertilizer demand (USD 66 - 81). Farmers demand larger values of DAP relative to Urea and NPK, likely because farmers perceive soil nutrient deficiencies to primarily be driven by phosphorous rather than nitrogen. For treatments where credit is offered during the VIF, farmers demand for urea increases significantly. The effect sizes range from 20 to 28 percent of the baseline mean urea demand (USD 20 - 29). We do not find effects of market organization on NPK demand.

Given these results, we focus on the effects of commitment and liquidity to make input markets. We test three hypotheses described above: whether farmer input demand is driven by time-inconsistent preferences (Hypothesis 2), the effect of within season liquidity on market demand (Hypothesis 3), and whether 'soft' commitment increases demand relative to 'hard' commitment mechanisms in forward contracts (Hypothesis 4).

A hypothesis tested by Duflo et al. (2011) is that time-inconsistent preferences drive farmer demand for commitment contracts. Their conclusion is driven by comparing farmer demand with a commitment mechanism relative to subsidized fertilizer in the planting season. Our hypothesis test compares soft commitment contracts in the form of 10 or 50 percent deposit requirements with credit availability rather than price discounts. We can not reject the hypothesis that commitment treatment effects are equal to treatment effects of markets organized with credit. Focusing on total fertilizer demand (Column 2), treatment effects for the 10 percent commitment (Group 1) and credit (Group 5) treatment groups are statistically similar. The pairwise t-test between coefficients of the 50 percent commitment (Group 2) and credit (Group 5) also are statistically similar. We interpret these effects as consistent with Duflo et al. (2011) findings that commitment mechanisms drive farmer demand. These results do provide a broader test of the relative effects of commitment and liquidity by using an alternative treatment to test reducing liquidity constraints. Credit availability might be a better intervention to reduce liquidity constraints rather than price subsidies, particularly in our Malian context where farmers are poorer and have lower welfare levels compared to many other small holder farmers. By reducing liquidity constraints, the results demonstrate that softer commitment mechanisms are substitutes for planting season agricultural credit.

Given that commitment and liquidity are substitutes, our results also test the effect of within seasonal liquidity on market demand (Hypothesis 3). Whether we focus on total input demand or fertilizer demand (Columns 1 and 2), we can not reject the hypothesis that treatments organized in the post-harvest season with commitment contracts are similar to those treatments with commitment contracts and credit. Though treatment effects are slightly larger in treatments organized with credit (group 1 vs group 3; group 2 vs group 4), effects on total input demand or total fertilizer demand are not statistically different. Credit offers with the commitment treatments did change the composition of fertilizer demand with statistically significant effects on urea demand as noted above.

Within the post-harvest treatments, we are also able to test whether 'soft' commitment increases demand relative to 'hard' commitment mechanisms (Hypothesis 4). If we think of commitment contracts from the perspective of a two period model of input demand, increases in commitment deposit amounts in period 1 might reduce demand, particularly if farmers have time inconsistent preferences. However, we do not find evidence that input demand is statistically different in the 10 or 50 percent commitment treatments. We are not able to reject the null hypothesis in pairwise t-tests between groups 1 and 2; and groups 3 and 4.

5.4 Adoption

In our unconditional demand analysis, treatment effects could be driven by either farm households that had not used agricultural inputs previously or those who are increasing the intensive margin of input demand. Table 4 presents the extensive margin of demand use, the binary variable if the household uses inputs or not. At baseline, fertilizer and pesticide use at the household level is relatively high. The baseline average household input use is 91 percent, with 85 percent of households using fertilizer and 87 percent using a pesticide. Despite relative high baseline mean adoption, we find significant treatment effects on adoption for fertilizer use, consistent with the patterns of treatment effects for unconditional fertilizer demand. This result shows that making village markets supplies the marginal farmers that would otherwise be denied market access. In this sense, village input fairs represent an opportunity to address the last-mile problem in household input adoption.

We find statistically significant effects of commitment contract VIFs and spot markets organized with credit on input use. Fertilizer effect sizes range from 9.6 percentage points to 13.7 percentage points and are driven primarily by urea adoption rather than DAP or NPK adoption. We cannot reject the hypothesis that any of the treatment groups are different from each other. The adoption results are broadly consistent with the hypotheses testing whether time inconsistent drive farmer demand, the effect of within season liquidity on demand and the relative effect of "soft" versus "hard" commitment mechanisms.

5.5 Complementary Inputs: Crop Choice and Labor

Observed changes in input demand could affect household output or other input margins such as the household crop portfolio or agricultural labor demand. Table 5 presents the ITT estimates of VIFs on household crop choice. Despite the potential for VIFs with commitment contracts to increase a farmer's seasonal planning horizon, we do not find a consistent pattern of crop portfolio substitution among the most commonly grown crops in our sample. These results show that there is no systematic substitution into or out of a particular crop for the average treatment group farmer. We estimate the treatment on the treated in Appendix Table C1. Among farmers who purchased inputs at VIFs, we do find crop substitution effects. Farmers who purchased inputs at VIFs were more likely to substitute into higher value cereal and legume crops (+18.7 pp more likely to cultivate rice and + 12.6 pp more likely to cultivate peanut) and substitute out of subsistence cereals like millet (- 16.8 pp less likely to cultivate millet).

We also estimate agricultural labor supply effects that may result from increased input utilization as labor and fertilizer are complementary inputs. In Table 6, we report ITT estimates for agricultural household labor days and agricultural hired labor days. Household labor supply increased in the VIF treatments relative to the control, primarily for planting (25-34 labor days) and weeding (30-36 labor days). We do not estimate significant treatment effects on household labor supply response during the harvest period. On hired labor demand, we largely do not see an effect on hired labor in response to the VIF treatments. For the 10 percent commitment group, we observe a 7 day or 116 percent increase in hired labor for the weeding season. There is not an increase in hired labor during the weeding period for the VIF treated households, but overall hired labor demand is very low in these households as most planting and weeding is conducted by household members. Though agricultural labor supply data is often noisy, these results suggest that increased labor demand resulted from VIF treatments where we found first-order input demand effects.

Among farmers who purchased inputs at VIFs, our treatment on the treated results (Appendix Table C2) indicate increases in household labor allocated to planting labor (76 pp increase relative to the control group mean), weeding labor (59 pp increase relative to the control group mean), and harvest labor (37 pp increase relative to the control group mean). Though farm households primarily utilize household labor rather than hired labor, we also estimate substantial treatment on the treated effects for hired weeding season labor (+ 181 pp increase relative to the control group mean).

5.6 Production and Marketed Surplus

Table 7 presents ITT estimates of VIFs on household production value and marketed surplus. Changes in input demand associated with the VIFs organized either as a commitment contract or as a spot market lead to increases in the total value of household agricultural production, but these effect sizes are not statistically significant. We also do not find evidence that these treatment effects are statistically different from each other.

Among farmers that purchased inputs at the VIF, we estimate an increase in fertilizer use (50 pp increase relative to the control group mean) and total inputs used at the household level (42 pp increase relative to the control group mean). Increases in input values are primarily driven by DAP purchases (75 percent of the TOT estimate of total inputs used). Associated with these increases in inputs purchased among farmers who purchased inputs at the VIF, our treatment on the treated estimate indicates a 52 pp increase in production value and a 60 pp increase in marketed surplus value relative to the control group mean for this subsample (Appendix Table C3).

Our ITT and TOT estimates provide some indications about mechanisms. It is not uncommon to find agricultural interventions that have first stage effects without second stage effects on production or income. A related agricultural technology adoption literature also often finds increased adoption without yield or profitability (Beaman et al. (2013) Cole and Fernando (2021), Udry and Kolavalli (2019)). When we observe first-order effects on adoption and input demand, there are several reasons why we might not observe second order ITT effects on production or income. First, complementary substitution effects may affect production outcomes. In our study, we can rule out this possibility as households that purchased inputs at VIFs increase labor and shift to more profitable crops. Second, we estimate TOT effects on quantities and values of fertilizer used per hectare in Appendix Table C4. Here we find that though the level that the household is using increases, this increased input demand had no statistically significant effect on increasing quantities used per hectare (though all parameter estimates are greater than zero). Hence, farmers are not necessarily intensifying production. Insignificant ITT results may be due to low intensification of fertilizer used on plots cultivated. They could also result mechanically from low take-up or effect size which reduces the statistical power of our production ITT estimates.

Though we can not directly test this hypothesis, our results are also consistent with theoretical predictions from the nonseparability literature about incomplete markets. A main theoretical prediction from the nonseparability literature is that resolving one missing market does not improve welfare if other markets are incomplete. While much of the nonseparability literature has focused on indirect tests, the VIF treatments directly create markets in rural villages for agricultural inputs. Our results are consistent with the nonseparable model prediction that creating markets may not increase yields or household welfare if other markets remain incomplete. In our rural Malian context, farmers also face missing markets for insurance, land or water-control technologies which may prevent increased productivity.

5.7 Multiple Hypothesis testing

We present aggregated outcomes for input demand, input adoption and production measures to reduce the dimensionality of our dataset following Anderson (2008), but each of the main outcomes that we can potentially aggregate have sub-components which themselves are of interest (for example, fertilizer and fertilizer type (NPK, DAP, urea) demand). This leads to many hypotheses and potential multi-hypothesis testing bias. Our results are qualitatively similar whether we focus exclusively on aggregated outcomes relative to disaggregated components of demand.

To assess whether potentially controlling for the false discovery rate would substantially change our empirical conclusions, we track the number of hypotheses by treatment group and between treatment groups; and the share of significant hypotheses reported in our main specifications. In tracking our hypotheses, we exclude compliance-related hypotheses from this analysis, as they are expected to be positive and strongly significant. For our main hypotheses, the sign and significance is the empirical motivation for this paper. Hence, finding groups of hypotheses that reject the null hypothesis at rates meaningfully above or below the power of the test, set to conventional 10 percent, suggests that the empirical results are not driven by multiple hypothesis testing.

In Appendix D, the statistical significance of our empirical results far exceeds the statistical power of our hypothesis test. At conventional powered tests of statistical significance (10 percent), we find that T1, T3, T4 and T5 all have percentages of significant hypothesis above 23 percent of the hypotheses tested. For our spot market treatment where we did not find convincing patterns of statistically significant results, the percentage of significant hypotheses (6 percent) is below our conventionally powered tests of statistical significance.

6 Conclusion

A central feature of low-income countries is the absence of markets for goods and services. In Mali, farmers demand fertilizer but often can not purchase it. Ag-input dealers would like to increase their fertilizer profits, but do not service rural communities. We focus on market organization mechanisms to better understand how to make markets, estimating the relative effects of timing, the deposit rate (commitment levels) used in forward contracts, and the availability of credit offers. Markets organized in the post-harvest period and with credit access had strong effects on farmer demand, input adoption, and household agricultural labor supply. The creation of spot markets during the planting season did not have an effect on agricultural input demand suggesting that timing and liquidity are market making mechanisms. We also find these market making mechanisms can be substitutes: forward contracts in the post-harvest period are substitutes for agricultural credit interventions organized during the planting season.

The results suggest several reflections on the potential behavioral implications of forward contracts which we are not able to fully disentangle in the present design. While the demand for forward contracts is consistent with time-inconsistent farmer preferences and higher liquidity in the post-harvest period, an alternative explanation is that farmers value forward contracts to protect them from market volatility that can occur later at planting time. At planting, farmers often face supply shortages, generating the risk of not having access to their agricultural input of choice. The timing of when inputs are available is a risk for rain-dependent farmers because input application depends on the uncertainty of when the rainy season begins, a period of a few weeks. Farmers often wait for subsidized fertilizer which does not come. Input quality is another risk factor perceived by the farmers which may encourage ordering inputs from trusted ag-input dealers earlier, although farmer beliefs are not generally a good proxy for true input quality (Michelson et al. (2021), Michelson et al. (2023)). Lastly, input prices can be volatile as high prices resulting from the Ukrainian war illustrated in 2022.

These are all factors that the literature has shown to be crucial in farmers' investment decisions (Suri and Udry (2022)) and consistent with prospect theory (Kahneman and Tversky (1979), Prietzel (2020)) interpretations. In their study of index insurance products, Shin et al. (2022) provide evidence for a *certainty effect* which occurs when agents give greater weight to outcomes that are perceived as certain as opposed to those that are only possible. Although we cannot test it formally, our results are also consistent with the certainty effect in the context of agricutlural input markets. Entering into a forward contract with an input dealer who commits to provide quality inputs at a future date generates certainty, as opposed to the alternative of waiting until planting, when market conditions may or may not hold.

The experiment also distinguishes the effects of forward contracts with soft or hard commitment levels. Village compliance was much lower with harder commitments, even though farmer input demand effects were similar across both types of commitment deposit levels. From an ag-input dealers perspective, hard commitment contracts would not be profitable as the fixed costs of organizing the fair would be lost if whole villages are not willing to participate. From a scaling persective, an open question would be whether effective marketing might change village compliance rates with higher commitment. Ag-input dealers do prefer higher deposit rates as it increases their liquidity at a period of time when they are financing their stocks with larger suppliers in the supply chain.

Although VIF profitability is heterogeneous, our administrative data demonstrate that each VIF generated, on average, XOF 275,450 (equal to USD 525.31) in revenue for input dealers. The ITT effect show that farmers in villages where VIFs were implemented increased their demand for fertilizer by an average of XOF 50,780 (USD 96.84) compared to farmers in control villages. Based on farmers' fair participation, we can calculate a potential marginal revenue generation of XOF 577,144 (USD 1,100.7) for each VIF. These calculations provide suggestive evidence that, with VIFs, ag-input dealers are capturing potential returns that they would otherwise forego.

We also highlight the potential importance of trust in market creation, but are unable to identify the direct effect of trust in this experiment. In all treatment groups, a microfinance organization was present to collect deposits, even when their role during the fair was not to provide access to credit. By integrating a third-party financial intermediary, the microfinance organization provide some assurance to farmers and ag-input dealers that transactions are being monitored. Also, VIFs were organized in public, so transactions were more publicly observable by farmers and ag-input dealers, potentially increasing social pressure to respect the transaction terms. While we are not able to estimate these effects directly and they do not bias our results, they are informative of potential market making mechanisms and other avenues for future research.

Though VIFs did not have strong effects on household agricultural productivity, VIFs are a promising policy response to missing agricultural input markets and the promotion of last-mile adoption extension efforts. During the experiment, we estimated VIF costs at USD 100 per fair, suggesting that promoting input market development with VIFs is cost effective. We also highlight that the VIF model leverages private sector actors by providing ag-input dealers access to new potential markets, building the private sector rather than competing directly with it. We plan in future work to further investigate supply-side effects on profitability and business growth.

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Figures and Tables

Figure 1: Experimental Design and Timeline

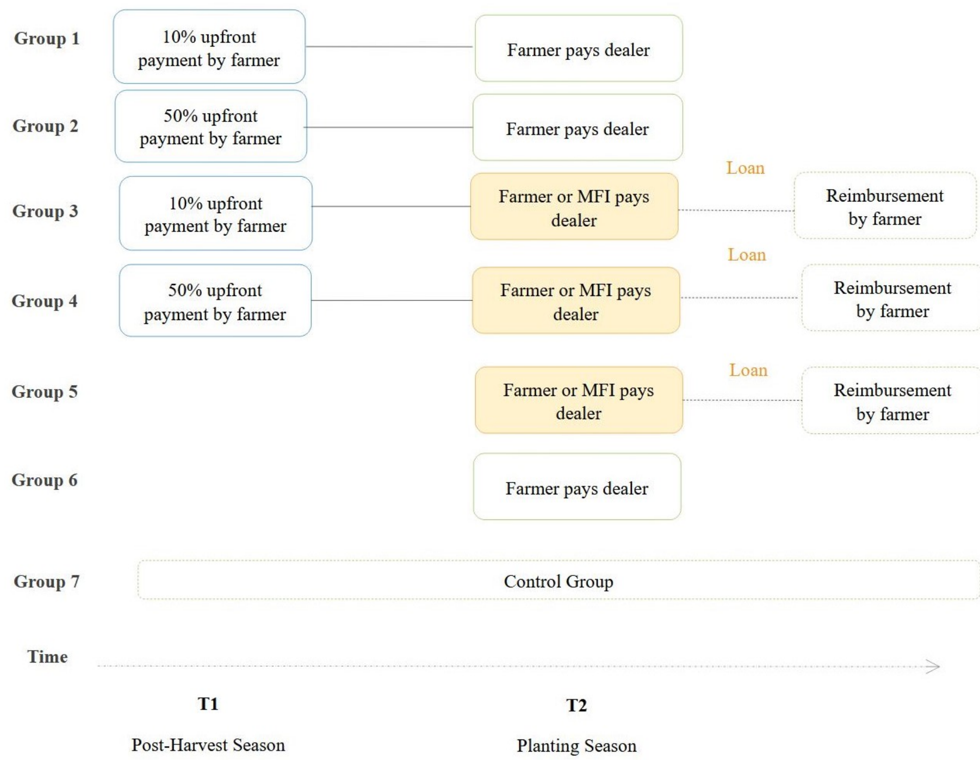

Table 1: Supply and Demand-side Compliance

	Village is aware of the input fair	**Percentage of villages with at least one purchase**	**Proportion of participants who have ordered inputs (unconditional)**	**Delivery value (% of order amount)**
	1	**2**	**3**	**4**
Post-Harvest Season				
10% Commitment	100.0%	80%	18.2%	70%
50% Commitment	95.0%	70%	23.0%	82%
10% Commitment + Credit	90.0%	70%	23.9%	95%
50% Commitment + Credit	90.0%	45%	21.9%	98%
Planting season				
Credit	100.0%	100%	53.2%	100%
Market Access	95.0%	100%	52.9%	100%
Total	95.0%	78%	33.2%	94%

Notes: Village-level summary statistics. Column 3 shows the proportion of participants who ordered inputs out of the total number of participants; village-level data is aggregated to the treatment group level regardless of village take-up, i.e. whether individual villages registered purchases or not.

Table 2: ITT Estimates on Market-level Outcomes

	Urea price	**DAP price**	**NPK price**	**Pesticide price**	**Farmer confidence in ag-dealers**
	1	**2**	**3**	**4**	**5**
Post-Harvest Season					
Group 1	-0	-3*	25	87	0.054**
10% Commitment	(2)	(2)	(18)	(325)	(0.023)
Group 2	0	-3	7	-201	0.004
50% Commitment	(2)	(3)	(19)	(253)	(0.025)
Group 3	0	0	18	-433	0.059**
10% Commitment + Credit	(3)	(2)	(13)	(276)	(0.024)
Group 4	-0	-4**	-4	-212	0.018
50% Commitment + Credit	(2)	(2)	(21)	(250)	(0.029)
Planting Season					
Group 5	2	-2	15	65	0.047**
Credit	(1)	(2)	(21)	(257)	(0.022)
Group 6	-3	-2	12	-32	0.040*
Market Access	(2)	(2)	(13)	(245)	(0.023)
Cons	230***	230***	257***	3,862***	0.899***
	(1)	(1)	(11)	(136)	(0.020)
Number of observations	254	254	254	254	2,543
i			$Ho: \beta^i = \beta^j$		
1					2
2					3, 5, 6
3				5	
4					
5	6				

Notes: Symbols ***,**,* denote significance at 1, 5, and 10 percent level, respectively. Uses village-level data, with the specification in equation (1). Columns 1-4 report average input unit prices in XOF by kilogram. Average exchange rate in 2017: 1000 XOF= $1.7126. Column 5 reports average responses to the question "Do you consider commercial activities to be a respectable job?" a question that was asked in a module about ag-dealers to solicit the respondent's trust or confidence in ag-dealers with whom the interacted with at the VIFs. The last panel of the regression table notes when a pairwise coefficent test was statistically significant. The rows represent the ith coefficent. The boxes indicate the number of the treatment group j whose coefficient is statistically different from group i (left column), based on F-tests for equality of coefficients. P-values for these tests are reported in the appendix.

Table 3: ITT Estimates on (Unconditional) Input Demand

		Total input value	**Total fertilizer value**	**Total pesticide value**	**Total urea value**	**Total DAP value**	**Total NPK value**
		1	**2**	**3**	**4**	**5**	**6**
Post-Harvest Season							
	Group 1	42,220	39,948*	7,738	12,530	31,782**	-2,625
10% Commitment		(25,935)	(22,237)	(9,406)	(8,105)	(15,362)	(5,349)
	Group 2	47,266*	41,441*	7,264	10,558	31,331**	-3,406
50% Commitment		(25,210)	(21,046)	(8,662)	(7,556)	(14,649)	(4,701)
	Group 3	56,425**	47,470**	16,470*	16,740**	27,019*	2,406
10% Commitment + Credit		(28,447)	(22,844)	(9,543)	(7,565)	(15,396)	(5,450)
	Group 4	50,219**	45,969**	8,806	14,849**	34,371**	-1,739
50% Commitment + Credit		(23,603)	(20,175)	(7,987)	(6,758)	(14,144)	(5,115)
Planting Season							
	Group 5	49,211**	38,577**	11,736	11,772*	25,643*	-2,536
Credit		(23,552)	(19,314)	(8,804)	(6,532)	(15,154)	(5,303)
	Group 6	1,051	5,822	-3,487	2,912	11,318	-6,322
Market Access		(24,405)	(21,052)	(8,534)	(7,239)	(14,959)	(4,700)
Baseline control mean		216,016	167,750	55,990	59,844	97,502	2,483
		(210,039)	(173,646)	(61,129)	(59,984)	(107,691)	(10,876)
Number of observations		4,961	4,794	4,667	4,753	4,749	4,912
i				$Ho : \beta^i = \beta^j$			
1							
2		6					
3		6		6			
4		6	6				
5		6		6			

Notes: Symbols ***,**,* denote significance at 1, 5 and 10 percent level, respectively. Difference in difference specification (2017-2018, equation (2)). Fertilizer: Urea, DAP, NPK, other chemical. Pesticide: Insecticide, fongicide, herbicide. Input: fertilizer + pesticide. The estimations used robust standard errors clustered at the village level and added these controls: household land size quartiles, median prices of DAP, pesticides, NPK, and other chemicals at the village level. All outcome variable values in XOF. Average exchange rate in 2017: 1000 XOF= \$1.7126. The last panel of the regression table notes when a pairwise coefficent test was statistically significant. The rows represent the ith coefficent. The boxes indicate the number of the treatment group j whose coefficient is statistically different from group i (left column), based on F-tests for equality of coefficients. P-values for these tests are reported in the appendix.

Table 4: ITT Estimates on Input Adoption

	Input use	**Fertilizer use**	**Pesticide use**	**Urea use**	**DAP use**	**NPK use**
	1	**2**	**3**	**4**	**5**	**6**
Post-Harvest Season						
Group 1	0.107	0.137**	0.135*	0.139*	0.066	0.108
10% Commitment	(0.065)	(0.068)	(0.077)	(0.077)	(0.049)	(0.092)
Group 2	0.054	0.099	0.083	0.089	0.012	0.079
50% Commitment	(0.068)	(0.061)	(0.086)	(0.069)	(0.050)	(0.081)
Group 3	0.122	0.126*	0.138	0.152**	0.078	0.097
10% Commitment + Credit	(0.077)	(0.073)	(0.090)	(0.075)	(0.057)	(0.085)
Group 4	0.093	0.096*	0.138*	0.144**	-0.020	0.099
50% Commitment + Credit	(0.057)	(0.053)	(0.071)	(0.059)	(0.058)	(0.067)
Planting Season						
Group 5	0.137*	0.117*	0.164*	0.128**	0.036	0.062
Credit	(0.071)	(0.062)	(0.085)	(0.064)	(0.051)	(0.079)
Group 6	0.057	0.047	0.090	0.057	-0.039	0.042
Market Access	(0.061)	(0.057)	(0.076)	(0.067)	(0.050)	(0.079)
Baseline control mean	0.910	0.851	0.866	0.821	0.123	0.746
	(0.286)	(0.357)	(0.342)	(0.384)	(0.329)	(0.436)
Number of observations	4,961	4,961	4,961	4,961	4,961	4,961
i			$Ho: \beta^i = \beta^j$			
1					6	
2						
3					6	
4						
5						

Notes: Symbols ***,**,* denote significance at 1, 5 and 10 percent level, respectively. Diffence in difference specification (2017-2018, equation (2)). Fertilizer: Urea, DAP, NPK, other chemical. Pesticide: Insecticide, fongicide, herbicide. Input: fertilzer, pesticide. Input use, Fertilizer use and Pesticide use outcomes are defined as follows: they take the value 1 when at least one plot managed by the household is using respectively an input, a fertilizer, or a pesticide, and 0 otherwise. The estimations used robust standard errors clustered at the village level and added these controls: household land size quartiles, median prices of DAP, pesticides, NPK and other chemical at the village level. The last panel of the regression table notes when a pairwise coefficent test was statistically significant. The rows represent the ith coefficent. The boxes indicate the number of the treatment group j whose coefficient is statistically different from group i (left column), based on F-tests for equality of coefficients. P-values for these tests are reported in the appendix.

Table 5: ITT Estimates on Crop Choice: Share of Plots with Given Crop Listed as Main Crop

	Peanut	**Cotton**	**Fonio**	**Gombo**	**Maize**	**Millet**	**Rice**	**Sesame**	**Sorghum**
	1	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**
Post-Harvest Season									
Group 1	0.069	-0.128**	0.054	-0.089***	-0.033	0.118	-0.062	0.036	0.032
10% Commitment	(0.072)	(0.063)	(0.036)	(0.029)	(0.046)	(0.090)	(0.085)	(0.024)	(0.035)
Group 2	-0.013	-0.011	0.002	-0.044	0.006	0.081	0.011	0.002	-0.008
50% Commitment	(0.073)	(0.059)	(0.020)	(0.034)	(0.070)	(0.133)	(0.073)	(0.021)	(0.040)
Group 3	0.031	-0.031	0.026	-0.059*	0.025	0.084	-0.086	0.056	-0.043
10% Commitment + Credit	(0.060)	(0.055)	(0.030)	(0.034)	(0.045)	(0.086)	(0.090)	(0.043)	(0.048)
Group 4	-0.032	-0.053	-0.006	-0.036	0.042	0.029	-0.015	0.011	0.022
50% Commitment + Credit	(0.057)	(0.069)	(0.021)	(0.027)	(0.056)	(0.095)	(0.070)	(0.031)	(0.042)
Planting Season									
Group 5	0.114*	-0.066	0.010	-0.064**	0.033	-0.009	-0.049	0.024	-0.008
Credit	(0.058)	(0.060)	(0.020)	(0.031)	(0.045)	(0.102)	(0.073)	(0.033)	(0.040)
Group 6	-0.012	-0.108*	0.019	-0.039	-0.017	0.125	0.030	0.052	0.009
Market Access	(0.053)	(0.055)	(0.023)	(0.039)	(0.049)	(0.086)	(0.063)	(0.033)	(0.038)
Endline control mean	0.198	0.128	0.012	0.037	0.166	0.126	0.120	0.021	0.080
	(0.190)	(0.139)	(0.057)	(0.103)	(0.165)	(0.208)	(0.176)	(0.087)	(0.122)
Number of observations	2,537	2,537	2,537	2,537	2,537	2,537	2,537	2,537	2,537

Notes: Symbols ***,**,* denote significance at 1, 5 and 10 percent level, respectively. Cross-sectional analysis in 2018 (equation 3) with interaction between treatment and gender. All estimations are at the household level. The estimations use robust standard errors clustered at the village level.

Table 6: ITT estimation on labor demand

	Household labor (days)			Hired labor (days)		
	Planting	**Weeding**	**Harvest**	**Planting**	**Weeding**	**Harvest**
	1	**2**	**3**	**4**	**5**	**6**
Post-Harvest Season						
Group 1	25**	36**	16	1	7***	-1
10% Commitment	(11)	(15)	(12)	(1)	(2)	(3)
Group 2	34***	13	8	-1	3	-0
50% Commitment	(13)	(13)	(14)	(1)	(2)	(4)
Group 3	23	30*	7	0	3	-1
Credit + 10% Commitment	(14)	(18)	(11)	(1)	(3)	(3)
Group 4	22	23	6	-0	1	-1
Credit + 50% Commitment	(13)	(20)	(14)	(1)	(2)	(3)
Planting Season						
Group 5	30**	33**	22*	-0	3	-1
Credit	(13)	(15)	(12)	(1)	(3)	(3)
Group 6	17	10	8	1	3	0
Market Access	(11)	(15)	(12)	(1)	(2)	(3)
Endline control mean	94	121	106	3	6	15
	(85)	(120)	(114)	(9)	(14)	(32)
Number of observations	2,543	2,543	2,543	2,543	2,543	2,543
i		$Ho : \beta^i = \beta^j$				
1				2	4	
2						
3						
4						
5						

Notes: Symbols ***,**,* denote significance at 1, 5 and 10 percent level, respectively. Cross sectional specification (2018, equation (3)).The last panel of the regression table notes when a pairwise coefficent test was statistically significant. The rows represent the ith coefficent. The boxes indicate the number of the treatment group j whose coefficient is statistically different from group i (left column), based on F-tests for equality of coefficients. P-values for these tests are reported in the appendix.

Table 7: ITT Estimates on Production Value and Marketed Surplus

		Total value of agricultural production	Marketed surplus
		1	2
Post-Harvest Season			
	Group 1	36,634	79
10% Commitment		(42,556)	(139)
	Group 2	52,068	203
50% Commitment		(48,882)	(140)
	Group 3	61,794	179*
10% Commitment + Credit		(37,490)	(106)
	Group 4	39,715	145
50% Commitment + Credit		(39,608)	(129)
Planting Season			
	Group 5	20,905	52
Credit		(36,893)	(124)
	Group 6	-2,118	67
Market Access		(40,068)	(126)
Endline control mean		353,125	983
		(278,868)	(1,104)
Number of observations		2,332	2,325
i		$Ho: \beta^i = \beta^j$	
1			
2			
3			
4			
5			

Notes: Symbols ***,**,* denote significance at 1, 5 and 10 percent level, respectively. Cross sectional specification (2018, equation (3)). The estimations used robust standard errors clustered at the village level and added these controls: household land size quartiles, median prices of DAP, pesticides, NPK and other chemical at the village level. All outcome variables in XOF. Average exchange rate in 2017: 1000 XOF= $1.7126. The last panel of the regression table notes when a pairwise coefficent test was statistically significant. The rows represent the ith coefficent. The boxes indicate the number of the treatment group j whose coefficient is statistically different from group i (left column), based on F-tests for equality of coefficients. P-values for these tests are reported in the appendix.

Appendix

Making Markets: Experiments in Agricultural Input Market Formation

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This appendix contains four parts. Appendix A shows balance tables between treatment groups on household demographics and asset ownership. Appendix B reports p-values of tests for difference between treatment coefficients in ITT estimates. Appendix C includes ToT estimates on production, sales input usage, labor, and crop choice. Finally, Appendix D reports the number of ITT hypotheses tested, as well as the share of significant hypotheses.

A Appendix: Balance tables

Table A1: Household Agricultural and Demographic Descriptive Statistics

		Total Plot Area	Average Plot Size	Number of Plots	Household Size
		1	2	3	4
Post-Harvest Season					
	Group 1	7.03	1.36	6	4
10% Commitment		(7.29)	(1.29)	(5)	(3)
	Group 2	7.88	1.20	7	5
50% Commitment		(7.56)	(1.02)	(5)	(4)
	Group 3	6.74	1.30	5	4
10% Commitment + Credit		(6.90)	(1.11)	(4)	(3)
	Group 4	6.57	1.26	5	4
50% Commitment + Credit		(6.78)	(1.15)	(4)	(3)
Planting Season					
	Group 5	8.03	1.61	5	4
Credit		(8.33)	(1.42)	(5)	(3)
	Group 6	7.70	1.41	6	4
Market Access		(7.54)	(1.15)	(5)	(3)
T-Tests and Mean Differences Relative to the Control Group					
10% Commitment		-1.385***	-0.014	-0.737***	-0.249*
50% Commitment		-0.539	-0.167***	0.067	0.429***
10% Commitment + Credit		-1.677***	-0.071	-1.410***	-0.135
50% Commitment + Credit		-1.843***	-0.113**	-1.201***	-0.200
Credit		-0.387	0.236***	-1.035***	0.073
Market Access		-0.718*	0.036	-0.773***	-0.027

Notes: Symbols ***,**,* denote significance at 1, 5 and 10 percent level, respectively.

Table A2: Average Value (in CFA) of a Given Asset

	Telephone	**Bed**	**Radio**	**Bike**	**TV**	**VCR**	**Lounge**
	1	**2**	**3**	**4**	**5**	**6**	**7**
Post-Harvest Season							
Group 1	12,662	1,718	2,737	16,858	6,962	1,160	502
10% Commitment	(10,597)	(5,394)	(2,739)	(15,091)	(14,618)	(3,498)	(3,787)
Group 2	14,637	2,800	3,378	17,679	8,083	1,342	362
50% Commitment	(13,309)	(7,884)	(3,479)	(19,710)	(16,924)	(4,084)	(3,347)
Group 3	14,109	2,640	2,585	17,846	6,103	1,120	505
10% Commitment + Credit	(12,915)	(7,074)	(2,643)	(17,609)	(14,126)	(3,564)	(4,096)
Group 4	14,154	2,977	2,761	14,737	6,158	914	519
50% Commitment + Credit	(12,221)	(7,423)	(2,986)	(18,295)	(13,875)	(3,294)	(4,941)
Planting Season							
Group 5	13,062	1,938	2,307	14,050	5,914	909	312
Credit	(12,594)	(6,122)	(2,595)	(16,266)	(14,064)	(3,516)	(3,049)
Group 6	13,603	2,127	2,807	15,369	9,102	1,616	180
Market Access	(11,979)	(5,674)	(2,755)	(16,490)	(16,236)	(4,165)	(1,491)
T-Tests and Mean Differences Relative to the Control Group							
10% Commitment	-1,538***	-766***	-199	-1,752**	-334	-105	190
50% Commitment	437	316	442***	-931	787	77	49
10% Commitment + Credit	-91	156	-352***	-765	-1,192*	-146	193
50% Commitment + Credit	-46	492	-176	-3,873***	-1,138*	-351**	207
Credit	-1,138*	-546*	-629***	-4,560***	-1,381**	-356**	-1
Market Access	-596	-358	-130	-3,242***	1,807**	351*	-132

Notes: Symbols ***,**,* denote significance at 1, 5 and 10 percent level, respectively.

B Appendix: P-values of tests for difference between treatment coefficients for tables 2-7

Table B1: P-values of tests for difference between treatment coefficients table 2

	Urea price	DAP price	NPK price	Pesticide price	Farmer confidence in ag-dealers
	1	2	3	4	5
$\beta^1 = \beta^2$	0.963	0.990	0.376	0.336	0.005
$\beta^1 = \beta^3$	0.879	0.134	0.661	0.105	0.758
$\beta^1 = \beta^4$	0.984	0.578	0.200	0.315	0.133
$\beta^1 = \beta^5$	0.242	0.746	0.660	0.943	0.626
$\beta^1 = \beta^6$	0.188	0.753	0.393	0.684	0.366
$\beta^2 = \beta^3$	0.899	0.335	0.504	0.345	0.003
$\beta^2 = \beta^4$	0.956	0.710	0.649	0.960	0.580
$\beta^2 = \beta^5$	0.196	0.822	0.726	0.233	0.011
$\beta^2 = \beta^6$	0.143	0.851	0.784	0.420	0.045
$\beta^3 = \beta^4$	0.884	0.081	0.254	0.363	0.096
$\beta^3 = \beta^5$	0.585	0.425	0.876	0.047	0.435
$\beta^3 = \beta^6$	0.275	0.293	0.537	0.093	0.250
$\beta^4 = \beta^5$	0.414	0.490	0.455	0.209	0.210
$\beta^4 = \beta^6$	0.314	0.452	0.428	0.384	0.360
$\beta^5 = \beta^6$	0.009	0.942	0.854	0.650	0.619

Notes: F-test of equivalence $\beta^i = \beta^j$.

Table B2: P-values of tests for difference between treatment coefficients for table 3

	Total input value	**Total fertilizer value**	**Total pesticide value**	**Total urea value**	**Total DAP value**	**Total NPK value**
	1	**2**	**3**	**4**	**5**	**6**
$\beta^1 = \beta^2$	0.842	0.946	0.957	0.819	0.975	0.869
$\beta^1 = \beta^3$	0.615	0.752	0.355	0.618	0.759	0.379
$\beta^1 = \beta^4$	0.724	0.768	0.893	0.756	0.850	0.864
$\beta^1 = \beta^5$	0.754	0.944	0.640	0.918	0.671	0.987
$\beta^1 = \beta^6$	0.085	0.113	0.184	0.227	0.169	0.433
$\beta^2 = \beta^3$	0.704	0.757	0.249	0.359	0.741	0.242
$\beta^2 = \beta^4$	0.888	0.803	0.819	0.526	0.806	0.711
$\beta^2 = \beta^5$	0.923	0.864	0.549	0.845	0.665	0.853
$\beta^2 = \beta^6$	0.041	0.076	0.151	0.298	0.145	0.478
$\beta^3 = \beta^4$	0.791	0.937	0.309	0.767	0.571	0.433
$\beta^3 = \beta^5$	0.740	0.599	0.557	0.364	0.918	0.367
$\beta^3 = \beta^6$	0.029	0.052	0.016	0.053	0.268	0.085
$\beta^4 = \beta^5$	0.954	0.619	0.656	0.546	0.486	0.874
$\beta^4 = \beta^6$	0.019	0.036	0.069	0.068	0.081	0.316
$\beta^5 = \beta^6$	0.016	0.065	0.041	0.148	0.302	0.430

Notes: F-test of equivalence $\beta^i = \beta^j$.

Table B3: P-values of tests for difference between treatment coefficients for table 4

	Input use	**Fertilizer use**	**Pesticide use**	**Urea use**	**NPK use**	**DAP use**
	1	**2**	**3**	**4**	**5**	**6**
$\beta^1 = \beta^2$	0.426	0.583	0.532	0.537	0.197	0.749
$\beta^1 = \beta^3$	0.845	0.892	0.970	0.877	0.814	0.906
$\beta^1 = \beta^4$	0.787	0.485	0.957	0.940	0.101	0.908
$\beta^1 = \beta^5$	0.634	0.757	0.701	0.875	0.504	0.593
$\beta^1 = \beta^6$	0.373	0.164	0.514	0.279	0.013	0.456
$\beta^2 = \beta^3$	0.318	0.700	0.490	0.347	0.209	0.811
$\beta^2 = \beta^4$	0.468	0.953	0.436	0.362	0.537	0.741
$\beta^2 = \beta^5$	0.191	0.764	0.308	0.518	0.597	0.815
$\beta^2 = \beta^6$	0.949	0.356	0.918	0.625	0.250	0.627
$\beta^3 = \beta^4$	0.665	0.653	0.999	0.899	0.100	0.970
$\beta^3 = \beta^5$	0.821	0.891	0.754	0.696	0.433	0.618
$\beta^3 = \beta^6$	0.323	0.243	0.533	0.176	0.029	0.481
$\beta^4 = \beta^5$	0.420	0.675	0.704	0.740	0.295	0.505
$\beta^4 = \beta^6$	0.431	0.308	0.416	0.121	0.728	0.341
$\beta^5 = \beta^6$	0.162	0.200	0.299	0.227	0.109	0.782

Notes: F-test of equivalence $\beta^i = \beta^j$.

Table B4: P-values of tests for difference between treatment coefficients for table 7

	Total value of agricultural production	Marketed surplus
	1	**2**
$\beta^1 = \beta^2$	0.747	0.376
$\beta^1 = \beta^3$	0.535	0.375
$\beta^1 = \beta^4$	0.935	0.597
$\beta^1 = \beta^5$	0.664	0.827
$\beta^1 = \beta^6$	0.313	0.924
$\beta^2 = \beta^3$	0.826	0.842
$\beta^2 = \beta^4$	0.791	0.673
$\beta^2 = \beta^5$	0.478	0.265
$\beta^2 = \beta^6$	0.240	0.313
$\beta^3 = \beta^4$	0.534	0.741
$\beta^3 = \beta^5$	0.191	0.205
$\beta^3 = \beta^6$	0.075	0.277
$\beta^4 = \beta^5$	0.568	0.411
$\beta^4 = \beta^6$	0.256	0.497
$\beta^5 = \beta^6$	0.510	0.893

Notes: F-test of equivalence $\beta^i = \beta^j$.

C Appendix: Treatment on the treated estimates

Table C1: ToT estimates - Crop Choice (Instrument: Treatment indicator)

	Peanut	**Cotton**	**Fonio**	**Gombo**	**Maize**	**Millet**	**Rice**	**Sesame**	**Sorghum**
	1	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**
Purchase at VIF	0.126*	-0.004	0.036	-0.006	-0.078	-0.168***	0.187***	0.035	-0.047
	(0.075)	(0.027)	(0.025)	(0.025)	(0.047)	(0.063)	(0.063)	(0.040)	(0.043)
Endline Control Group Mean	0.198***	0.128***	0.012*	0.037***	0.166***	0.126***	0.120***	0.021**	0.080***
	(0.026)	(0.014)	(0.006)	(0.008)	(0.016)	(0.035)	(0.023)	(0.009)	(0.014)
F-test (1st stage)									
Kleibergen-Paap F-stat	82.949	82.949	82.949	82.949	82.949	82.949	82.949	82.949	82.949
Number of observations	1,795	1,795	1,795	1,795	1,795	1,795	1,795	1,795	1,795

Notes: Symbols ***,**,* denote significance at 1, 5, and 10 percent levels, respectively. Purchase at VIF is an indicator for households with at least one purchase at a VIF. A binary treatment indicator is used as instrument for Purchase at VIF. Sample excludes treatment group 6. Robust standard errors clustered at the village level are in parentheses. Estimations used the following controls: Input median prices by village (urea, DAP, NPK, pesticide); Household land size quartiles; Soil quality and soil type indicators; Household size; Baseline input use binary indicators (any, fertilizer, pesticide, herbicide, NPK). All outcome variable values are household-level share of plots with the listed crop as main crop.

Table C2: ToT estimates - Labor demand (Instrument: Treatment indicator)

	Household labor (days)			Hired labor (days)		
	Planting	**Weeding**	**Harvest**	**Planting**	**Weeding**	**Harvest**
	1	**2**	**3**	**4**	**5**	**6**
Purchase at VIF	71.213**	71.560**	39.298*	-0.493	11.208**	6.488
	(27.663)	(33.853)	(23.639)	(2.686)	(4.620)	(8.981)
Endline Control Group Mean	94.182***	120.630***	106.261***	2.903***	6.183***	14.653***
	(6.373)	(8.920)	(7.490)	(0.699)	(1.219)	(2.667)
F-test (1st stage)						
Kleibergen-Paap F-stat	82.715	82.715	82.715	82.715	82.715	82.715
Number of observations	1,799	1,799	1,799	1,799	1,799	1,799

Notes: Symbols ***,**,* denote significance at 1, 5, and 10 percent levels, respectively. Purchase at VIF is an indicator for households with at least one purchase at a VIF. A binary treatment indicator is used as instrument for Purchase at VIF. Sample excludes treatment group 6. Robust standard errors clustered at the village level are in parentheses. Estimations used the following controls: Input median prices by village (urea, DAP, NPK, pesticide); Household land size quartiles; Soil quality and soil type indicators; Household size; Baseline input use binary indicators (any, fertilizer, pesticide, herbicide, NPK). All outcome variable values are in labor days.

Table C3: ToT estimates - Production, Sales, and Input value (Instrument: Treatment indicator)

	Production Value	**Sales Value**	**Total Input Value**	**Fertilizer Value**	**Pesticide Value**	**Urea Value**	**DAP Value**	**NPK Value**
	1	**2**	**3**	**4**	**5**	**6**	**7**	**8**
Purchase at VIF	183,729*	65,280**	91,706*	84,275*	20,779	20,281	68,955**	4,357
	(108,735)	(32,843)	(48,892)	(44,901)	(14,949)	(17,327)	(27,824)	(12,408)
Endline Control Group Mean	353,125***	108,289***	217,624***	169,475***	56,145***	63,292***	79,703***	13,425***
	(31,118)	(11,278)	(22,495)	(18,612)	(6,945)	(6,973)	(10,402)	(4,081)
F-test (1st stage)								
Kleibergen-Paap F-stat	81.037	79.99	82.715	78.286	78.361	80.491	77.711	80.991
Number of observations	1,733	1,721	1,799	1,753	1,704	1,742	1,731	1,779

Notes: Symbols ***,**,* denote significance at 1, 5, and 10 percent levels, respectively. Purchase at VIF is an indicator for households with at least one purchase at a VIF. A binary treatment indicator is used as instrument for Purchase at VIF. Sample excludes treatment group 6. Robust standard errors clustered at the village level are in parentheses. Estimations used the following controls: Input median prices by village (urea, DAP, NPK, pesticide); Household land size quartiles; Soil quality and soil type indicators; Household size; Baseline input use binary indicators (any, fertilizer, pesticide, herbicide, NPK). All outcome values are in XOF.

Table C4: ToT estimates - Inputs used per Ha (Instrument: Treatment indicator)

	Value per Ha						Quantity per ha			
	Total input	**Fertilizer**	**Pesticide**	**Urea**	**DAP**	**NPK**	**Pesticide**	**Urea**	**DAP**	**NPK**
	1	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**
Purchase at VIF	5,330	2,607	346	1,607	1,533	97	0.085	2.989	8.482	-1.270
	(5,940)	(4,505)	(692)	(1,823)	(3,216)	(2,045)	(0.171)	(8.397)	(13.230)	(6.344)
Endline Control Group Mean	31,451***	22,825***	2,431***	8,354***	11,927***	2,063***	0.546***	36.266***	50.675***	7.084***
	(3,029)	(2,217)	(303)	(835)	(1,380)	(576)	(0.070)	(3.775)	(5.719)	(1.945)
F-test (1st stage)										
Kleibergen-Paap F-stat	82.076	82.232	80.601	81.274	82.437	82.409	78.688	81.62	82.726	81.657
Number of observations	1,751	1,755	1,725	1,753	1,765	1,797	1,683	1,739	1,744	1,764

Notes: Symbols ***,**,* denote significance at 1, 5, and 10 percent levels, respectively. Purchase at VIF is an indicator for households with at least one purchase at a VIF. A binary treatment indicator is used as instrument for Purchase at VIF. Sample excludes treatment group 6. Robust standard errors clustered at the village level are in parentheses. Estimations used the following controls: Input median prices by village (urea, DAP, NPK, pesticide); Household land size quartiles; Soil quality and soil type indicators; Household size; Baseline input use binary indicators (any, fertilizer, pesticide, herbicide, NPK). All Values per Ha are in XOF, Pesticide quantity is in liters, Urea/DAP/NPK in Kgs.

D Appendix: Hypothesis

Table D1: Number of ITT hypotheses tested

	T1-C	T2-C	T3-C	T4-C	T5-C	T6-C	T_i-C	T_i-T_j
# of hypothesis tested	34	34	34	34	34	34	204	375
# of hypothesis significant at 10%	12	4	11	8	14	2	51	17
% significant hypothesis	35.29%	11.76%	32.35%	23.53%	41.18%	5.88%	25.00%	4.53%

Notes: Table D1 tracks the number of ITT hypotheses tested, the number of null hypotheses rejected at 10% and the share of significant hypotheses at 10%. We highlight with a graded color scale the share of hypothesis above 10% (in green) and the share of hypothesis below 10% in red.