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Access to Digital Credit for Smallholder Farmers: Experimental Evidence from Ghana

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Abstract

Digital finance in agriculture is a nascent technology which could help improve rural financial inclusion. In an experimental evaluation of a digital lending product for farmers in Southern Ghana, credit increases farm investments but has few statistically significant average effects on downstream outcomes. However, logistical challenges generated imperfect compliance with the treatment assignment, with some loans delivered in a timely fashion for agricultural investments and others coming later. The researchers cautiously exploit this unplanned non-experimental implementation heterogeneity and conclude that agriculturally-focused digital credit platforms have potential to tackle persistent rural financial market imperfections, but the timing seems critical and deserves further study.

1. Introduction

Financial market imperfections pose major barriers to low-income farmers throughout developing countries. Such farmers typically lack scorable credit histories and collateral, two common tools used by lenders to overcome information asymmetries. Farmers can be left with unsatisfactory choices, such as underinvesting in their farms or non-farm enterprises or borrowing at particularly high interest rates (Banerjee, 2013). Furthermore, when entire communities need financing simultaneously, as is often the case in rain-fed agricultural economies, informal networks are less effective than they otherwise would be for satisfying farmer credit demand. Lastly, while in theory savings markets could succeed in delivering timely liquidity for farmers, savings poses its own set of market failures from social, institutional, and behavioral barriers (Karlan, Ratan, and Zinman, 2014; Afzal et. al., 2018; Kremer, Rao, and Schilbach, 2019).

Making the problem more poignant, the returns to agricultural investment can be high (Beaman et. al., 2023). In West Africa, farmers identify lack of capital as a primary reason for underinvestment and their inability to realize high returns (Fosu and Dittoh, 2011). Farmers typically face capital constraints at planting, when investment is needed yet cash is low (because the last harvest was months prior). Relaxing capital constraints could generate substantial welfare gains through agricultural investment and its ensuing income, yet such gains may depend critically on timing and magnitude (Brune et. al., 2011; Karlan, Osei. et. al., 2014; Casaburi and Willis, 2018).

We examine this by conducting a randomized controlled trial in which a lender offers credit (or not) to Ghanaian smallholder farmers who applied for loans through a digitized process, thus incorporating a nascent but promising technological innovation that likely lowers transaction costs for both lenders and borrowers.

We partnered with Farmerline, a digital lender and farm input provider. Farmers applied to receive farm inputs on credit from Farmerline from a cellphone or on an app, and could receive technical support for the application process from a local Farmerline agent. Farmerline processed applications using a non-traditional credit-scoring algorithm designed to generate quick results; 62% of applicants were deemed eligible for a loan. We then randomly assigned the eligible applicants to either treatment or control. The remainder of the lending process, up to the delivery of the farm inputs, was also digital and no human interaction was required with the credit providers (farmers received notifications about the status of their loans on their phones). Prior to treatment assignment Farmerline informed farmers that, conditional on their credit application being successful, farm inputs would be delivered to their farmgate within 30 days of their application.

Ensuring affordable access to credit for poor borrowers has long been a key financial inclusion goal for researchers and policymakers (Banerjee, 2013). Yet, empirical evidence on the economic impacts of traditional microcredit programs have, at best, been mixed (Banerjee, Karlan, and Zinman, 2015; Meager, 2019). Some evidence suggests, however, that innovative lending models could be more transformative (Field et. al., 2013; Fink, Jack, and Masiye, 2020; Beaman et. al., 2023).

Aside from contributing to a broad literature on the impact of improved credit access for low income rural households, the loans tested here differ in three important and more nascent dimensions with respect to the literature: the loans are digitally-offered, are in-kind via agricultural inputs, and have disbursal and repayment terms designed around the agricultural season.

Some research has documented the promise of digital finance in promoting financial inclusion (Karlan, Kendall, et al., 2016), and related work has highlighted how digital credit, in particular, has significant potential over traditional models of lending to the poor (Blumenstock, Francis, and Robinson, 2017; Björkegren and Grissen, 2018; Suri, Bharadwaj, and Jack, 2021). Traditional non-digital lenders typically incur high costs, relative to the size of the loan, to reach poor borrowers. And borrowers incur time costs, with group meetings in more traditional group-based microcredit programs, or travel time to distant bank or microcredit institution branch visits in individual but non-digital based lending models. Farmerline's digital model aims to overcome these barriers. Lender costs are low as their underwriting process requires minimal official data and in-person marketing and vetting. Similarly, borrowers spend much less time or money filing paperwork, visiting branches, and participating in group meetings.

The loan proceeds are also paid in-kind via agricultural inputs, and are thus less fungible than a cash loan. This likely leads to more expenditure on farm inputs than a traditional micro-lending product would generate. Previous research has noted that the limited observable impact of traditional microcredit on entrepreneurial outcomes could be because people borrow to increase their consumption, and not to expand their enterprises (Banerjee, 2013; Banerjee, Karlan, and Zinman, 2015; for counter-evidence, see Karlan, Osman, and Zinman, 2016).

Lastly, our loan product gives farmers a three-month period between when they receive their loans and when they start the repayment process. Classic microcredit models often involve small, frequent repayments beginning immediately after the recipient receives the loan. Such setups can limit investment in enterprises which may have high but illiquid returns. Previous work has shown how a two-month grace period before repayment increased the short-run investment and long-run profits for small firms (Field et. al., 2013). In agriculture, this may be particularly salient as farmers face liquidity constraints when they need it most during planting season and receive lumpy cash incomes only after crops are sold after harvest (Casaburi and Willis, 2018).

We have three main findings. First, using an intent-to-treat (ITT) specification, we find an average increase in farm input expenditures and amount of land dedicated to mixed-cropping. But we do not find any statistically significant impact on final outcomes such as value of crops produced, sales, or profits. The point estimates are negative, and we can rule out large positive effects—for example, a larger than 15% increase over the baseline mean in crop profits due to the treatment—on these downstream final outcomes. While several theories may shed light on why this might have happened, a plausibly compelling argument pertains to the lack of timely disbursement of credit (in the form of farm inputs) as crop productivity is likely to be impeded by the delayed application of essential farm inputs like fertilizers and insecticides. This theory was also put forward in ex-post focus group discussions with farmers in which they specifically attributed the lack of impacts to the delayed disbursement of loans.

Second, we explore non-experimental variation in the timeliness with which loans were disbursed. Compliance was lower than intended with respect to timing of loans (discussed more below), and this creates a window to explore, with caveats, the importance of the timing of the loan disbursal with respect to the agricultural planting season. Farmers who received a timely loan have much larger crop production and sales than the control group. These effects are large in magnitude, over 29% of the baseline mean. Farm profits also increase substantially, albeit imprecisely (p=0.20). However, while the lower bound of the 95% confidence interval allows us to rule out profit decreases greater than 17% of the baseline mean, the upper bound can only rule out profit increases larger than 79% of the baseline mean. We find no evidence of heterogeneous impact—through an ITT approach—on overall farm input expenditures or mixed-cropping acreages for farmers who received timely loans. But we find a statistically significant increase in spending on fertilizers, which is one of the key inputs provided on credit by Farmerline. Additionally, farmers who received timely loans relied less on the informal credit market. Taken together, these results suggest that the provision of inputs on credit may have had tangible, positive effects on financial outcomes, for those that received a timely delivery of the loans.

Third, focusing on gender differences (one of the two dimensions of heterogeneity registered in our pre-analysis plan)[1], female farmers in the treatment group spent less on farm inputs than control, whereas male farmers spent substantially more than control. While treatment induces male farmers to increase the amount of crops they grow, female farmers instead reduce crop diversification. Consistent with this gender differential, treatment also reduces the non-farm business income of male farmers, while treatment leads female farmers to invest more and earn more in non-farm enterprises. In net, treatment led male farmers to invest more in farms, but led female farmers to divest from farms and invest more in non-farm enterprises.

Although it was not the intention of the research design, we document an important lesson on the importance of timely access to loans for smallholder farmers. New technologies like digital credit platforms can tackle persistent issues of rural financial market imperfections, but the successful logistics of loan and input delivery remains essential. Our research, thus, adds to the emerging literature on digital finance, which is rapidly evolving as a promising improvement to traditional microcredit models in developing countries (Blumenstock, Francis, and Robinson, 2017; Karlan, Kendall, et al., 2016).

[1] The other is farming experience, and we find little evidence of heterogeneity (Appendix Table A1).

2. Background of Our Experiment

2A. Contextual Details

Our research takes place in Ghana's Ashanti region. Like many other low-income countries, a relatively high share of Ghana's labor force – about 52% – work in farms. 85% of farms are smaller than five hectares. The Ashanti region is a major producer of cocoa—Ghana is the world's second largest producer (FAO, 2017)—as well as rice and vegetables.

Ghana provides an ideal setting to study the effects of credit in agriculture. Despite the large population involved in agriculture, farmers in remote regions of Ghana face imperfect financial markets. Only about 40% of adults in Ghana have a bank account (World Bank, 2022). Moreover, while microcredit providers have not expanded across West Africa as much as in other parts of the world, mobile phone usage in Ghana has rapidly grown, with at least 84% of the population owning a mobile phone, and up to 120 mobile phone subscriptions per 100 (World Bank, 2022). Ghana's low bank branch penetration (6.1 per 100,000 adults) along with the rapid growth of mobile suggests a ripe frontier for digital finance operations.

2B. The Experimental Intervention and the Credit Product

We partner with Farmerline, a social enterprise that supports the entrepreneurial efforts of small-scale farmers. Farmerline is active in 13 African countries and has developed *Mergdata,* a web and mobile application which contains several software modules for services such as weather forecasts, market prices, and farming tips. Mergdata allows farmers to apply for farm inputs on credit. Importantly, the entire application process is completed digitally and does not require farmers to physically travel to distant bank branches or participate in group meetings.[2] Farmerline's proprietary credit scoring algorithm calculates farmers' creditworthiness using non-traditional data including farm characteristics, production history, and crop sales.[3] Farmers are informed about the application's outcome within 2-3 days following which the input disbursement process begins. Farmers are notified that the entire process, from submission of the application to receipt of the farm inputs, is designed to be completed within 30 days.

We sampled small-scale farmers from the population of farmers who: (i) had previously registered with Farmerline and used at least one of its services, (ii) cultivated cocoa, vegetables, or rice as a primary crop, and (iii) had applied for and were deemed eligible to receive credit from Farmerline.

We randomly assigned eligible farmers to either the treatment or control group. Farmers assigned to treatment were notified that they would receive loans. Farmers could choose to

[2] Farmerline also appoints and trains local agents to provide technical advice.

[3] Additionally, most farmers avail of Farmerline's services repeatedly over successive seasons, allowing Farmerline to improve its underwriting over time.

receive a variety of farm inputs, including inorganic fertilizers, insecticides, and herbicides, as the credit product. Farmers could receive loans worth up to GHS 350 (about $75 at the time). Farmers assigned to the control group were notified that they would not receive farm inputs on credit. All farmers, including those in the control group, could continue to use all of Farmerline's other services.

The loans were designed to be delivered to recipients before input application for the main cocoa season began. The loans had to be repaid at a 4% per-month rate with monthly payments starting at the end of 3 months following input disbursement, with a total repayment term of up to 6 months.[4] The goal of this setup was to allow farmers to start loan repayment after harvest when they are more likely to realize the financial returns on their agricultural investments. Finally, the loans were uncollateralized, and farmers were informed that default would likely exclude them from Farmerline's services in the future.

3. Study Design and Empirical Strategy

3A. Descriptive Statistics

Our sample consisted of 1,372 farmers. Cocoa was the most commonly cultivated primary crop. Over half of the farmers cultivated at least one other crop, most commonly plantain and cassava. Table 1 provides descriptive statistics.[5]

The farmers were small landholders, with a median cultivated acreage of 5.5 acres and average of 7.3 acres. The farmers reported little past usage of formal credit: only 11% of farmers had borrowed from a formal institution the prior year. However, up to 21% of farmers reported borrowing from informal sources such as moneylenders, family, and peers. Interestingly, farmers believe that credit could improve their crop yields: 65% of farmers report applying for input credit in order to "increase yield".

We note that some key endline outcomes—including the value of crops produced, farm revenues, and farm profits—are considerably lower, on average, at the endline compared to the baseline. A plausible factor contributing to these differences is the timing of COVID-19, which led to stringent lockdowns, including in the major trading centers of Accra and Kumasi. These lockdowns were in place during the harvest season, prior to our data collection at the endline during late summer 2020. Further, these lockdowns affected crop supply chains across and beyond Ghana, and hampered trading activity likely affecting crop sales, revenues and profits.

[4] At the time of the endline survey, Farmerline received complete repayments from 77% of the borrowers, and had many borrowers that were still repaying their loans. Unfortunately, we could not obtain long-run repayment data from the partner, and therefore cannot comment on what proportion of repayment they received from the remaining 23% of borrowers.

[5] See Appendix for the surveys.

3B. Experimental Design and Compliance

We randomly assigned 917 farmers (67%) to the treatment group and 455 farmers (33%) to the control group. Columns 1–3 of Table 1 describes balance tests across a range of demographic, agricultural, and other non-agricultural economic variables at baseline between farmers assigned to treatment and control groups. In addition to these baseline balance checks, qualitative focus groups and semi-structured interviews conducted at the end of the intervention confirmed participants were aware that the allocation was computer-based. The study had an overall attrition rate of 2.7%, and little compositional differences were observed.

Unfortunately, compliance with the treatment assignment was imperfect on two levels, primarily due to operational challenges faced by Farmerline: (1) Although all participants had applied for a loan and been deemed eligible, only 59% (544) of treatment farmers received a loan, while 17% (77) of control farmers also received a loan;[6] (2) Of those who received loans, many treatment farmers did not receive the farm inputs at the optimal time for investments for the main planting season. Specifically, only about 25% of eligible treatment farmers received loans within 30 days of their application, the intended timeframe. For the other 75%, loans did not arrive in time for the optimal input application period. Ex-post focus group discussions with farmers identified this as a potential driver of impacts (or lack thereof), and thus we return to this in our empirical analysis.

For the first compliance issue (non take-up in treatment and take-up in control), farmers assigned to treatment who received a loan were similar to those who did not along many dimensions, but differ on a few: borrowers are less likely female, older, and cultivators of larger farms (Table 1, Columns 4-6). In contrast, we observe less selection within control for those that received versus did not receive a loan (Table 1, Column 7-9). These tests reinforce our belief observation that the main drivers of imperfect overall compliance were operational challenges faced by Farmerline, but that these challenges led to selection with respect to observables (and likely unobservables) into compliance. Therefore as we describe below, we include all farmers in our analysis and focus on measuring intent-to-treat (ITT) effects.

For the second issue (timeliness of loan delivery), of the 621 farmers who received loans, irrespective of being assigned to the treatment or control group, 171 farmers (27.5%) received timely loans, i.e. the loan product was delivered to them within 30 days of applying, as initially promised by Farmerline. We examine selection effects from this non-compliance by comparing baseline values and demographics for those who received timely versus less timely loans (Table 1, Column 10-12). Farmers with timely loans had *lower* input expenditures and are *less* likely to have owned a business.[7] These farmers may have been more proactive in ways that helped

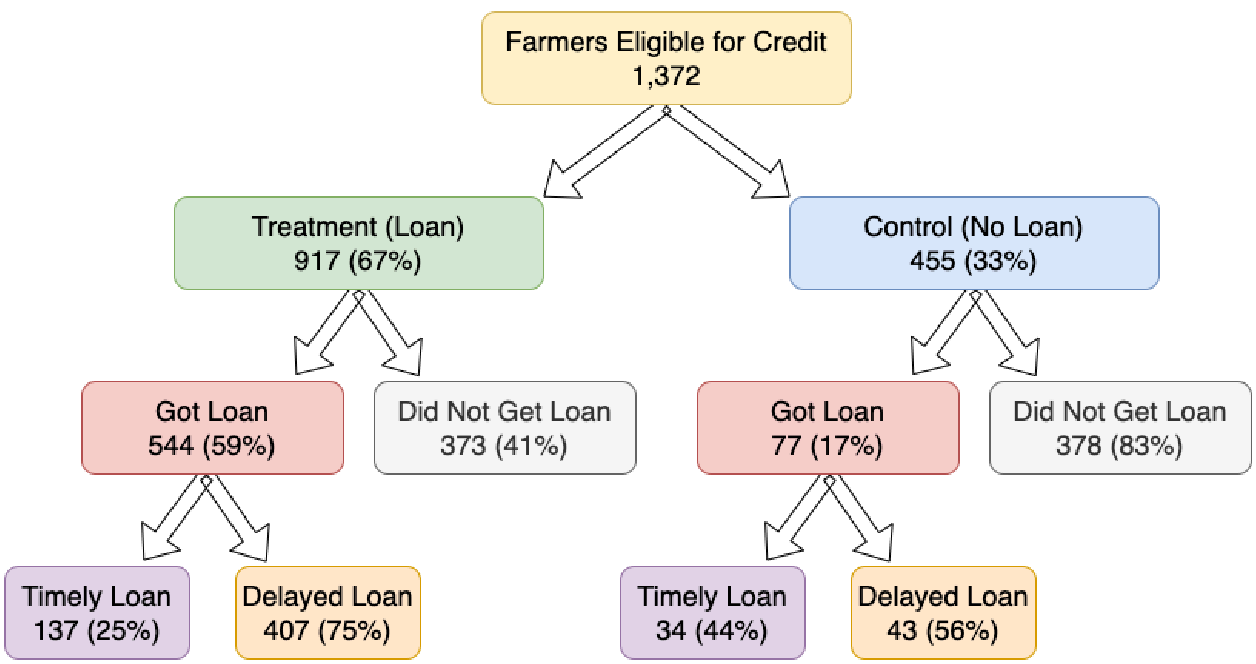
[6] This happened, for example, in a few cases when the farm inputs were delivered in batches to a local agent or a chief farmer before being individually disbursed; some control farmers present at the scene obtained the loan product instead of any absent treatment farmers.

[7] Notably, we do not find statistically significant differences in baseline expenditures on output, sales, profits, or fertilizers and insecticides (the main inputs offered on credit by Farmerline).

them to receive inputs. Focus groups support this conjecture: for example, one respondent noted how farmers like himself were "*closer to the local Farmerline agent and got access to the information*" on when the Farmerline personnel would deliver the loan products to the village, and how "t*he day the Farmerline personnel came, he only met 4 farmers*". Taken together, farmers that received more timely loans may have faced more binding credit constraints and therefore have valued access to credit more.[8] Keeping in mind these potentially endogenous causes of selection into timely credit receipt, we will estimate the differential impact using an ITT specification comparing timely to less timely loans.

Figure 1 illustrates the experimental design, summarizing the randomized allocation to treatment groups and compliance by participants.

Figure 1: Experimental Design and Compliance



3C. Pre-registered Hypotheses

Our theory of change posits that binding financial constraints limit farm investment, thereby lowering crop output and profits. To test this, we estimate whether and how our short-term credit intervention affects farmers' seasonal investment choices as well as their downstream agricultural and financial outcomes. We pose the following hypotheses.

First, we hypothesize that *many farmers face a binding credit constraint:* at the terms Farmerline offers, they would choose to borrow, but such terms are not available to them. Our treatment

[8] At the community level, there were wide disparities in the share of farmers who received timely loans, but we do not observe any clear patterns by type of community (including size).

relaxes this constraint, thereby lowering the shadow cost of working capital and increasing farm investment.[9] We test this hypothesis by examining treatment effects on a range of farm input expenditures and land use choices.

Second, we hypothesize that *access to credit improves downstream agricultural outcomes*. To test this, we focus on the key indicators of farm performance, including the market value of crops produced, revenues earned from crop sales, and farm profits.[10]

Our other hypotheses posit that access to credit has positive impacts on farmers' *non-agricultural economic outcomes*, *experience with formal credit and self-perceptions of creditworthiness*, as well as *livelihood and well-being*.[11] To test these, we estimate treatment effects for several economic outcomes relating to household finances and business ownership. These may be positively affected if the binding credit constraints are relaxed by the treatment and farmers are able to divert resources from farming to higher return activities. We also test for treatment effects on several actual and perceived outcomes on farmers' credit usage. We hypothesize that Farmerline's loan approvals may provide farmers with a signal about their credit worthiness. Finally, we examine whether the treatment impacted several variables pertaining to livelihood and well-being, such as food security, perceived position on an imaginary social ladder, and psychological well-being.[12]

3D. Empirical Strategy

Our primary estimation uses an ITT framework that adheres to the experimental assignment to treatment irrespective of compliance:

$$Y_i = \alpha + \beta\, Treatment_i + \gamma\, BaselineY_i + \delta App\ Month_i + \theta Survey\ Month_i + \varepsilon_i$$

Outcomes *Y* are for farmer *i*. We focus on all outcomes for the main agricultural season, as the program aimed to deliver loans before the start of the main season. The variable *Treatment* is an indicator which equals 1 for farmers assigned to treatment, 0 for control, irrespective of

[9] An increase in investment contingent upon receiving a loan is sufficient to reject neoclassical separation. It does not imply that the farmer had no access to credit at a higher cost. If the loan displaces all borrowing from high-cost lenders this would lower the opportunity cost of capital to the farmer and induce greater investment. We are referring to such borrowers as capital constrained, even though they might be able to borrow at a cost higher than that of Farmerline.

[10] To estimate profits, we are only able to capture farm input expenditures as costs.

[11] Our pre-analysis plan also hypothesized that the loans allow farmers to better deal with shocks (such as the COVID-19 crisis). However, being unable to survey farmers in the early COVID-19 outbreak period, we drop this hypothesis.

[12] A nascent field of research links poverty and psychological outcomes and suggests that poverty may cause stress, lead to worse decision making, and form a cycle that perpetuates poverty (Haushofer and Fehr, 2014).

actual loan receipt. *App Month* and *Survey Month* represent the month of loan application and follow-up survey.

4. Results

4A. Main Results

We focus first on farmers' agricultural investment choices. Table 2 Panel A reports treatment effects on farm input expenditures. Treatment led to an 11% (95% CI: -1% to 24%) increase in total farm input expenditures, compared to control (Column 1). To unpack this increase, Table 3 presents estimates on specific farm expenditures. The most precisely measured increases in expenditures are for insecticides, land rental, hired machinery, and fees farmers paid for irrigation services. These effects are quite large, ranging from 15% above the baseline mean for insecticides, to over 280% for hired machinery. While the coefficients for all other farm inputs are positive, they are noisy and not statistically significant. This paints an interesting picture: treatment farmers are likely to spend more on inputs which they are less likely to have received on credit as part of the experiment (with the possible exception of insecticides). This suggests that farmers face binding credit constraints and that when such constraints are relaxed with key farm inputs being offered on credit, farmers spend more, on average, on other farm inputs to complement their investment decisions.

We then focus on key farm outcomes. In the rest of Table 2 Panel A, we find no statistically significant treatment effects on the market value of crops produced, crop sales, and farm profits. In the cases of production and sales, the coefficients are small and not statistically significant. For profits, the coefficient is a relatively large negative value but, once again, not statistically significant. The 95% confidence intervals allow us to rule out effect sizes of greater than 14%, 13%, and 15% of the baseline mean for crop production, sales, and profits, respectively. We cannot definitively comment on why there is such a limited impact of the loans. We could speculate that extending credit in itself is not sufficient, and perhaps could have greater impact when combined with other factors such as extension services. We argue below that one cause is the delayed delivery of loans to most farmers, which may have prevented them from making decisions that would have led to more positive outcomes.

In Panel B, we consider farmers' land use choices, another form of measurable agricultural investment. There is no detectable effect of the treatment on the area cultivated with a single crop. We are not surprised by this, especially given the short-term nature of the treatment. Perhaps surprisingly, however, we find that farmers do increase the area on which they practice mixed cropping.[13] The negative coefficient on the area in which farmers cultivate a single crop suggests that at least some of this increase may be due to a switch from single to mixed

[13] The increasing effect on area used for mixed cropping is larger (and statistically significant) for those farmers who, at baseline, (i) do not practice mixed cropping and (ii) do not grow cocoa—the most common primary crop in our sample. Taken together, these suggest improvements in cultivation practices, especially for farmers not practicing mixed cropping or the cultivation of a major cash crop at baseline.

cropping, but the estimate is noisy. As before, this is suggestive of the notion that relaxing binding credit constraints may allow farmers to cultivate more, at the intensive margin.

Panel C shows that farmers assigned to treatment have 32% more money owed by others to themselves, compared to the baseline mean. Treatment farmers have 24% more household savings compared to control group farmers, although the latter estimate is imprecise. Lending locally and to family, and saving, requires little labor and helps diversify risk from farming.

Treatment farmers are 10% less likely to own a non-farm business at the endline, compared to the average baseline non-farm business ownership (Panel D). This estimate is statistically significant at the 5% level. Treatment farmers also record negative, albeit imprecise, coefficients for non-farm business investment and income. This result is consistent with the finding in Panel A that treatment farmers spent more on inputs complementary with the in-kind inputs provided on credit. Treated households may also be providing additional household labor to their farms (which we cannot measure), and thus reducing effort on non-farm enterprises.

In Panel E, we show that treatment farmers borrow more from other microfinance lenders compared to control farmers, but there is no statistically significant difference in their borrowing from banks, moneylenders and other informal sources such as social networks. Borrowing from microfinance lenders suggests that the treatment induces farmers to seek additional sources of formal credit. We find no detectable differences between treatment and control across variables aimed at capturing farmers' self-perception of how creditworthy they are, except some suggestive evidence (p=0.12) that treatment farmers are more likely to believe that they will receive a better interest rate from a different lender.

Finally, in Panel F, we examine effects on indicators of health and well-being. We note that these variables are based on subjective responses. We find no statistically significant differences in indices capturing food security, psychological distress and subjective well-being. We note that the loans could have had a viable benefit on food security especially in the lean season (Fink et. al., 2020) that we could not capture because of the timing of our measurement. However, we do find that the treatment and control group farmers differ on their assessment of where they would place themselves on a social ladder measuring absolute wellbeing. Treatment farmers, on average, place themselves higher on the ladder.[14]

4C. Heterogeneity

We test for heterogeneous treatment effects (HTE) along three dimensions with the below regression specification. First, we explore whether there were heterogeneous effects for farmers who received timely loans, although this was not registered in our pre-analysis plan. Then, we also test for heterogeneous effects along: i) gender and ii) farming experience, both of which were pre-specified.

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[14] Appendix Table A4 presents adjusted p-values and q-values from multiple hypotheses tests. Most results retain similar levels of statistical significance.

$$Y_i = \alpha + \beta\, Treatment_i + \lambda\, Heterogeneous\ Dimension_i$$
$$+ \mu\, Treatment_i\ X\ Heterogeneous\ Dimension_i$$
$$+ \gamma\, Baseline\ Y_i + \delta App\ Month_i + \theta Survey\ Month_i + \varepsilon_i$$

4C.1. Timeliness of Loans

We conducted 40 focus groups and 55 semi-structured individual interviews with farmers at the end of the experiment. From these, a common theme emerged: many borrowers received the loan product, i.e. farm inputs, later than they had anticipated, and these delays affected their farming operations.[15]

We explore HTE by loan timeliness despite the endogenous selection of less-resourced farmers into timely loans discussed above for two reasons. First, it sheds suggestive evidence of a key implementation fidelity point, one that may drive impact. In particular, failure to find differential impact may have suggested that what seems like an indicator of strength of demand for credit, or urgency of demand, is not indeed also predictive of impact of access to credit. Second, it is plausible that when interventions like ours are scaled up by policymakers and other lenders, a similar subset of farmers with a high need for credit and binding credit constraints are more likely to self-select into borrowing.

To test for a timing effect, we split the ITT estimate into timely disbursal (received within 30 days) or less timely (received after 30 days). Additionally, in these regressions, we control for age, baseline farm input expenditure, and baseline business ownership—covariates that are predictive of selection into timely loan take-up.

Table 2 Columns 2-3 report these results. The results show a strong pattern: mean crop sales and crop production are notably and statistically significantly higher for treatment farmers that receive timely loans, as promised, within the first month after application approval than those who do not. Similarly, profits are substantially higher for these farmers. Even though the total profit estimate is imprecisely measured (p=0.20), the point estimate illustrates a 31% increase (95% CI: -17% to 80%) over the baseline mean profits, and a 118% increase over the endline control group mean profits.

We note two additional results. First, while the point estimate of the interaction is large and positive, we cannot reject the null that total farm expenditure is the same for treatment farmers receiving the loans on time as for other treatment farmers; the p-value for the total effect on farm input expenditures is p=0.14. Second, these farmers with timely loans record large increases in expenditures on fertilizer (Table 3), which they likely also receive in kind from Farmerline as part of their loan, relative to farmers whose loans are delayed. This is in contrast to the ITT effects discussed earlier that showed farmers assigned to treatment increasing spending on other

[15] The Appendix presents several farmers' quotes from these qualitative data.

inputs. Combined with the results on crop sales and profits, this is indicative of the importance of constraints farmers face in obtaining fertilizers for the crop production process.

Our results suggest that although the overall treatment effect may have been weak, the loans, conditional on being delivered in time, may indeed have had substantial beneficial impacts on farmers. This interpretation must be tentative because the outcome of receiving a timely loan is likely determined in part by the activities of the borrower and correlated with unobserved determinants of farm output. We argue, however, that these estimates, despite this potential endogeneity, provide important insights in understanding the necessary conditions for credit to have impact on final outcomes. Further, as we allude to above, the “take-up” of timely loans in our settings appears to be driven by the farmers who need credit the most, and these results may be important for other external interventions offering credit to underserved farmers.

4C.2. Gender

Our motivation to test for heterogeneous effects by gender stems from strong regional evidence showing that plots farmed by women receive fewer inputs than comparable plots farmed by men. This evidence implies that intra-household agricultural resource allocation can be pareto inefficient (Udry, 1996). Additionally, traditional microcredit models have often intentionally targeted women (Banerjee, 2013) and we believe that it is useful to document whether and how outcomes, in our agriculture and digital credit setup, vary by the borrower’s gender.

Table 2 Columns 4-5 presents impact by gender. The treatment effect on farm expenditures is entirely driven by male farmers. While virtually all categories of farm input expenditures record a negative coefficient for the heterogeneous effect, the most precisely measured coefficients are for insecticides, rented equipment, and fees paid for irrigation and registration. Female treatment farmers also borrow statistically significantly less from informal networks such as relatives and peer groups.

We do not detect any statistically significant heterogeneous treatment effects on crop production and sales: the point estimates for both are large, positive and quite imprecise. We do observe a positive differential effect on profits for treated women vs treated male farmers, but it is only marginally statistically significant (p-value = 0.09). We believe, however, that the most striking result is that treatment leads to a substantial *increase* in non-farm business income for women farmers relative to male farmers. Importantly, this relative increase in business is driven almost entirely by the intensive margin, as we do not detect any effects on business ownership at the extensive margin.

Taken together, these estimates suggest that women may not be fully utilizing the farm inputs they receive on credit. They may, for instance, be giving it away to their (unsurveyed) husbands.[16] Alternatively, they may be selling the inputs and using the proceeds elsewhere, such as in their non-farm enterprises. If the latter is the case, the results further indicate that the

[16] Previous research suggests that women, in particular, feel pressured to share income with their kin (Jakiela and Ozier, 2016).

treatment allows women farmers to shift intensity from their farms to other enterprises. However, we note that these estimates are accompanied by certain caveats: we cannot rule out that at least a part of the increase in business income, and the accompanying marginally significant increase in farm profits, may be reported due to sales of the loan products, rather than use. Additionally, we are not equipped to comment on aggregate household-level welfare effects..[17]

4C.3. Farming Experience

We consider heterogeneous effects by farming experience as it is plausible that farming knowledge accumulates with experience and, all else equal, more experienced farmers may be able to make better use of relaxed credit constraints compared to less experienced farmers.

To test for heterogeneity, we construct an indicator variable that equals 1 if the farmer has above median years of farming experience in our sample, and 0 otherwise. We report these results in Columns 2–3 of Appendix Table A1.

We find little evidence of heterogeneity by farmer experience across most outcomes. There are, however, a few exceptions. First, we find evidence that treated experienced farmers spend more on fertilizers and insecticides, and less on land rent relative to treated inexperienced farmers. This is similar to what we find with respect to farmers who received timely loans, and may reflect experienced farmers's ability to better manage the uncertain timing of loan delivery by modifying practices. We do not, however, find a significant relative increase in aggregate farm input expenditures for more experienced treatment farmers.

Second, we find that treatment is associated with more experienced farmers increasing business ownership at the extensive margin, compared to less experienced farmers. While speculative, this could occur if a set of more experienced farmers gauge that the delayed loans would not be as useful in agriculture and therefore decide to divert resources to other enterprises.

Finally, we cautiously note that the average farmer in our experiment has over 18 years of farming experience. Given this high farming experience, it is perhaps not surprising that we observe no substantial heterogeneous treatment effects.

5. Conclusion

We document the effects of access to agricultural inputs on credit for farmers in a major cocoa-growing region of Ghana through digital credit tools. Farmers randomly assigned access to credit increased overall farm input expenditures although there was no average effect of the intervention on crop output, sales or farm profits.

[17] Appendix Table A3 shows robustness to a single specification that contains all heterogeneity dimensions.

Our experiment was compromised by non-compliance, driven partly by logistical difficulties faced by our implementing partner. Exploring one type of non-compliance (the failure to deliver credit to the treatment group on time), we show that the receipt of a timely loan generates large increases in crop output, sales, and profits. In spite of there being some potentially endogenous selection in this form of non-compliance, we highlight how these estimates are useful for external validity. In addition, we document interesting heterogeneous treatment effects for women, who are less likely to increase farm input expenditures when provided with access to credit.

One possible reason for the muted impact of the overall intervention is that credit itself may not be enough to generate increases in crop production and sales. Perhaps other services, such as extension workers, can provide farmers with the knowledge to use farm inputs more efficiently. Alternatively, farmers may face other binding constraints due to other market imperfections, such as the lack of insurance. One promising relatively underexplored avenue of research would be to test the role of local agents in a similar setting. Local agents appointed by the credit provider work simultaneously as extension workers, helping farmers improve their agricultural decision making and investment choices, and as loan recovery agents, allowing credit providers to improve loan recovery and reduce default rates. Whether designing incentive structures for such agents can impact farmers' outcomes to a greater extent is left for future research.

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Table 1: Baseline Summary Statistics, Balance Tests, and Selection Analysis

Means (standard deviations / standard errors)

Analysis:	**Treatment Assignment Balance**			**Compliance Analysis (All Loans)**						**Take Up Analysis (Timely Loans)**		
Sample of Farmers:	**All Farmers**			**Treatment Group**			**Control Group**			**All Borrowers**		
	Control Mean (Std Dev)	Diff in Means: Treat - Control (Std Error)	p-value of Diff	No Loan Mean (Std Dev)	Diff in Means: Got Loan - No Loan (Std Error)	p-value of Diff	No Loan Mean (Std Dev)	Diff in Means: Got Loan - No Loan (Std Error)	p-value of Diff	Late Loan Mean (Std Dev)	Diff in Means: Timely - Late Loan (Std Error)	p-Value of Diff
Dependent Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Demographics												
Age	51.0	-1.1	0.12	48.8	1.7	0.04	51.0	-0.06	0.97	49.7	3.5	0.00
	(12.6)	(0.72)		(12.5)	(0.85)		(12.8)	(1.58)		(12.5)	(1.1)	
Female (Indicator)	0.28	-0.02	0.47	0.32	-0.09	0.00	0.27	0.04	0.49	0.22	0.07	0.09
	(0.45)	(0.03)		(0.47)	(0.03)		(0.45)	(0.06)		(0.41)	(0.04)	
Years Farming	18.3	0.26	0.64	18.3	-0.42	0.53	18.7	-2.1	0.07	18.3	0.77	0.36
	(9.4)	(0.56)		(10.3)	(0.66)		(9.7)	(1.2)		(9.5)	(0.84)	
Household Education	4.0	0.07	0.34	4.2	-0.08	0.32	4.0	0.01	0.96	4.0	0.11	0.28
	(1.2)	(0.07)		(1.2)	(0.08)		(1.2)	(0.15)		(1.2)	(0.11)	
Farm Finances												
Value of Agricultural Assets	699.9	25.6	0.68	666.6	100.3	0.19	698.5	8.3	0.95	735.8	82.5	0.42
	(1041.4)	(63.0)		(1078.6)	(75.6)		(1055.6)	(130.4)		(1161.6)	(101.9)	
Farm Input Expenditures	1313.9	116.8	0.17	1436.3	-9.5	0.93	1332.4	-109.7	0.52	1487.3	-311.7	0.02
	(1374.4)	(84.5)		(1626.3)	(102.3)		(1388.3)	(172.0)		(1536.4)	(127.9)	
Value of Crops Produced	3809.0	371.6	0.39	4312.5	-222.3	0.65	3804.4	27.2	0.98	4161.8	-376.6	0.57
	(7756.2)	(429.2)		(7583.6)	(494.1)		(7692.3)	(970.8)		(7616.9)	(656.1)	
Crop Sales	3476.0	498.7	0.20	4160.9	-313.9	0.49	3426.2	294.3	0.73	3885.1	-195.6	0.74
	(6694.1)	(386.8)		(7158.5)	(455.3)		(6506.5)	(837.8)		(6823.2)	(596.3)	
Farm Profits	2495.2	254.7	0.54	2876.1	-212.7	0.66	2472.0	136.9	0.88	2674.5	-64.9	0.92
	(7504.9)	(416.5)		(7123.7)	(480.1)		(7481.7)	(939.4)		(7538.1)	(648.6)	
Other Farm Characteristics												
Total Cultivated Area	7.0	0.41	0.32	7.0	1.2	0.02	7.2	-0.89	0.30	7.8	0.44	0.52
	(6.8)	(0.41)		(7.4)	(0.51)		(6.9)	(0.86)		(7.4)	(0.68)	
Crop Diversification	0.84	-0.00	0.97	0.86	-0.03	0.21	0.84	0.03	0.52	0.83	0.01	0.66
	(0.36)	(0.02)		(0.34)	(0.02)		(0.37)	(0.05)		(0.37)	(0.03)	
Farm Shocks (Std Index)	-0.02	0.03	0.61	-0.04	0.08	0.23	0.02	-0.23	0.07	-0.01	0.08	0.42
	(1.0)	(0.06)		(1.0)	(0.07)		(1.0)	(0.13)		(1.0)	(0.09)	
Cocoa Primary Crop (Indicator)	0.47	0.02	0.45	0.47	0.04	0.25	0.47	0.01	0.81	0.49	0.06	0.20
	(0.50)	(0.03)		(0.50)	(0.03)		(0.50)	(0.06)		(0.50)	(0.04)	
Non Farm Characteristics												
No. of Loans in the Last 1 Year	0.46	-0.02	0.74	0.39	0.08	0.13	0.48	-0.11	0.22	0.48	-0.06	0.39
	(0.74)	(0.05)		(0.75)	(0.06)		(0.77)	(0.09)		(0.88)	(0.08)	
Business Ownership	0.48	0.02	0.53	0.51	-0.01	0.74	0.48	0.03	0.67	0.53	-0.11	0.02
	(0.50)	(0.03)		(0.50)	(0.03)		(0.50)	(0.06)		(0.50)	(0.04)	
Food Security (Index)	0.04	-0.06	0.27	-0.03	0.01	0.91	0.08	-0.25	0.04	-0.02	-0.05	0.56
	(0.99)	(0.06)		(1.0)	(0.07)		(0.95)	(0.12)		(1.0)	(0.09)	
Take-up rate	66.8%			59.3%			16.9%			27.5%		
Number of observations	1372			917			455			621		
F-stat (p-val) multivariate test	0.93 (0.53)			2.2 (0.00)			1.1 (0.35)			2.2 (0.01)		

Table 2: Intent to Treat (ITT) Estimates

	Base Spec	Timeliness of Loans		Gender		Means	
	ITT Full Sample	ITT	ITT x Timely Loan	ITT	ITT x Female	Baseline: All	Endline: Control
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent Variable							
Panel A: Farm Financial Outcomes							
Total Farm Input Expenditures	158.9*	118.7	275.1	255.7**	-376.3**	1391.9	1556.2
	(90.2)	(97.5)	(285.2)	(110.3)	(187.8)	(1474.3)	(1459.4)
Market Value of Crops Produced	-8.8	-236.2	1410.7**	-169.8	554.7	4057.4	2258.1
	(283.5)	(298.5)	(669.0)	(360.1)	(549.5)	(7483.7)	(4945.3)
Crop Sales	-48.5	-248.4	1280.4**	-187.9	481.7	3809.3	2172.9
	(277.8)	(292.9)	(653.3)	(353.5)	(536.5)	(6746.8)	(4840.8)
Crop Profits	-173.8	-348.9	1179.2*	-432.9	932.9*	2665.5	702.0
	(284.0)	(302.4)	(726.4)	(357.6)	(561.2)	(7261.2)	(4787.8)
Panel B: Farm Land Use							
Sole Crops Area	-0.24	-0.35	0.50	-0.22	-0.09	3.9	2.6
	(0.33)	(0.35)	(0.86)	(0.43)	(0.65)	(6.4)	(5.5)
Mixed Crops Area	0.61**	0.59**	0.30	0.50	0.28	3.3	3.3
	(0.27)	(0.29)	(0.75)	(0.34)	(0.51)	(4.3)	(4.2)
Crop Diversification	0.03	0.01	0.15	0.06**	-0.12**	0.85	0.74
	(0.03)	(0.03)	(0.09)	(0.03)	(0.06)	(0.36)	(0.44)
Panel C: Household Finances							
Money Owed to Respondent	149.5**	122.7*	146.7	188.9***	-154.5	462.9	438.2
	(59.9)	(64.6)	(153.6)	(73.7)	(121.0)	(1030.7)	(1092.4)
Household Savings	115.9	94.5	-272.0	119.0	-29.8	483.6	746.4
	(89.8)	(94.4)	(291.7)	(112.9)	(172.0)	(1072.8)	(1737.9)
Panel D: Business Outcomes							
Owning a Business (Indicator)	-0.05**	-0.04	-0.12	-0.05*	0.02	0.50	0.47
	(0.03)	(0.03)	(0.11)	(0.03)	(0.06)	(0.50)	(0.50)
Investment into Business	-54.6	-79.3	62.9	-75.9	73.6	475.8	395.3
	(55.0)	(55.8)	(199.1)	(67.1)	(117.0)	(1327.6)	(947.0)
Income from Business	-60.0	-68.9	28.8	-120.7*	216.5**	540.6	348.2
	(50.2)	(54.7)	(138.1)	(65.8)	(97.5)	(1158.0)	(916.4)
Panel E: Experience with Credit							
Credit Received -- Banks	40.5	24.3	108.2	10.0	113.2	262.3	325.5
	(73.1)	(81.1)	(180.3)	(93.9)	(134.5)	(1056.3)	(1425.1)
Credit Received -- Microfinance	25.1***	27.5***	-5.5	24.0**	4.2	16.5	2.5
	(9.3)	(10.9)	(21.7)	(11.1)	(20.2)	(177.9)	(47.9)
Credit Received -- Moneylender	-27.2	-37.4	68.6	-46.6	67.8	57.0	109.8

	(42.2)	(45.7)	(97.0)	(57.3)	(69.0)	(339.1)	(826.1)
Credit Received -- Informal	26.5	33.2	-109.6*	57.4	-116.7**	167.1	64.2
	(31.0)	(31.9)	(67.5)	(43.6)	(58.2)	(718.9)	(261.6)
Perceived Creditworthiness (Index)	0.08	0.08	-0.07	0.11*	-0.14	-	-0.05
	(0.06)	(0.06)	(0.21)	(0.07)	(0.13)		(0.98)
Panel F: Health and Well Being							
Social Ladder Position, Absolute Wellbeing	0.11**	0.10*	0.02	0.18***	-0.27**	-	-0.07
	(0.06)	(0.06)	(0.19)	(0.07)	(0.13)		(0.94)
Food Security (Index)	-0.04	-0.04	-0.16	-0.03	-0.00	0.0	0.0
	(0.06)	(0.06)	(0.21)	(0.07)	(0.13)	(1.0)	(0.99)
Psychological Distress (Index)	-0.02	-0.03	0.10	-0.02	-0.02	-	0.02
	(0.06)	(0.06)	(0.19)	(0.07)	(0.13)		(0.98)
Subjective Well Being (Index)	-0.01	0.02	-0.31	-0.00	-0.01	-	0.01
	(0.06)	(0.06)	(0.21)	(0.07)	(0.13)		(0.98)

N = 1,335 (Baseline: 1,372, Endline: 1,335, Attrition Rate: 2.7%). Baseline mean values are missing for questions asked only at endline (and not at baseline). Timely loan regressions include controls for age, farm input expenditures, and business ownership, the three variables that predict timely loan takeup with p-value<0.05 in Table 1. See Data Appendix for definitions of all variables. *, **, *** refer to statistical significance at 1%, 5%, and 10% respectively.

Table 3: ITT Estimates of Impact on Farm Input Expenditures

	Base Spec	Timeliness of Loans		Gender		Means	
	ITT Full Sample	ITT	ITT x Timely Loan	ITT	ITT x Female	Baseline: All	Endline: Control
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent Variable							
Fertilizers	27.3	6.7	162.5*	46.4	-73.4	191.7	281.8
	(27.8)	(29.6)	(90.9)	(34.5)	(58.0)	(449.8)	(449.1)
Insecticides	35.8*	22.0	64.3	63.5***	-104.7**	231.8	208.4
	(19.8)	(21.1)	(55.7)	(22.8)	(46.4)	(386.3)	(316.6)
Herbicides and Weedicides	5.9	-0.73	33.6	15.8	-39.3	125.4	148.4
	(15.0)	(16.7)	(42.7)	(18.3)	(31.6)	(183.5)	(262.4)
Land Rental	45.5**	44.3*	-39.4	52.7*	-30.0	106.7	81.9
	(21.2)	(23.3)	(58.0)	(28.1)	(34.5)	(404.5)	(306.7)
Hired Labor	18.9	5.1	50.1	35.7	-66.3	403.8	494.7
	(38.7)	(41.9)	(111.7)	(41.5)	(98.8)	(526.2)	(718.0)
Hired Tractor	7.1*	7.0*	-1.9	9.2*	-8.3	2.5	9.6
	(3.8)	(4.0)	(5.0)	(4.9)	(6.4)	(29.5)	(51.0)
Hired Animals	0.83	1.2	-1.0	0.24	2.2	0.16	0.8
	(1.2)	(1.5)	(1.7)	(1.3)	(3.0)	(4.3)	(14.3)
Seeds	3.8	0.45	6.4	5.3	-9.2	208.4	225.8
	(23.0)	(24.6)	(63.7)	(30.3)	(41.2)	(351.5)	(375.7)
Rented Equipment	6.5	7.7	-8.0	14.9*	-30.9**	62.8	41.3
	(6.9)	(7.5)	(18.6)	(8.3)	(14.3)	(145.6)	(97.5)
Irrigation and Registration Fees	18.1**	21.0**	-18.8	24.1**	-23.3*	34.0	31.9
	(7.7)	(8.5)	(12.7)	(10.3)	(12.8)	(143.6)	(90.6)
Other Inputs	6.9	6.1	11.1	11.0	-16.1	24.7	31.5
	(6.6)	(7.2)	(14.6)	(8.5)	(12.5)	(133.3)	(99.1)

N = 1,335 (Baseline: 1,372, Endline: 1,335, Attrition Rate: 2.7%). Baseline mean values are missing for questions asked only at endline (and not at baseline). Timely loan regressions include controls for age, farm input expenditures, and business ownership, the three variables that predict timely loan takeup with p-value<0.05 in Table 1. See Data Appendix for definitions of all variables. *, **, *** refer to statistical significance at 1%, 5%, and 10% respectively.