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# Building Women's Skills for Economic Inclusion and Resilience

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## Abstract

Can skills-based programs promote women's economic inclusion? This study randomizes access to a program teaching entrepreneurship skills in rural Uganda. The program covers record-keeping, identifying business opportunities, raising capital, and soft skills like perseverance and confidence, but it provides no access to cash or capital. Treated women are 17 percent more likely to generate income from their own businesses 18 months post-program. They heavily re-invest in their businesses. High-frequency data show that treated women also fare significantly better during the COVID-19 lockdown than women in the control group. Exploiting social network data, this paper detects positive network-based spillovers to the control group and provides novel tools to adjust estimates accordingly. Although the program is not transformative, the results indicate an important role for skills-based programming in efforts for economic inclusion among rural, low-income women.


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# Building Women's Skills for Economic Inclusion and Resilience*

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programming in efforts for economic inclusion among rural, low-income women.

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# 1 Introduction

Flagship programs for women's economic inclusion often include programming to develop hard and soft skills alongside cash and asset transfers (e.g., BRAC, Concern International). Developing skills for women is theoretically attractive. Women with limited access to education and wage work likely have few opportunities to develop skills, skills may help program impacts persist, and unlike loans, cash transfers, or asset transfers, skills cannot be expropriated. However, skills-based programming is intensive in terms of staff time relative to other interventions (Banerjee, Duflo, Goldberg, et al. 2015). It is also difficult to target because the returns to skills are heterogeneous and hard to predict. Uncertainty around the returns to skills and the practice of pairing skills-based programming with other interventions makes it difficult to understand to what extent skills are a relevant barrier to women's economic inclusion.

Can skills-based programs promote women's economic inclusion?[1] We take an intentionally broad view of economic inclusion that includes women's capacity to generate income, make investment and consumption choices, and cope with economic shocks. We make progress on our question first by using a randomized control trial of a skills-based program with 601 ultra-poor women in rural Uganda. The massive, unanticipated shock of the first national COVID-19 lockdown in Uganda augments our original experiment to provide evidence of program impacts on income resilience.

The program we study teaches general skills for business and entrepreneurship in eight, 2–3 hour modules over six months, complemented with optional individual coaching from program instructors. The hard skills components target low-literacy populations and aim to improve business practices through simple heuristics (similar to e.g., Drexler, Fischer, and Schoar (2014) and Batista, Sequeira, and Vicente (2022)). The program builds additional skills through modules on identifying business opportunities and performing market

[1] By "skills-based" we refer to programs that do not explicitly provide capital, cash, or access to credit. However, we broadly define skills to include technical and management skills along with soft skills.

research.[2] Modules on stepping out of one's comfort zone and exposing women to success stories aim to build soft skills. Critically, the program does not include access to credit or any transfer of cash or capital. Women self-select into participation after attending a detailed orientation session. Self-selection mitigates concerns about ineffective program targeting, allowing us to focus on the importance of skills for economic inclusion among women motivated to learn new skills.[3]

We collect three types of data to build a holistic understanding of the role skills play in economic inclusion among the women in our sample. First, we collect detailed in-person survey data on households and businesses at baseline before the intervention begins, midline shortly after women graduate from the program, and endline 12–18 months after graduation.[4] Our three rounds of in-person data collection elicit information about all businesses a woman runs, allowing us to observe how women's business portfolios change over time. Second, we collect high-frequency SMS data on revenues over the entire study period to understand whether the program enables women to better cope with negative shocks. Finally, we collect data on the social and business networks of women in our sample. Doing so allows us to quantify network-based spillovers.

The program strengthens hard and soft skills. We observe improved business tracking, price management, and more hours spent working in the main business. Effects on select business practices persist in the medium-run (18-24 months). Entrepreneurial soft skills also improve, with treated women being 38% more likely to set goals than women in the control group and exhibiting improvements in grit at endline.

Improvements in skills lead to gains in women's economic inclusion. At

[2]These program components are similar in spirit to the ILO's Start and Improve Your Business program as in deMel, McKenzie, and Woodruff (2014) and the personal initiative training in Campos et al. (2017).

[3]We study the differential impacts of a variation of this program that delivers coaching at women's homes or businesses in Lang and Seither (2022). The full design is pre-registered as AEARCTR-0003214Z (Lang, Magruder, and Seither, 2022).

[4]These differences in timing for the endline survey were caused by COVID-19 restrictions that prevented travel within Uganda.

baseline, the women who self-select into our sample are poorer than the average rural Ugandan household.[5] Upon graduating from the program, treated women are 16pp more likely to have an active business than women in the control group. They own 0.22 more businesses, on average, at endline. The program is also effective at promoting productive entrepreneurship: treated women are 10pp more likely to generate positive profits (off a control mean of 59%) and they are 8pp more likely to have positive profits in additional businesses (off a control mean of 16%). Although individual effects on profit levels in both the main business and all other businesses are noisy, our index of intensive-margin business performance shows positive, significant effects at both midline and endline. Importantly, our effects represent gains in women's employment, not simply switching between salaried and self-employment: treated women are 8pp more likely to be employed at midline (over a control mean of 72% of women being either employed or self-employed).

We exploit the unanticipated market shock of the first COVID-19 lockdown in Uganda to test for women's ability to cope with negative economic shocks. High-frequency SMS data shows that the program is highly effective at enabling women to navigate the first COVID lockdown. Treated women experience no decrease in revenues relative to the period immediately preceding the lockdown. By contrast, the control group only recovers to pre-lockdown revenue levels a month before the lockdown was fully lifted. We interpret this as promising evidence that skills-based programs can increase women's economic resilience.

We examine dynamic treatment effects on savings, investments, and household consumption spending to understand how women allocate their earnings. We find no significant effects on savings or investments at midline, right after program completion. However, treated women invest 97% more in other businesses at endline and our index of savings and investment outcomes shows positive, significant effects. Together, results on savings and investments indi-

[5]Half of the sampled women have an active business at baseline, and merely 11% are employed outside of self-employment. Many existing businesses are in perishable goods, livestock, and energy, but we also observe service-based businesses like salons as well as restaurants, retail, and construction.

cate high rates of re-investment in women's business portfolios, which we take to be indicative of increases in the resources women control. We find no significant increases in consumption expenditures at midline or endline, indicating that the program does not have significant impacts on poverty reduction over the period of our study. Given high observed rates of re-investment and overall low baseline profits, our results on consumption spending align with other program impacts.

Our study bridges a gap between the evidence on graduation from poverty programs and that on entrepreneurship training programs. We do so by studying the effects of building skills for entrepreneurship in an ultra-poor population of women. The women in our sample demographically resemble the populations targeted by graduation programs (Banerjee, Duflo, Goldberg, et al. 2015, Blattman et al. 2016, Bandiera, Burgess, et al. 2017, Banerjee, Duflo, and Sharma 2021, Bossuroy et al. 2022, Angelucci, Heath, and Noble 2023). Although the individual coaching and some of the financial management units in the program we study resemble skills-based components of graduation programs, the entrepreneurship skills are more closely related to those studied in the business training literature (see McKenzie, Woodruff, et al. (2023) for a systematic review of the literature).

In bridging these two bodies of evidence, we make three contributions. First, our results support the inclusion of skills in graduation approaches by showing that skills alone generate gains in women's economic inclusion. Second, we demonstrate that general entrepreneurship skills can promote economic inclusion for ultra-poor, rural women, particularly on the extensive margin of women entering entrepreneurship. Our results align with those in Calderone et al. (2022), who to our knowledge provide the only other experimental evidence on extensive-margin impacts in rural areas.[6] More broadly, our results suggest that the heavily mixed results on business training programs may be driven by differences in the populations studied.[7] The women

[6]deMel, McKenzie, and Woodruff (2014) and Brudevold-Newman et al. (2023) document positive extensive margin effects in urban populations that diminish over time.

[7]Thirteen of seventeen studies of programs with existing entrepreneurs in urban markets cited in McKenzie, Woodruff, et al. (2023) found no significant effect on profits (Berge,

who self-select into our sample are not all existing entrepreneurs and they fall well below median levels of consumption in rural Uganda, in contrast to most studies that focus on urban entrepreneurs. Our results highlight that the population where skills-based constraints bind may not be the population typically targeted by entrepreneurship programs.

Third, our paper makes two methodological contributions that provide a comprehensive understanding of the value of entrepreneurship skills in the lives of ultra-poor, rural women. First, we use high-frequency SMS data to better understand how the program we study affects income volatility and women's ability to cope with large economic shocks. Understanding volatility is first-order to alleviating poverty in rural areas, but traditional surveys are ill-suited to measure such dynamics. Second, we demonstrate the effectiveness of a relatively fast, low-cost method for collecting social network data using randomly ordered photobooks of study participants. Our photobooks allow low-literacy respondents to fully participate, avoid concerns about different names being used for the same person, and reduce respondent fatigue by allowing respondents to quickly identify network links. This is helpful in any setting where measuring social networks is important but is not the central focus of an intervention. We additionally demonstrate that in settings where network-based spillovers may occur, researchers can use baseline network data to quantify them.

Positive spillovers through business networks, but not friendship networks, speak to peer effects.[8] Our results stand in contrast to Field et al. (2016), who

Bjorvatn, and Tungodden (2015), Bruhn and Zia (2013), deMel, McKenzie, and Woodruff (2014), Brooks, Donovan, and Johnson (2018), Campos et al. (2017), Arráiz, Bhanot, and Calero (2019), Alibhai et al. (2019), Anderson et al. (2020)). By contrast, five out of seven studies on existing entrepreneurs in rural markets find positive effects from training (Calderon, Cunha, and Giorgi (2020), Bakhtiar, Bastian, and Goldstein (2022), Buvinic et al. (2020), Giné and Mansuri (2021)), though Avdeenko, Frölich, and Helmsmüller (2021) find null results and Giné and Mansuri (2021) document null results for female entrepreneurs.

[8]The literature on social networks finds that both the size and composition of an individual's network can have large effects on outcomes ranging from employment to technology adoption (e.g., Munshi (2003); Bandiera and Rasul (2006); Magruder (2010); Beaman and Magruder (2012); Beaman, Keleher, and Magruder (2018); Munshi and Rosenzweig (2016)), but women often benefit less from these social networks. For instance, Magruder (2010) finds that inter-generational network effects only increase employment rates for sons, and Bea-

find positive peer effects from social ties. In line with the results by McKenzie and Puerto (2021) and contrasting those in Cai and Szeidl (2022) and Bakhtiar, Bastian, and Goldstein (2022), our results suggest that successful women entrepreneurs generate positive economic spillovers.

Combined, our results show that building skills can be an effective tool for improving economic inclusion among ultra-poor, rural women. We find that skills increase the likelihood that women in our sample independently generate and control income, and improve their resilience to shocks. Although the results we observe are far from transformative for poverty alleviation or private sector development, they provide key evidence on the importance of skills in larger efforts to promote women's economic inclusion.

## 2 Background and Context

Based on the 2018 Living Standards and Measurement Survey (LSMS) in Uganda, 74% of rural women are employed in some form of productive activity (including paid work, self-employment, and unpaid work in family businesses). Thirteen percent of rural women engage in self-employment. In central Uganda, the region where our study is based, average monthly household expenditures are UGX 678,876 (USD 172.52 with a median of UGX 504,966) per household, and the median household size is four.

Although our partner implements its programs throughout Uganda, the women in our sample reside in five communities in central Uganda. Our implementing partner selected all study locations based on conversations with community leaders, their evaluation of the economic needs of the communities, and their estimate of the population of women who might be interested in participating.[9] Of the five communities where we worked, four are rural and

man and Magruder (2012) show that women are less likely to get job referrals than equally qualified men.

[9]Allowing our partner to select the study locations precludes random site selection; however, we argue that it yields representative study sites given that the program we study and others like it are unlikely to work in communities that are uninterested in participating or otherwise unable to participate.

one is peri-urban.

On average, 54% of women in our sample report working for at least part of their time in their own business at baseline, with median monthly profits of UGX 50,000 (USD 13.78). The most common types of businesses are those selling food products, both perishable and non-perishable, but around 7% of women also have businesses raising livestock, selling charcoal, vending clothes, and selling drinks. For context, women report median daily expenditures are UGX 5,900 (USD 1.63) in their households at baseline with a median household size of 4, indicating that the women in our sample tend to be poorer than the average rural household in central Uganda.

Self-selection yields a sample with higher rates of business ownership than the national average; however, nearly half of the women in our sample do not have a business at baseline. This sample composition is critical to considering the different margins at play when using entrepreneurship as a tool for poverty alleviation. Although programs like the one we study aim to help existing entrepreneurs run more profitable businesses, they also aim to remove barriers to entrepreneurship on the extensive margin for women who have not been entrepreneurs before. Self-selection further acts as a revealed preference measure for which women perceive skills-based barriers to economic inclusion: those who are among the lowest-income in their communities and who may have limited opportunities for building skills.

# 3 Experimental Design

## 3.1 Treatment

The program we study is called "Street Business School" (SBS). Coaches from the program teach entrepreneurial and business skills (good business practices). Beyond the potential psychological impact of skills-based training, Street Business School includes some content that is explicitly targeted at psychological empowerment, which we consider a form of entrepreneurial soft

skills.[10] After an orientation day for women who are interested in participating, coaches begin a series of modules as well as individualized coaching.

The first month focuses on teaching skills to start enterprises and increasing women's beliefs in their abilities. Coaches schedule three different sessions lasting 2–3 hours each. The first is called "getting out of your comfort zone" and aims to show participants that they have untapped potential. The second is "identifying business opportunities", which focuses on helping participants identify potential business ideas that may be successful in their communities. The third is called "finding capital and starting small". The program does not provide capital, so this module is designed to help participants understand how to raise capital to start a business through savings, formal and informal loans, and by leveraging smaller, less capital-intensive businesses into larger, more capital-intensive ones. It teaches that even small amounts of money may be enough to start growing an enterprise. SBS considers the lack of capital provision to be critical because the women they work with often face negative shocks that can cause their businesses to close. By teaching women to raise capital rather than providing it, SBS attempts to ensure that women can restart enterprises after their formal engagement with SBS has ended.

In the second month, the program schedules two modules on management practices. The first is bookkeeping and record-keeping, where coaches teach simple techniques for tracking key aspects of the business. The second module is called "market research", and is designed to help participants think about how they can understand the local market before investing their time and resources to start a business. The third month only has one module on skills: business planning. In this module, coaches show participants the steps to planning a business and emphasize the benefits of developing a plan before trying to start a new business.

While the first three months focus on starting and running a business, the last three months of the program focus on teaching skills for firm growth. Month four of the program has two modules. The first is "growing your customer base", which covers topics like actively pursuing customers, customer

[10] See Figure A1 for more detail on the curriculum.

service, and offering promotions. The second module is "money management", which teaches the value of saving and budgeting and provides tools to help participants start separating and prioritizing personal versus business expenses. Month five is entirely given to implementation. Ideally, participants start or continue working on their business in this month using the skills they have learned.

The program ends with a formal, public graduation ceremony to celebrate the achievements of the women who participated. Before the ceremony, women walk through the village in a celebration. At the ceremony, program coaches call women individually and award certificates for successfully completing the program.[11]

Coaches make themselves available for office hours on three designated days: one in the first month of the program, one in the third, and one in the final month. On these days, women can opt to come and receive individualized coaching and ask questions specific to their business. In total, 43% of women in the treatment group attended office hours at least once.[12]

While some of the modules are similar to the personal initiative training as described in Campos et al. (2017) and the ILO's Start and Improve Your Business (SIYB) program, SBS differs in the following aspects. The program explicitly targets women: chants of female empowerment, female role models of program alumni, and dances are a substantial part of the training. All program coaches are women, which potentially facilitates learning by reducing gender barriers. Program participants are not required to have concrete business ideas, literacy, or technical skills.[13] Lastly, SBS falls at the low end of comparable business trainings in terms of the time participants spend in the classroom: a maximum of around 20 hours, including all classroom and potential mentoring sessions. For comparison, McKenzie, Woodruff, et al. (2023) estimates that most traditional business trainings involve 3-12 full days

[11] Women have to attend at least four of the eight modules to receive the certificate.

[12] Only 3% of women attended two individual coaching sessions. None attended all three.

[13] This last requirement is the main difference to the ILO's SIYB program which requires potential entrepreneurs to have the motivation, idea, and skills to start a business. Existing entrepreneurs in SIYB are selected on having a viable business and business experience.

of instruction.

## 3.2 Sampling Frame

Our implementation partner recruited participants in each of our five study locations over several days. Program coaches undertook the same type of mobilization they typically do, but over a slightly larger area to accommodate the sample required for the RCT. Coaches mobilize in a new community by speaking with community leaders and visiting households to inform them about the program. During these efforts, coaches emphasize that the program does not provide any financial assistance but offers skills training and guidance on how to become a successful entrepreneur. Coaches then invite all women interested in the program to an orientation day at a central location. There are no restrictions on who can participate other than gender.

Orientation aims to convince motivated women to enroll in the program. Coaches explain the structure of the six months, the official graduation ceremony, and bring successful alumni to share their stories, but they also emphasize that each woman is responsible for working hard to make her business successful. As such, the women who choose to sign up for the program have detailed information about the types of activities that the program will entail. During the orientation, the RCT project manager also introduced the study and explained that by signing up to participate, the women would be randomly assigned to different groups. She emphasized that all groups would eventually get to participate in the program but that some would be asked to wait until the end of the study.

After the orientation meeting, we enrolled all interested women in the study by collecting their contact details, obtaining media consent, and taking pictures of all women. With these pictures, we print photobooks to identify social network connections between women at baseline, midline, and endline within each location.

Our sampling strategy maintains the self-selection that typically occurs at the start of the program. While self-selection into the program has implica-

tions for the external validity of our results for the entire population of women in Uganda, our results are externally valid for the subset of rural women interested in increasing their income through entrepreneurship. It is also important to note that even in programs that include cash and asset transfers alongside skills, it may be feasible to allow self-selection into the skills-based components of the program.

In total, we enrolled 601 women in five different communities over the course of fifteen months (August 2018–October 2019). We worked in five communities to adequately power our study. Capacity constraints prevented us from working in more than one location at once, which is why we enroll the sample over time. While these logistical considerations were the primary motivators for our sampling frame, it enables us to effectively stratify on location, though the strata are not precisely equal in size. Our sample consists of 101 women in the first location, 153 in the second, 112 in the third, 136 in the fourth, and 99 in the fifth.

## 3.3 Timeline

We conducted three in-person surveys with each woman in our sample: once at baseline in the two weeks following orientation, once at midline in the 2–3 weeks following graduation, and once at endline 12–18 months after graduation. Figure A2 shows a complete timeline including all data collection, implementation of the program, and COVID-19 lockdowns. In the first four locations, all treatment activities finished prior to the first COVID lockdown. The first lockdown delayed graduation in our fifth location. The timeline highlights two important considerations. First, we had originally intended to collect endline data 18 months after the baseline survey, but the COVID-19 lockdown pushed back our timeline. Therefore, our endline survey in all but the first location occurs around two years after baseline. Second, the delay in implementation for the fifth location means that the endline survey occurs around one year after midline (the same spacing as in the first location), whereas locations 2–4 have the endline 18 months after midline (two years

after baseline).

### 3.4 Assignment to Treatment

We implemented a temporarily double blind, individual-level randomization at the end of the baseline survey. The enumerator asked each woman to draw a colored candy from a paper bag. Women received a matching colored paper with information about the time, date, and venue of the first training session. Whereas time and date were the same for both groups, the venue differed depending on treatment status. We did not reveal to participants which venue corresponded to each treatment until the first day of training when they were at the venue. We changed the color of the candies corresponding to each group in each new location and never revealed the correspondence to enumerators.[14] The randomization is only temporarily double blind in the sense that women knew after the first day of training which group they had been assigned to, and all program coaches knew which group they were coaching.

While the control group did not receive any training during the RCT, women in this group took part in a placebo activity during the very first day of the program where we invited them to a designated venue to get to know each other and ask questions to the research staff regarding when they would be eligible to participate in the program. The placebo activity assisted with treatment compliance and allowed us to re-explain the process of randomization so that we addressed any concerns from women in the control group before the program was already underway.

Program coaches took careful attendance to ensure compliance with treatment, particularly during the first month. Monitoring from the coaches largely succeeded in limiting non-compliance. Of participants in the control group, 1.7% entered the treatment. Our main results show average treatment effects based on the randomly assigned treatments, but instrumenting for each participant's actual group with their treatment assignment yields qualitatively similar results (see Appendix D). Of women assigned to treatment, 6.5% never

[14]Note that the number of women in treatment and control is not precisely even as a result of random chance.

attended and 9.6% attended only one session, suggesting that our estimated effects are a lower bound on the effect for compliers.

We check for baseline balance on age, marital status, educational attainment, parental educational attainment, employment status, household size, number of minors, business ownership, network size, and select psychometric measures. We test for selective attrition along the same dimensions. The groups are generally balanced (see Table A1). We observe a slight imbalance on education levels but this is in line with what we would expect by chance given the number of covariates we test. Attrition is correlated with some baseline covariates: women with lower levels of formal education are significantly less likely to drop out of the sample than those with higher levels of education. Younger women are more likely to drop out, however effect sizes are not economically meaningful (see Table A2). Attrition is not correlated with treatment status.

We randomize treatment at the individual level. Individually randomizing assignment to treatment raises concerns about spillovers. If treated women increase the level of competition in local markets then prices may drop, leading to general equilibrium effects that generate negative spillovers to women in the control group. General equilibrium effects would cause us to overstate average treatment effects. Since the randomization we perform is not blind, women in the control group know that they will be able to participate in SBS in the future. This could potentially lead to some women delaying entry into entrepreneurship. Conversely, if treated women transmit some of the skills learned to women in the control group, there may be positive spillovers to women in the control group. Positive spillovers would cause us to under-state effects. In the next section, we describe two data sources that allow us to overcome challenges associated with measuring spillovers with individually-randomized treatment.

## 3.5 Data

Baseline, midline, and endline surveys for women consist of five modules. The first covers household characteristics and socio-economic background. The second asks about household consumption decisions, including information on the overall contributions of household members to household income as well as expenditures in various consumption categories. The third measures business outcomes: established measures of sales and profits, business practices, investment decisions, and expectations about future business growth. Fourth, we collect detailed data on psychometric indicators including locus of control, self-efficacy, grit, and various measures of expectations and aspirations for the future.[15]

Finally, we obtain detailed network data among the women in our sample using the photobooks produced at baseline in each location. Photobooks had 14–16 pages depending on the sample size in each location. Each of those pages displayed pictures of 16 women's faces without any further identifying information.[16] For each location we produced distinct photo books with randomly ordered photographs. We then asked women to look at each page and indicate which women they knew. Identifying a woman triggered a set of questions confirming the identity of the other woman and eliciting information about the type and intensity of interactions. We construct the woman's social network within our sample using the unilateral links that she defines when looking through the photobook. The network data allow us to test for spillovers to women in the control group, as we can observe the number of treated women each woman in the control group is connected to at baseline. We use this variation in baseline connections as a continuous measure of exposure to treatment for women in the control group.

Using photobooks allows us to collect detailed network data among the women in our sample at a relatively low cost, as the survey module on so-

[15] Appendix A contains detailed descriptions of the variables and indices we use in our empirical analysis.

[16] We ensured that all pages had 16 pictures (to effectively randomize) by filling the missing slots with enumerator pictures. All enumerators at baseline were women. We took those network nodes out of our dataset after to construct our village networks.

cial networks still fits within a two-hour survey. Although the speed with which respondents can go through the photo books alleviates concerns about respondent fatigue, the network module came near the end of the survey. Randomly ordering photos and using multiple photo books with pictures placed in a different random order in each location allows us to precisely quantify and correct for respondent fatigue. We estimate the likelihood that a given woman is identified based on the page where her picture appears and the position of her picture on the page in the relevant photo book.[17] We find that women appearing later in the photo book may be identified up to 73% fewer times than those appearing on the first page, and that women appearing lower down on each page may be identified up to 18% fewer times than women appearing at the top (see Table A11.) Given that these differences are distributed randomly, they do not bias our estimates; however, we use estimates of respondent fatigue to re-weight the network data for ease of interpretation.

We complement sales and profit data from our three in-person surveys with high-frequency data collected through SMS surveys. Starting the week after baseline surveys were completed, all women in our sample received a weekly text message on a randomly selected day asking them to report total sales revenue from the previous day. Those who did not have a business were told to reply with zero. We incentivized responses by offering participants UGS 1,000 (USD 0.30) in airtime. Each month, an enumerator supplemented the SMS surveys by calling each woman who had not responded to any SMS survey in the past month. We find that average daily revenues reported in SMS surveys in the month that baseline, midline, or endline surveys took place in-person have a correlation of 0.31 with in-person reports, with 37% of reports being lower in the SMS survey than the in-person survey, 50% being higher in the SMS reports than the in-person reports, and 13% matching precisely. Given high variability in daily revenues, we view the SMS reports as credible.

[17] See Appendix C for more information on the weights.

# 4 Empirical Strategy

Our design permits us to obtain intent to treat (ITT) effects of the program. For an outcome of interest in a given survey round, $O_{it}$, we estimate the ANCOVA specification

$$O_{it} = \alpha + \beta Treat_{it} + \delta_1 X_i + \delta_2 O_{i0} + \epsilon_{it}. \quad (1)$$

$\beta$ gives the ITT effect of participating in the program. We control for a range of pre-specified baseline covariates: age, marital status, household size, the number of minors living in a household, and location strata fixed effects. $O_{i0}$ is the outcome variable at baseline. We are interested in variation in treatment effects over time, so we estimate effects wave by wave rather than pooling data over both survey rounds.[18]

To estimate network-based spillover effects on women in the control group, we combine our ITT estimating equation with a specification similar to that used in Fafchamps, Vaz, and Vicente (2020). This specification allows us to estimate the effect of each treated woman in the baseline social network of a woman in the control group, controlling for the overall size of a woman's baseline social network. The identifying assumption is that, conditional on the size of a woman's social network at baseline, the number of treated women in her network is random. Random assignment to treatment ensures that this identifying assumption holds. The effect of each treated woman in the baseline social network provides an estimate of the spillovers from the program, similar to the approach in Miguel and Kremer (2004). We estimate these effects using the specification

$$y_{it} = \alpha + \eta \sum_p Treated_{ip0} + \delta \sum_p g_{ip0} + \delta_1 X_i + \delta_2 O_{i0} + \epsilon_{it}. \quad (2)$$

[18] Dynamics in treatment effects over time motivate our decision to use an ANCOVA specification rather than the two-way fixed effects specification that we originally pre-registered, which pooled data across rounds. Appendix D shows that effects are qualitatively similar using a range of specifications, including the originally specified two-way fixed effects specification.

In this estimating equation, $\sum_p Treated_{ip0}$ is the weighted sum of treated women identified by woman $i$ in the control group at baseline, where weights correct for each woman's position within the photobook. Weighting in this way allows us to interpret $\eta$ as the effect from each additional baseline connection with a treated woman.[19] $\sum_p g_{ip0}$ controls for the overall number of women in the study to whom a woman is connected at baseline, again using weighted sums of network connections. We control for location fixed effects, marital status, household size, number of children, age, network size, education, and the lagged outcome variable.

Our interest in estimating Equation 2 is twofold. First, we are interested in whether there are spillovers from the program and, if so, which types of network links are most likely to transmit spillovers. Second, we use estimated spillover effects to calculate adjusted average treatment effects. Doing so allows us to provide estimates and confidence intervals for the effect of the program accounting for network-based spillovers to give a sense of the magnitude of the bias in our main effects.[20]

We use the high-frequency SMS data to qualitatively evaluate general equilibrium effects and to estimate the effect of the COVID-19 lockdown on sales revenues. However, response rates to the SMS surveys are lower than response rates for our in-person surveys.[21] We correct for this non-response bias using a two-step procedure. First, we use a LASSO procedure to select the set of baseline covariates, including baseline outcomes, that best predict responding to the SMS survey in the first month of the experiment. We then regress a binary indicator for responding during the first month of the SMS survey on

[19] We calculate weights as follows. Let $w_p$ be -1 multiplied by the coefficient in Table A11 Column (1) that corresponds to the page on which an identified woman appears. Let $w_s$ be -1 multiplied by the coefficient in Table A11 Column (2) that corresponds to the position on the page where an identified woman appears. We compute the woman's weight as $w = 1 + w_p + w_s$ such that women appearing on the first page in the upper left corner have a weight of 1 and women appearing elsewhere in the photo book get up-weighted to adjust for respondent fatigue.

[20] Our confidence intervals account for uncertainty in the traditional intent to treat effect as well as the estimated spillover effect which we compute using the delta method.

[21] See Table A3 for balance in the SMS survey and Table A4 for a description of the correlates of SMS attrition.

the selected baseline variables. We use the resulting coefficients to estimate the probability that each woman in our sample responds to the SMS survey. We weight each woman's responses to the SMS survey by her estimated inverse probability of responding. Throughout our analysis of the SMS data, we show results using both weighted and unweighted responses.

# 5 Results

Our primary estimates of interest are intent to treat effects for a range of pre-specified outcomes that shed light on how effective the program is at promoting women's economic inclusion.

## 5.1 Program Impacts on Skills

Given the nature of the program we study, we first examine program impacts on skills to determine whether women are implementing the practices taught in the entrepreneurship modules, and for how long. Columns (1) and (2) of Table 1 show that the program has initial impacts of 28%–38% on our two indices of "hard skills". The first is an index of business tracking, which combines a number of questions about record keeping and inventory management. The second is an index of price management that combines questions about price negotiations with suppliers, research on competitor pricing, and efforts to attract customers through promotions. Effects survive multiple inference corrections at midline but decline in size and significance at endline. We also examine changes in work hours and find large increases at midline and smaller increases at endline that do not survive multiple inference corrections.

Table 1: ITT Effects: Skills

| | | | | | | Locus of Control | | | Income Aspirations | |
|---|---|---|---|---|---|---|---|---|---|---|
| | (1) Tracking | (2) Price Mgmt. | (3) Work Hours | (4) Goal Setting | (5) Grit | (6) Internal | (7) PO | (8) Chance | (9) Levels | (10) Index |
| *Panel A: Midline (6 months)* | | | | | | | | | | |
| ITT | 0.319** | 0.472*** | 13.507*** | 0.340** | 1.014** | -0.004 | 0.115 | 0.362 | 13807.09 | 0.19** |
| | (0.145) | (0.164) | (4.713) | (0.143) | (0.486) | (0.215) | (0.401) | (0.342) | (246678.64) | (0.08) |
| | [0.099] | [0.050] | [0.050] | [0.089] | [0.109] | [1.000] | [0.990] | [0.683] | [1.000] | [0.089] |
| Observations | 290 | 281 | 228 | 243 | 544 | 543 | 543 | 544 | 430 | 546 |
| Control Mean | 0.957 | 1.012 | 27.909 | 0.643 | 29.488 | 15.836 | -12.914 | -14.645 | 1504624.45 | -0.10 |
| Adj. $R^2$ | 0.167 | 0.114 | 0.265 | 0.145 | 0.108 | 0.029 | 0.146 | 0.094 | 0.225 | 0.197 |
| *Panel B: Endline (18–24 Months)* | | | | | | | | | | |
| ITT | 0.222 | 0.311* | 7.402* | 0.264* | 0.917* | 0.330 | 0.443 | -0.224 | -331680.85* | 0.12 |
| | (0.145) | (0.174) | (4.468) | (0.153) | (0.485) | (0.246) | (0.441) | (0.352) | (194343.19) | (0.08) |
| | [0.485] | [0.455] | [0.485] | [0.485] | [0.406] | [0.485] | [0.495] | [0.525] | [0.485] | [0.485] |
| Observations | 286 | 274 | 229 | 231 | 541 | 540 | 540 | 541 | 441 | 544 |
| Control Mean | 1.133 | 1.239 | 34.850 | 0.688 | 30.094 | 15.801 | -12.121 | -14.191 | 1577941.91 | 0.01 |
| Adj. $R^2$ | 0.060 | -0.011 | 0.145 | 0.047 | 0.110 | 0.043 | 0.037 | 0.046 | 0.062 | 0.116 |

*Note:* Coefficients are ANCOVA estimates that control for the outcome at baseline, the respondent's location, marital status, household size, number of children, age, network size and level of education. ITT shows traditional intent to treat effects of participating in SBS. Tracking is an index about record keeping for the business with a maximum value of 3. Price management is an index about setting prices, running promotions, comparing prices with competitors, and negotiating for better prices with suppliers with a maximum value of 4. Goal setting is an index about setting goals for the business over various time horizons with a maximum value of 3. Work hours is the number of hours the respondent typically works in her main business. Our measures of grit follow Duckworth, Peterson, et al. (2007) and Duckworth and Quinn (2009). We draw our locus of control measures from Levenson (1973). Internal, PO, and Chance is the dimension of the locus of control score. Positive values for Internal, PO and Chance provide evidence of improvements in locus of control measures independent of type. Index is an Anderson index of all outcomes using the swindex command in Stata. We winsorize the income aspirations variable at the 99th percentile. Values in columns marked with heading *Levels* are values in UGX. We report White robust standard errors in parentheses. Romano-Wolf multiple hypothesis test q-values are presented in brackets. Q-values include the same controls as above and are computed within pre-specified families of outcomes.

Columns (4) through (9) of Table 1 show effects on "soft skills": goal setting, grit (Duckworth, Peterson, et al. 2007, Duckworth and Quinn 2009), and locus of control, where we examine internality, powerful others, and chance separately (Levenson 1973). We also measure participants' income aspirations.[22] The program causes sustained improvements in goal setting and grit. Treated women are 53% more likely to set goals for their business at midline than women in the control group. Although the effect at endline does not survive our multiple inference corrections, the magnitude of the effect remains sizable at 38%. We also find sustained improvements in grit of around 3% at midline and endline, though neither survives our multiple inference corrections. We observe no significant changes in locus of control or income aspirations.

Combined, our results indicate that the program is successful at imparting specific hard and soft entrepreneurial skills: price management, working hours, goal setting, and grit. Taking all measures in an index, we find a significant increase at midline that declines slightly and becomes statistically insignificant at endline. This is consistent with roughly half of the skills we measure showing no prolonged improvement. We next consider whether the skills the program imparts are effective at improving women's business performance.

## 5.2 Program Impacts on Income Generation

When examining program impacts, we first consider whether the program allows women to generate independent income. Our first set of outcomes relate to impacts on the extensive margin of business creation and generating any positive profits, then we examine intensive margin impacts on profit and revenue levels. After establishing program impacts on income generation, we proceed to consider effects on income resilience before turning to the effects on investments and household poverty alleviation.

Column (1) of Table 2 shows that the program removes barriers to income generation through entrepreneurship on the extensive margin. At midline, women who participate in the program are 16.4pp (29%) more likely to own a business than women in the control group. The effect declines to 8pp (12%) at endline. Column (2) shows that the program generates large and persistent impacts on the number of businesses owned, with treated women owning 24.5% more businesses than women in the control group at endline. This implies that many treated women open multiple businesses.

[22] We pre-registered two additional psychometric measures: self-efficacy and aspirations regarding social status. For brevity, we present these results in Table A14 and Table A15. Neither shows evidence of significant impacts from the program.

Table 2: ITT Effects on Business Outcomes: Extensive Margin

| | Business Creation | | Main Business | | Other Businesses | All Businesses | |
|---|---|---|---|---|---|---|---|
| | (1) Own a Business | (2) No. Businesses | (3) $>0$ Sales | (4) $>0$ Profits | (5) $>0$ Profits | (6) $>0$ Profits | (7) Index |
| | | | *Panel A: Midline (6 months)* | | | | |
| ITT | 0.164*** | 0.256*** | 0.140*** | 0.183*** | 0.055 | 0.171*** | 0.300*** |
| | (0.038) | (0.066) | (0.041) | (0.041) | (0.034) | (0.040) | (0.082) |
| | [0.010] | [0.010] | [0.010] | [0.010] | [0.119] | [0.010] | [0.010] |
| Observations | 546 | 546 | 547 | 547 | 547 | 547 | 547 |
| Control Mean | 0.566 | 0.833 | 0.420 | 0.486 | 0.179 | 0.521 | -0.051 |
| Adj. $R^2$ | 0.206 | 0.293 | 0.177 | 0.190 | 0.139 | 0.186 | 0.183 |
| | | | *Panel B: Endline (18–24 months)* | | | | |
| ITT | 0.080** | 0.221*** | 0.140*** | 0.095** | 0.083** | 0.103** | 0.258*** |
| | (0.039) | (0.066) | (0.043) | (0.042) | (0.034) | (0.041) | (0.088) |
| | [0.059] | [0.010] | [0.010] | [0.040] | [0.040] | [0.030] | [0.020] |
| Observations | 545 | 544 | 545 | 545 | 545 | 545 | 545 |
| Control Mean | 0.667 | 0.903 | 0.457 | 0.589 | 0.155 | 0.593 | 0.022 |
| Adj. $R^2$ | 0.093 | 0.163 | 0.097 | 0.066 | 0.041 | 0.079 | 0.063 |

*Note:* We winsorize all sales and profit measures at the 99th percentile. Coefficients are ANCOVA estimates that control for the outcome at baseline, the respondent's location, marital status, household size, number of children, age, network size and level of education. ITT shows traditional intent to treat effects. We record revenues and profits for women without a business as zero to preserve balance from randomization. Column (3) presents a dummy variable equal to 1 if the sales for the 3 days prior the survey are greater than 0, columns (4)–(6) present a dummy variable equal to 1 if the self-reported profits for the last month are greater than 0, for either the main business, other businesses or all businesses. Index is an Anderson index of all outcomes using the swindex command in Stata. We report White robust standard errors in parentheses. Romano-Wolf multiple hypothesis test q-values are presented in brackets. Q-values include the same controls as above and are computed within pre-specified families of outcomes.

We estimate treatment effects on income generation separately for the main business a woman reports and all additional businesses she starts.[23]

Columns (3) - (5) show that the businesses that women are starting are productive in the sense that they are significantly more likely to have positive sales and profits than the businesses of women in the control group at both midline and endline. At midline, only 49% of women in the control group generate any profits in their main business although 57% of control women declare having an active business. By contrast, 67% of treated women are able to generate income from their businesses, a 38% increase. At endline, treated women are 16% more likely to have positive profits from their main business and 54% more likely to have positive profits from additional businesses. All of our extensive margin results survive our multiple inference correction at endline, pointing to strong program impacts on participation in income generation through entrepreneurship.

Table 3 shows that the program goes beyond encouraging women to start new businesses: it allows them to run more productive firms. We present effects in levels (UGX) to understand magnitudes while allowing for zeros from women who do not have a business. However, estimating effects in levels is subject to more noise and treatment effects are thus less precisely estimated. Column (1) shows that the training increases sales in the main business by 11% at midline compared to the control group (UGX 4088 or around USD 1.28 over 3 days). At endline, the effect grows to UGX 9380 (USD 2.42), an effect of 21%. Effects on business profits show similar patterns. Column (2) of Table 3 show that adjusted monthly profits in the main business are 24% (USD 4.36) higher at midline and 11% (USD 2.12) at endline. However, part of the decline in the treatment effect on profits in the main business between midline and endline appears to reflect increased activity in other businesses for women in the treatment group. Column (3) shows that adjusted profits in other businesses are 38% (USD 1.81) higher at midline and 50% (USD 2.22) higher at endline relative to women in the control group.

[23] When a woman has more than one business, we ask her to consider the main business as the one that is most profitable at the time of the survey.

Table 3: ITT Effects on Business Outcomes: Intensive Margin

| | Main Business Sales | Main Business Profits | Other Businesses Profits | All Businesses Profits | |
|---|---|---|---|---|---|
| | (1) Levels | (2) Levels | (3) Levels | (4) Levels | (5) Index |
| | | | *Panel A: Midline (6 months)* | | |
| ITT | 4087.60 | 16949.96 | 7015.56 | 27589.10** | 0.18** |
| | (6477.32) | (10892.99) | (5518.12) | (13570.82) | (0.09) |
| | [0.545] | [0.327] | [0.327] | [0.188] | [0.188] |
| Observations | 532 | 525 | 547 | 547 | 547 |
| Control Mean | 37714.29 | 69693.20 | 18589.11 | 88718.68 | -0.05 |
| Adj. $R^2$ | 0.153 | 0.186 | 0.080 | 0.214 | 0.078 |
| | | | *Panel B: Endline (18–24 Months)* | | |
| ITT | 9380.31 | 8214.84 | 8606.68 | 18640.44 | 0.14** |
| | (7649.73) | (10581.50) | (5223.41) | (12726.53) | (0.06) |
| | [0.337] | [0.475] | [0.248] | [0.248] | [0.079] |
| Observations | 538 | 530 | 545 | 545 | 545 |
| Control Mean | 45476.26 | 76934.12 | 17251.55 | 94589.53 | 0.06 |
| Adj. $R^2$ | 0.146 | 0.113 | 0.058 | 0.126 | 0.058 |

*Note:* We winsorize all sales and profit measures at the 99th percentile. Coefficients are ANCOVA estimates that control for the outcome at baseline, the respondent's location, marital status, household size, number of children, age, network size and level of education. ITT shows traditional intent to treat effects. Column (1) presents the reported sales for the 3 days prior the survey, columns (2) to (4) present the self-reported profits for the last month for either the main business, other businesses, or all businesses. Values in columns marked with heading *Levels* are values in UGX. We report White robust standard errors in parentheses. Index is an Anderson index of all outcomes using the swindex command in Stata. Romano-Wolf multiple hypothesis test q-values are presented in brackets. Q-values include the same controls as above and are computed within pre-specified families of outcomes.

Despite consistently positive effects, none are statistically significant at endline and none survive multiple inference corrections at midline or endline. However, when we combine all measures into an index we observe a sustained, positive effect that survives multiple inference corrections at the 10% level at endline. Sustained, positive effects on the index measure of intensive margin business performance combined with sustained effects on the extensive margin indicate that the program succeeds in helping women start businesses and run them more successfully than they would in the absence of the program.

One concern could be that the program is simply inducing women to switch into self-employment, even when doing so would not increase their income. Column (1) of Table 4 shows that the program leads to an 8pp (11%) increase in the likelihood of being employed in any form at midline, though the effect drops to a statistically insignificant 3pp at endline.[24] Interestingly, column (2) shows that treated women are half as likely to engage in wage work at midline and 36% less likely to engage in wage work at endline than women in the control group, although the endline result does not survive multiple inference corrections. These results are consistent with some women discovering that self-employment is not optimal and exiting, but many women remaining self-employed on a consistent basis after participating in the program. We observe no significant changes in the wage at which women report that they would switch from self-employment to wage work.

[24]Note that this analysis was not pre-registered.

Table 4: ITT Effects on Employment Outcomes

| | Employment | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| | Any | Salaried | Outside Wage |
| *Panel A: Midline (6 months)* | | | |
| ITT | 0.08** | -0.08*** | 36870.754 |
| | (0.03) | (0.03) | (26375.090) |
| | [0.020] | [0.010] | [0.149] |
| Observations | 547 | 547 | 539 |
| Control Mean | 0.72 | 0.16 | 3.95e+05 |
| Adj. $R^2$ | 0.175 | 0.140 | 0.164 |
| *Panel B: Endline (18–24 Months)* | | | |
| ITT | 0.03 | -0.05* | 6154.552 |
| | (0.03) | (0.03) | (23810.000) |
| | [0.624] | [0.178] | [0.782] |
| Observations | 545 | 545 | 535 |
| Control Mean | 0.81 | 0.14 | 3.60e+05 |
| Adj. $R^2$ | 0.064 | 0.026 | 0.153 |

*Note:* We winsorize Outside Wage at the 99th percentile. Coefficients are ANCOVA estimates that control for the outcome at baseline, the respondent's location, marital status, household size, number of children, age, network size and level of education. ITT shows traditional intent to treat effects. Any Employment is a binary variable equal to one if the woman reports being either employed by someone else or being self-employed. Salaried Employment is a binary varibale equal to one if the woman reports being employed by someone else. Outside Wage is the salary a woman requires to prefer working in an important private company instead of having her own shop. Index is an Anderson index of all outcomes using the swindex command in Stata. We report White robust standard errors in parentheses. Romano-Wolf multiple hypothesis test q-values are presented in brackets. Q-values include the same controls as above and are computed within families of outcomes.

## 5.3 Impacts on Income Volatility

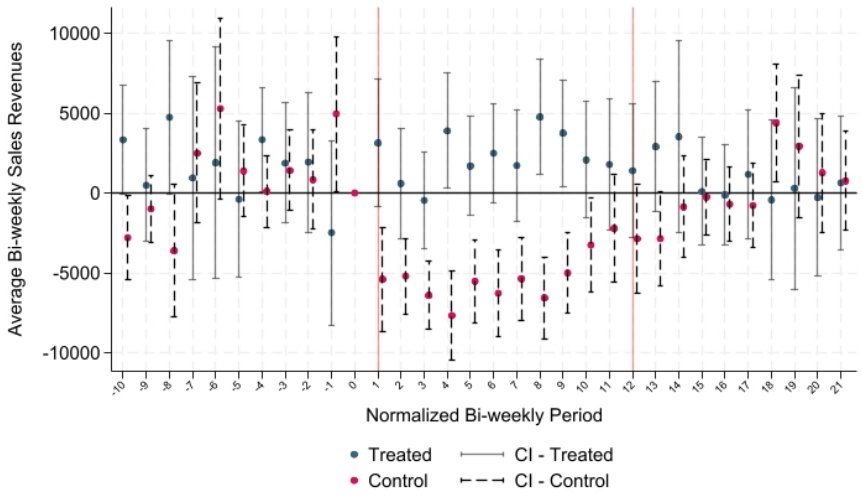
Entrepreneurship may be a way to both raise incomes and lower income volatility. To causally estimate women's capacity to cope with negative economic shocks, we exploit the unanticipated COVID-19 lockdown in Uganda.[25] Figure 1 shows bi-weekly event study estimates using revenue data from our SMS survey. All effects are relative to the two weeks prior to the first Ugandan lockdown, estimated separately for the control group and treatment group. As such, each estimate is normalized by revenues in each group in the two weeks prior to the lockdown. Doing so allows us to see how each group was affected by the COVID-19 lockdown, but the figure does not show treatment effects. We estimate effects 5 months before the lockdown, during the 6-month lockdown, and for 4.5 months after the lockdown was lifted.[26]

Prior to the lockdown, revenues of firms in both the treatment and control group are stable relative to the two weeks prior to the lockdown. While there are fluctuations in reported revenue, most bi-weekly periods before the lockdown exhibit no statistically significant differences for women in either treatment group. Such patterns confirm the validity of our event study design: the COVID-19 lockdown was not anticipated.

The lockdown causes immediate, significant reductions in revenues among women in the control group that persist over multiple months. Women in the control group have lower sales by around UGX 5,000 (USD1.29) per day until the final month of the lockdown. Strikingly, women in the treatment group do not experience any significant reductions in sales: we cannot reject that their revenues are the same as the two weeks prior to lockdown in most periods, and in three lockdown periods average revenues are significantly higher than the two weeks prior to lockdown. The difference in the effect of the lockdown between the control and treatment group is statistically significant up until the last month of the lockdown, when women in the control group begin to recover to pre-lockdown revenue levels.

[25] Note that this analysis was not pre-registered.

[26] The first COVID-19 case in Uganda was detected on March 21, 2020. On March 31, 2020 the president announced a 14-day lockdown effective April 1, 2020 which suspended all public and private transportation, closed all but essential businesses, and suspended all forms of public gatherings. Authorities started lifting select components of the lockdown 7 weeks after. Uganda's school closure lockdown and curfew were the longest worldwide.




*Note:* Each point represents a coefficient from a treatment group-specific event study where the reference period is the two weeks prior to the first COVID-19 lockdown in Uganda, using data from weekly SMS surveys. Therefore, estimates for each treatment group are normalized by the reference period mean for the group and cannot be directly interpreted as treatment effects. The SMS survey elicits daily revenues for the day preceding the survey. Each woman receives the survey on a randomly assigned day of the week. We winsorize at the 99th percentiles, weight all responses by the inverse probability of a woman responding as predicted by baseline covariates selected using LASSO, then take means at the bi-weekly level. The red lines indicate the beginning and end of the first nationwide Ugandan COVID-19 lockdown.

Figure 1: Reactions to COVID-19 Lockdown by Treatment Group

These results show that teaching rural women the necessary skills for productive entrepreneurship can help them navigate the consequences of economic shocks. Combined with our results on business outcomes, results on the COVID-19 lockdown demonstrate that the program is effective at helping women start income generating businesses that can survive major economic shocks. Having established program impacts on economic inclusion, we now consider whether the program is an effective tool for poverty alleviation.

## 5.4 Program Impacts on Income Use

Our final set of results is motivated by our focus on economic inclusion programs as tools for women's economic autonomy and poverty alleviation. As such, we examine how women use their income and what impact the program has on household outcomes.

Table 5: ITT Effects on Savings and Re-Investments Outcomes

| | Savings | Business Assests | Investments in Other Businesses | |
|---|---|---|---|---|
| | (1) Levels | (2) Levels | (3) Levels | (4) Index |
| | *Panel A: Midline (6 months)* | | | |
| ITT | 1193.12 | 3558.45 | 11562.89 | 0.09 |
| | (21159.68) | ( 21010.23) | (15108.80) | (0.08) |
| | [0.970] | [0.970] | [0.832] | [0.634] |
| Observations | 529 | 547 | 547 | 547 |
| Control Mean | 166976.10 | 119889.56 | 47501.17 | 0.06 |
| Adj. $R^2$ | 0.290 | 0.325 | 0.087 | 0.351 |
| | *Panel B: Endline (18–24 Months)* | | | |
| ITT | 26805.09 | 17021.32 | 27773.52** | 0.20** |
| | (25150.95) | ( 24990.66) | (12058.10) | (0.08) |
| | [0.455] | [0.455] | [0.050] | [0.040] |
| Observations | 532 | 545 | 545 | 545 |
| Control Mean | 162703.16 | 125942.20 | 29836.05 | 0.00 |
| Adj. $R^2$ | 0.194 | 0.188 | 0.029 | 0.212 |

*Note:* We winsorize all savings and investment measures at the 99th percentile. Coefficients are ANCOVA estimates that control for the outcome at baseline, the respondent's location, marital status, household size, number of children, age, network size and level of education. ITT shows traditional intent to treat effects. Savings is the total amount held in all financial savings instruments. Business assets is the estimated monetary value of all assets held in the main business. Investments in other businesses is the total estimated monetary value of all investments in businesses other than the main business in the last 6 months. Values in columns marked with heading *Levels* are values in UGX. Index is an Anderson index of all outcomes using the swindex command in Stata. We report White robust standard errors in parentheses. Romano-Wolf multiple hypothesis test q-values are presented in brackets. Q-values include the same controls as above and are computed within pre-specified families of outcomes.

Effects on savings and investment show no significant increases at midline, in part due to low statistical power and in part because different women may still be saving versus already investing in their businesses. Table 5 shows that the value of assets in the main business at midline is only 3% (USD .92) higher at midline relative to the control group, on average. By endline, that increases to 14% (USD 4.39), although estimates are noisy and not statistically significant. However, the value of investments in other businesses is nearly double the control group mean at endline, increasing by around USD 7.16 on average and surviving our multiple inference corrections. Results on savings and the value of assets in the main business are also sizable at endline, though not statistically significant. All three individual results combine to produce an overall positive and significant effect in the combined index, indicating that the program allows women to save and invest more.

The results on savings and investments coupled with the results on business outcomes indicate two important patterns. First, the program appears to be effective at helping women learn how to raise capital and identify profitable business opportunities to grow income in the medium-run. Second, women retain sufficient control over the income they generate to invest in the profitable opportunities that they identify.

Turning to household-level outcomes, Table 6 shows few significant effects on our three measures of consumption expenditures at midline and endline. Columns (1) and (2) show effects on overall levels of household consumption spending and the woman's contribution to household expenditures. Effects are imprecisely estimated and do not survive our multiple inference corrections, but they are also economically small.

Column (4) shows that there are large, significant increases in food insecurity at midline. The adjusted likelihood that participants did not have enough to eat more than once in the six months preceding the survey increases by 11.1pp, a 41% increase. These effects on food insecurity disappear by endline, suggesting that women may be reducing consumption in the short-run while building the capital necessary to start and build their businesses. We observe imprecise but relatively large negative effects on remittances received at midline, potentially suggesting some reductions in outside support. Our results on household outcomes suggest that programs like the one we study do not substantially reduce poverty in the short- to medium-run, a result that falls in line with others in the literature (e.g., Bandiera, Burgess, et al. (2017)).

Table 6: ITT Effects on Household Outcomes

| | Daily Expenditure | | | | Remittances | |
|---|---|---|---|---|---|---|
| | (1) HH Levels | (2) Participant Levels | (3) MUE | (4) Food Insecurity | (5) Levels | (6) Index |
| | | | *Panel A: Midline (6 months)* | | | |
| ITT | -184.35 | -644.36** | 0.075 | 0.111*** | -12313.70 | 0.07 |
| | (458.00) | (315.42) | (0.086) | (0.040) | (8308.04) | (0.07) |
| | [0.644] | [0.168] | [0.604] | [0.040] | [0.465] | [0.604] |
| Observations | 544 | 541 | 481 | 543 | 528 | 546 |
| Control Mean | 7205.16 | 3565.36 | -0.026 | 0.270 | 34309.80 | -0.12 |
| Adj. $R^2$ | 0.198 | 0.041 | 0.074 | 0.124 | 0.169 | 0.041 |
| | | | *Panel B: Endline (18–24 Months)* | | | |
| ITT | -123.61 | -171.88 | 0.126 | 0.016 | 760.15 | 0.11 |
| | (447.25) | (297.17) | (0.096) | (0.040) | (6417.55) | (0.07) |
| | [0.980] | [0.970] | [0.604] | [0.980] | [0.980] | [0.574] |
| Observations | 544 | 540 | 476 | 543 | 526 | 535 |
| Control Mean | 6929.52 | 3443.70 | -0.076 | 0.310 | 25547.62 | -0.17 |
| Adj. $R^2$ | 0.183 | 0.013 | 0.047 | 0.091 | 0.072 | 0.094 |

*Note:* We winsorize daily expenditures, MUE, and remittances at the 99th percentile. Coefficients are ANCOVA estimates that control for the outcome at baseline, the respondent's location, marital status, household size, number of children, age, network size and level of education. ITT shows traditional intent to treat effects. We calculate the MUE using consumption expenditures over the past week on seventeen food items, following the methods outlined in Ligon (2020). Higher values of the MUE indicate higher marginal utilities of expenditure, indicating that households are worse off. Food insecurity is a binary variable equal to one if the woman reports not having enough food more than once over the six months before the survey. Column (2) is included in column (1), however, we wanted to show the effect on the participating women expenditures as the information on the household corresponds to estimates the respondent thinks other members of the household have. Values in columns marked with heading *Levels* are values in UGX. Index is an Anderson index of all outcomes using the swindex command in Stata. We report White robust standard errors in parentheses. Romano-Wolf multiple hypothesis test q-values are presented in brackets. Q-values include the same controls as above and are computed within pre-specified families of outcomes.

## 5.5 Quantifying Spillovers

Given the potential for spillovers to women in the control group due to our individual-level randomization, we use baseline social network data to directly estimate spillovers. We find evidence of positive, significant network-based spillovers to women in the control group. Table 7 shows estimated spillovers at endline for our skills index, owning a business, earning positive profits, our extensive and intensive margin indices of business performance, employment, and wage work. Panel A shows spillovers estimated by accounting for any network link between a woman in the control group and a treated woman at baseline. Panel B shows spillovers for women connected at baseline through friendship links, and panel C shows spillovers for women connected at baseline through business links.

There is little evidence of positive spillovers in skills. None of estimated effects are statistically significant and they are all relatively small. However, we observe positive spillovers on a range of business outcomes. Having baseline business links with a treated woman increases the likelihood of owning a business by 5.7pp, 9%. It increases the likelihood of earning positive profits by 13% and the coefficient on the intensive margin business performance index is positive, though not statistically significant. Business links to treated women also reduce the likelihood of wage work by 32%, with no effects on overall employment. Strikingly, these effects are more than twice the size of the effects for women connected through friendship links at baseline, suggesting that there may be direct, positive economic spillovers between businesses.

Given that business links with treated women lead to positive spillovers, we adjust all estimated average treatment effects using spillovers specifically from business links.[27] At baseline, the average woman in the control group has 0.26 business links with a treated woman. We multiply the estimated spillovers by 0.26 and add them to our estimated average treatment effects (see Table A8, Table A5, Table A6, Table A7, and Table A9). Adjusted effects on skills and household outcomes do not change substantially when accounting for spillovers. However, extensive margin business effects increase by 7%–20% at endline and intensive margin effects increase by up to 38% when adjusting for spillovers. Savings and investment effects increase up to 31%. Our adjusted effects speak to substantial economic spillovers from the program.

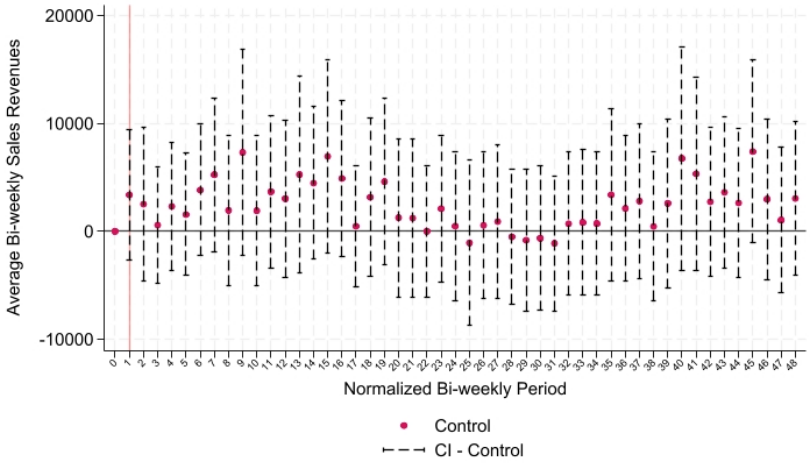
[27] We can provide tables showing estimated spillover effects from business links on all outcomes upon request.

Table 7: Spillover Effects on Business Outcomes at Endline

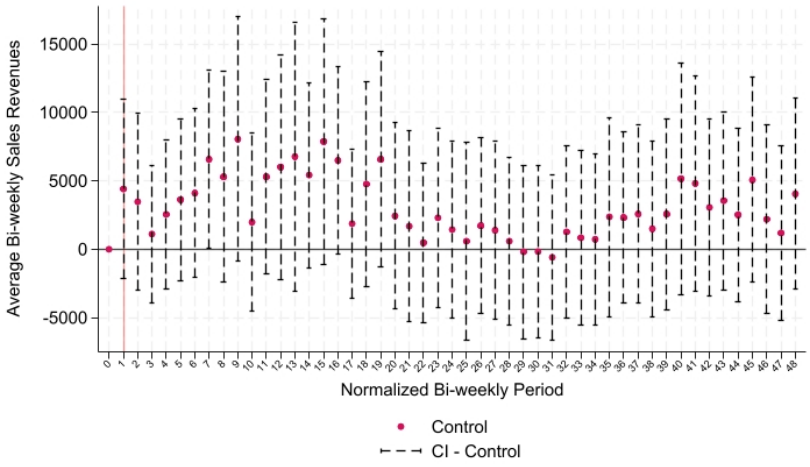
| | Control group only | | | | | | |
|---|---|---|---|---|---|---|---|
| | Skills | Business | Profit | Extensive Margin | Intensive Margin | Employment | |
| | (1) Index | (2) Ownership | (3) $> 0$ | (4) Index | (5) Index | (6) Any | (7) Salaried |
| *Panel A: Any Network Link* | | | | | | | |
| Link - Treated | -0.003 | 0.028** | 0.039** | 0.018 | -0.034 | 0.015 | -0.019* |
| | (0.030) | (0.014) | (0.015) | (0.029) | (0.029) | (0.011) | (0.010) |
| Observations | 253 | 253 | 253 | 253 | 253 | 253 | 253 |
| Control Mean | -0.182 | 0.493 | 0.420 | -0.147 | 0.024 | 0.710 | 0.217 |
| Adj. $R^2$ | 0.158 | 0.133 | 0.089 | 0.073 | 0.045 | 0.037 | 0.010 |
| *Panel B: Friendship Link* | | | | | | | |
| Link - Treated | 0.013 | 0.019 | 0.021 | 0.037 | 0.026 | 0.017 | -0.002 |
| | (0.030) | (0.016) | (0.017) | (0.048) | (0.031) | (0.014) | (0.014) |
| Observations | 253 | 253 | 253 | 253 | 253 | 253 | 253 |
| Control Mean | -0.026 | 0.635 | 0.571 | 0.005 | 0.019 | 0.782 | 0.147 |
| Adj. $R^2$ | 0.158 | 0.124 | 0.072 | 0.074 | 0.040 | 0.036 | 0.000 |
| *Panel C: Business Link* | | | | | | | |
| Link - Treated | 0.081 | 0.057* | 0.070** | 0.080 | 0.129 | 0.025 | -0.051*** |
| | (0.078) | (0.030) | (0.032) | (0.094) | (0.094) | (0.028) | (0.014) |
| Observations | 253 | 253 | 253 | 253 | 253 | 253 | 253 |
| Control Mean | -0.031 | 0.627 | 0.555 | -0.021 | 0.058 | 0.786 | 0.159 |
| Adj. $R^2$ | 0.162 | 0.130 | 0.081 | 0.076 | 0.056 | 0.035 | 0.013 |

*Note:* Coefficients are ANCOVA estimates that control for the outcome at baseline, and baseline network size. All Indeces are Anderson indeces of the respective outcome groups using the swindex command in Stata. Columns 2-3 are binary indicators. Any Employment is a binary variable equal to one if the woman reports being either employed by someone else or being self-employed. Salaried Employment is a binary varibale equal to one if the woman reports being employed by someone else. We report White robust standard errors in parentheses. These results correspond to spillover effects for the control group at endline.

Networks are only one potential channel for spillovers. We use high-frequency SMS surveys to provide suggestive evidence that the program does not lead to negative spillovers for women in the control group. Figure 2 shows that there are no significant changes in trends for revenues among women in the control group over the period of the experiment.




(a) Unweighted




(b) Weighted

*Note:* The SMS survey elicits daily revenues for the day preceding the survey. Each woman receives the survey on a randomly assigned day of the week. We winsorize at the 99th percentiles, weight all responses by the inverse probability of a woman responding as predicted by baseline covariates selected using LASSO, then take means at the bi-weekly level.

Figure 2: SMS Bi-weekly Average Reported Sales

Weighted revenues reported in SMS surveys for women in the control group show no significant differences in any bi-weekly period. Had the treatment caused negative effects for women in the control group, we would expect to see persistent declines in revenues for women in the control group over time as revenues grow for treated women. The high-frequency SMS data provide reassurance that the treatment did not lead to substantial general equilibrium effects that negatively impacted women in the control group.

The final channel through which negative spillovers may have occurred is on the extensive margin: women in the control group who did not have a business at baseline may have delayed their entry into entrepreneurship because they knew that they could participate in SBS at the end of the study. Although we cannot fully rule out this channel for negative spillovers, it is worth noting that a third of women in the control group who did not have a business at baseline report having a business at midline, and half open a business by endline.

# 6 Discussion and Conclusion

Our study shows that skills are a relevant barrier to economic inclusion among a self-selected sample of rural women. A program that teaches entrepreneurship and business skills is effective at increasing the number of women entrepreneurs, increasing the number of businesses that each woman runs, and improving the profitability and resilience of their businesses relative to the control group. Despite positive impacts on a range of entrepreneurship outcomes, we find no evidence that the program reduces poverty over the period of our study.

Our results show that skills-based programs for women can be successful in increasing women's incomes in rural areas and helping women cope with negative shocks, two core objectives of rural development efforts. We find that treated women's revenues remain stable throughout the first COVID-19 lockdown in Uganda, while revenues among control women drop significantly. Although our results suggest that entrepreneurship can significantly improve the resilience of low-income women in rural areas, it is worth noting that there are limitations to the external validity of our results. The COVID-19 lockdown affected the entire country and severely restricted the movement of goods and people, but it did not directly affect agricultural productivity. Events like droughts that reduce agricultural productivity may be more difficult for rural entrepreneurs to navigate because they can lead to broad-based reductions in local demand. Starting a different business or shifting the focus of a business may be less effective at coping with reductions in local demand.

The program has impacts beyond the women in the treatment group. We find evidence of significant, positive spillover effects to women in the control group through business linkages. Interestingly, these spillovers appear to be driven by economic activities between businesses: we find little evidence that business skills also get transmitted from women in the treatment group to women in the control group. The social network data that allows us to measure and adjust for these spillovers provides a novel way for studies limited to individual-level randomization to assess spillovers.

Although our study highlights the importance of skills-based barriers for women's economic inclusion, the women in our sample are self-selected in contrast to studies on graduation from poverty programs, which typically evaluate on representative samples of ultra-poor women (e.g., Banerjee, Duflo, Goldberg, et al. 2015, Blattman et al. 2016, Bandiera, Burgess, et al. 2017, Banerjee, Duflo, and Sharma 2021). This difference raises important directions for future policy-relevant work. Are the returns to skills lower when self-selection is not permitted, or do self-targeting errors limit returns to skills-based programming? Can the skills-based components of multifaceted programs allow for self-targeting, even if cash and asset transfers require alternative targeting methodologies? Understanding how to maximize the effectiveness of skills-based interventions is central to building effective and sustainable programs for women's economic inclusion.

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# For Online Publication

# A Appendix A - Variable definitions

## A.1 Baseline Covariates

All covariates described in this section are values the respondent reported at the baseline survey.

### A.1.1 Demographics

- **Location**: Set of dummy variables equal to one for the location where the respondent was enrolled in the study.
- **Marital Status**: Set of dummy variables that indicate the participant's marital status. Participants answer whether they are married, single, widowed, or divorced. The categories are mutually exclusive, and we exclude the category "single" because it is the largest group.
- **Household Size**: Set of dummy variables that indicate the number of people (adults and children) who regularly eat and sleep in the respondent's household.
- **Number of Children**: Set of dummy variables that indicate the number of dependents under 18 years old living in the respondent's household.
- **Age**: The respondent's age is calculated as the difference between the year the respondent was born and when the baseline survey was conducted. When the age is missing, we imputed the median value and control for a dummy flagging the change.
- **Education**: Set of dummy variables that indicate the participant's highest educational attainment. Participants can have no education, at least primary or secondary education, or a higher education degree. The excluded category is whether the participant has more than secondary education.
- **Father's Education**: Set of dummy variables that indicate the participant's father's highest educational attainment. The father can have no education, at least primary or secondary education, or a higher education degree. The excluded category is whether they have more than secondary education.
- **Mother's Education**: Set of dummy variables that indicate the participant's mother's highest educational attainment. The mother can have no education, at least primary or secondary education, or a higher education degree. The excluded category is whether they have more than secondary education.
- **Employed**: Binary variable equal to one if the respondent answers yes to the question, "Are you presently employed?", zero if the respondent answers no, and missing if the respondent does not know or chooses not to answer.

### A.1.2 Network Measures

- **Network size**: Total number of connections the participant recognizes from the location's photo book used during the survey, weighted by where the picture of the recognized person is inside the photo book.
- **Any Link to Treated Women**: Total number of treated women the participant recognized from the location's photo book used during the survey, weighted by where the picture of the recognized person is inside the photo book.
- **Friendship Link to Treated Women**: Total number of treated women the participant recognized from the location's photo book used during the survey, and describe their relationship as being friends, weighted by where the picture of the recognized person is inside the photo book.
- **Business Link to Treated Women**: Total number of treated women the participant recognized from the location's photo book used during the survey and report conducting business with them, weighted by where the picture of the recognized person is inside the photo book.

## A.2 Business Outcomes

- **Own a business**: Binary variable equal to one if the respondent answers yes to the question, "Do you currently own a business or engage in self-employment in any way?", zero if the respondent answers no, and missing if the respondent does not know or chooses not to answer.
- **No. Businesses**: Count of the number of businesses the respondent reports operating, including her main business and all other businesses.
- $> 0$ **Sales**: Binary variable equal to one if the respondent reports having greater than 0 sales during the three days before the survey in their main business. When the respondent has missing information for a day, we assign the observation a missing value. In case the respondent has no business, we replace it with 0.
- $> 0$ **Profits - Main Business (MB)**: Binary variable equal to one if the respondent reports having greater than 0 profits for the last month in their main business. When the respondent has missing information, we assign the observation a missing value. In case the respondent has no business, we replace it with 0.
- $> 0$ **Profits - Other Businesses (OB)**: Binary variable equal to one if the respondent reports having greater than 0 profits for the last month across all her other businesses. When the respondent has missing information, we assign the observation a missing value. In case the respondent has no business, we replace it with 0.
- $> 0$ **Profits - All Businesses**: Binary variable equal to one if the respondent reports having greater than 0 profits for the last month across all the businesses she runs.

When the respondent has missing information, we assign the observation a missing value. In case the respondent has no business, we replace it with 0.

- **Sales (Levels)**: The amount of the revenue reported each day for the five best-sold items in the respondent's business in the three days before the survey in UGX. We winsorize sales at the 99th percentile. We use the midpoint of intervals whenever the respondent cannot provide a number and missing if the respondent does not know or chooses not to answer. In case the respondent has no business, we replace it with 0.

- **Profits (Levels) - MB**: The amount of profits earned in the respondent's main business in the last month in UGX. We winsorize profits at the 99th percentile. We use the midpoint of intervals whenever the respondent cannot provide a number and missing if the respondent does not know or chooses not to answer. In case the respondent has no business, we replace it with 0.

- **Profits (Levels) - OB**: The amount of profits earned in other businesses owned by the participant in the last month in UGX. We winsorize profits at the 99th percentile. We use the midpoint of intervals whenever the respondent cannot provide a number and missing if the respondent does not know or chooses not to answer. In case the respondent has no business, we replace it with 0.

- **Profits (Levels) - All Businesses**: The sum of the amount of profits earned in the main business and all other businesses owned by the participant in the last month in UGX. We winsorize profits at the 99th percentile. We find missing if the respondent does not know or chooses not to answer. In case the respondent has no business, we replace it with 0.

## A.3 Savings and Investment Outcomes

- **Savings (Levels)**: The monthly amount the respondent reported saving in UGX. We winsorize savings at the 99th percentile. For daily responses, we multiply by 30.5 to estimate a monthly savings amount. We use the midpoint of intervals whenever the respondent cannot provide a number and missing if the respondent does not know or chooses not to answer. When the respondent reports not saving or not having a business, we replace it with 0.

- **Business Assets (Levels)**: The total value of all assets the respondent's business owns in UGX. We winsorize at the 99th percentile. When the respondent has missing information or chooses not to answer, we assign the observation a missing value.

- **Investments in OB (Levels)**: The amount of money in UGX the respondent invested in all other businesses during the last six months to purchase additional assets or increase her capital stock. We winsorize sales at the 99th percentile. When the respondent has missing information for all the businesses or chooses not to answer, we assign the observation a missing value.

## A.4 Explored Mechanisms

### A.4.1 Business Practices

- **Tracking**: Score that can take values between 0 and 3. The tacking score depends on the number of "yes" responses to the questions: (1) have a system for keeping track of their business activities, (2) keep track of which customers buy from them on credit, and (3) keep track of how much inventory they have. Set to missing if the respondent does not answer any of the three questions.

- **Price Management**: Score that can take values between 0 and 4. The price management score depends on the number of "yes" responses to the questions: (1) compared alternative suppliers for their business in the past six months, (2) visited a competitor to see what products they were offering in the last six months, (3) tried to negotiate a lower price with their supplier in the last six months, and (4) offered special prices to attract more clients in the last six months. Set to missing if the respondent does not answer any of the four questions.

- **Work Hours**: Number of hours per week the respondent takes care of her business personally. In case the respondent has no business, we replace it with 0.

- **Goal Setting**: Score that can take values between 0 and 3. The goal-setting score depends on the number of "yes" responses to the questions: (1) have a goal for how much profit they want to make in the next month, (2) have a goal for how much profit they want to make in the next year, and (3) know how much they can spend in business expenses in the next year. Set to missing if the respondent does not answer any of the three questions.

### A.4.2 Psychometric Measures

- **Grit**: Score that can take values between 8 and 40. The grit score depends on the sum of the questions: (1) I stay interested in my goals, even if they take a long time (months or years) to complete, (2) I think about my work even in my dreams and daydreams, (3) I work very hard. I keep working when others stop to take a break, (4) setbacks do not discourage me. I do not give up easily, (5) every day, I try to do one thing better than I did the day before, (6) I am constantly asking other people for feedback about how I can improve, (7) I am never fully satisfied with my performance, and (8) I finish whatever I begin. All questions are on a scale of 1–5, where one is "not at all like me" and five is "completely like me." Higher responses correspond to higher levels of grit. We have no missing responses for these questions.

- **Self-Efficacy**: Score that can take values between 10 and 50. The self-efficacy score depends on the sum of the questions: (1) I can always manage to solve difficult problems if I try hard enough, (2) if someone opposes me, I can find the means and ways to get what I want, (3) it is easy for me to stick to my aims and accomplish my goals, (4) I am confident that I could deal efficiently with unexpected events, (5) thanks to my resourcefulness, I know how to handle unforeseen situations, (6) I can solve most

problems if I invest the necessary effort, (7) I can remain calm when facing difficulties because I can rely on my coping abilities, (8) When I am confronted with a problem, I can usually find several solutions, (9) if I am in trouble, I can usually think of a solution, and (10) I can usually handle whatever comes my way. All questions are on a scale of 1–5, where one is "not at all like me" and five is "completely like me." Higher responses correspond to higher levels of self-efficacy. We have no missing responses for these questions.

- **Locus of Control - Internal**: Score that can take values between 4 and 20. The internality score depends on the sum of the questions: (1) when I make plans, I am almost certain to make them work, (2) I am usually able to protect my personal interests, (3) when I get what I want, it is usually because I worked hard for it, and (4) my life is determined by my own actions. All questions are on a scale of 1–5, where one is "disagree a lot" and five is "agree a lot". Higher responses indicate greater levels of agreement with statements indicating high levels of internality. We have no missing responses for these questions.

- **Locus of Control - PO**: Score that can take values between 5 and 25. The powerful others score depends on the sum of the questions: (1) I feel like what happens in my life is mostly determined by powerful people, (2) my life is chiefly controlled by powerful others, (3) people like myself have very little chance of protecting our personal interests when they conflict with those of strong pressure groups, (4) getting what I want requires pleasing those people above me, and (5) in order to have my plans work, I make sure that they fit in with the desires of people who have power over me. All questions are on a scale of 1–5 where one is "disagree a lot" and five is "agree a lot". Higher responses indicate greater levels of agreement with statements indicating high levels of belief that powerful others control the respondent's life. We multiply all variables by -1 so that higher scores indicate a more internalized locus of control. We have no missing responses for these questions.

- **Locus of Control - Chance**: Score that can take values between 5 and 25. The chance score depends on the sum of the questions: (1) to a great extent my life is controlled by accidental happenings, (2) often there is no chance of protecting my personal interests from bad luck happenings, (3) when I get what I want, it's usually because I'm lucky, (4) I have often found that what is going to happen will happen, and (5) it's not always wise for me to plan too far ahead because many things turn out to be a matter of good or bad fortune. All questions are on a scale of 1–5 where one is "disagree a lot" and five is "agree a lot". Higher responses indicate greater levels of agreement with statements indicating that many things in life are due to chance, so we multiply all variables by -1 so that higher scores then indicate a more internalized/self-driven locus of control. We add up the five questions to generate a chance score for each participant. We have no missing responses for these questions.

- **Income Aspirations (Levels)**: Difference between the reported values to the questions (1) "What income do you want to have per month in 10 years?" and (2) "What

income do you currently have per month?" We winsorize income aspirations at the 99th percentile.

- **Social Status Aspirations**: Difference between the reported values in a scale from the questions: (1) "What level of social status do you want to have in 10 years?" and (2) "What level of social status do you have today?". The level of social status was selected from the image of a ladder by the participants, enumerated from 0 to 9.

## A.5 Household Outcomes

- **Daily HH Expenditure (Levels)**: The sum of all the daily contributions to household expenses for all the adult members living in the respondent's household in UGX. We winsorize daily expenses at the 99th percentile. If answered in a monthly amount, we convert it to a daily total by dividing it by 30.5. We use the midpoint of intervals whenever the respondent cannot provide a number and missing if the respondent does not know or chooses not to answer.

- **Daily Participant Expenditure (Levels)**: The respondent's daily contribution to household expenses in UGX. We winsorize daily expenses at the 99th percentile. If answered in a monthly amount, we convert it to a daily total. We use the midpoint of intervals whenever the respondent cannot provide a number and missing if the respondent does not know or chooses not to answer.

- **MUE**: The marginal utility of expenditures calculated using consumption expenditures over the past week on seventeen food items, following the methods outlined in Ligon (2020).

- **Food Insecurity**: Binary variable equal to one if the respondent answers "A lot of times (at least 5 or 6)" or "some times (2 to 4 times)" to the question, "During the last six months, how many times, if any, did you experience not having enough food to eat?". The variable equals 0 if the respondent answers "only once" or "never" and is missing if the respondent does not know or chooses not to answer.

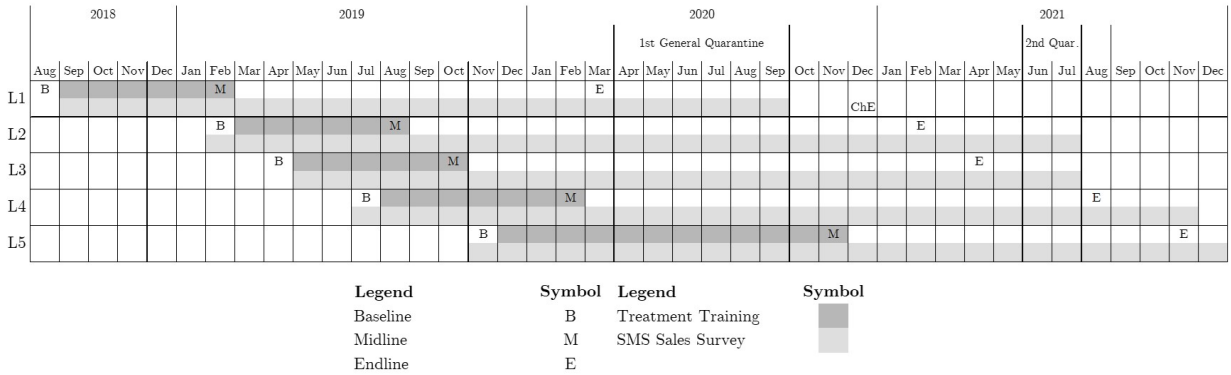
- **Remittances (Levels)**: The amount of money or value of goods the household received from family members or friends during the last month in UGX. We winsorize remittances at the 99th percentile. For daily responses, we multiply by 30.5 to estimate a monthly amount. If the respondent has not received money or goods from family or friends, we replace it with 0.

# B Appendix B - Supporting Figures and Tables

| Month 0 | Month 1 | Month 2 | Month 3 | Month 4 | Month 5 | Month 6 |
|---|---|---|---|---|---|---|
| - Mobilization<br>- Orientation (aspirations intervention) | - "Getting out of your comfort zone"<br>- Identifying business opportunities<br>- Finding capital and starting small | - Bookkeeping and record keeping<br>- Market research | - Business planning | - Growing your customer base<br>- Money management | - No modules (implement business plans) | - Graduation Ceremony |

*Note:* Each module is between 2–3 hours long and taught at a central training venue such as a school or church. Women participate in groups of 50–70.

Figure A1: Training Module Content




*Note:* Each line of the timeline refers to a study location. Letters denote in-person surveys. Light grey denotes SMS surveys. Dark grey shows the period of active treatment, or the period when the program took place.

Figure A2: Project Timeline

Table A1: Balance Table

| Variable | (1) Control N | (1) Control Mean/SD | (2) Treatment N | (2) Treatment Mean/SD | T-test P-value (1)-(2) |
|---|---|---|---|---|---|
| Age | 285 | 37.884 | 316 | 38.028 | 0.887 |
| | | (12.916) | | (11.863) | |
| Married | 285 | 0.618 | 316 | 0.677 | 0.126 |
| | | (0.487) | | (0.468) | |
| Divorced | 285 | 0.182 | 316 | 0.168 | 0.635 |
| | | (0.387) | | (0.374) | |
| Single | 285 | 0.084 | 316 | 0.070 | 0.503 |
| | | (0.278) | | (0.255) | |
| Widowed | 285 | 0.116 | 316 | 0.085 | 0.216 |
| | | (0.321) | | (0.280) | |
| Primary Ed. | 285 | 0.488 | 316 | 0.560 | 0.076 |
| | | (0.501) | | (0.497) | |
| Secondary Ed. | 285 | 0.902 | 316 | 0.930 | 0.205 |
| | | (0.298) | | (0.255) | |
| Father Primary Ed. | 285 | 0.765 | 316 | 0.810 | 0.176 |
| | | (0.425) | | (0.393) | |
| Father Secondary Ed. | 285 | 0.933 | 316 | 0.937 | 0.867 |
| | | (0.250) | | (0.244) | |
| Mother Primary Ed. | 285 | 0.828 | 316 | 0.864 | 0.224 |
| | | (0.378) | | (0.343) | |
| Mother Secondary Ed. | 285 | 0.954 | 316 | 0.975 | 0.177 |
| | | (0.209) | | (0.157) | |
| Employed | 285 | 0.537 | 316 | 0.541 | 0.916 |
| | | (0.500) | | (0.499) | |
| HH Size | 285 | 4.225 | 316 | 4.408 | 0.385 |
| | | (2.488) | | (2.675) | |
| Minors in HH | 285 | 2.926 | 316 | 3.174 | 0.153 |
| | | (2.101) | | (2.132) | |
| Daily Expenditure HH | 285 | 8177.598 | 315 | 9218.595 | 0.233 |
| | | (9453.527) | | (11672.218) | |
| Savings | 284 | 1.50e+05 | 312 | 1.40e+05 | 0.712 |
| | | (3.12e+05) | | (2.99e+05) | |
| Remittances | 277 | 37618.412 | 313 | 36006.390 | 0.910 |
| | | (1.60e+05) | | (1.82e+05) | |
| Own a Business | 285 | 0.551 | 316 | 0.522 | 0.482 |
| | | (0.498) | | (0.500) | |
| Sales - MB | 279 | 31299.642 | 314 | 28798.089 | 0.653 |
| | | (70258.956) | | (64997.485) | |
| Profits - MB | 280 | 57199.643 | 307 | 52258.958 | 0.603 |
| | | (1.17e+05) | | (1.13e+05) | |
| Profits - OB | 285 | 17781.053 | 316 | 17600.000 | 0.975 |
| | | (67995.450) | | (71818.526) | |
| Network Size | 285 | 4.544 | 316 | 4.905 | 0.217 |
| | | (3.546) | | (3.612) | |
| Locus - Internal | 284 | 16.127 | 314 | 16.010 | 0.540 |
| | | (2.388) | | (2.281) | |
| Locus - PO | 284 | -13.528 | 314 | -14.146 | 0.118 |
| | | (4.673) | | (4.961) | |
| Locus - Chance | 285 | -15.123 | 314 | -15.261 | 0.649 |
| | | (3.814) | | (3.629) | |
| F-test of joint significance (F-stat) | | | | | 0.822 |

*Notes*: Mean baseline covariates by treatment group. Standard deviations are in parentheses. The last column reports p-values associated with T-tests of joint equality between the groups.

Table A2: Attrition

| | (1) At Exit | (2) At Endline |
|---|---|---|
| Treat | -0.011 | 0.011 |
| | (0.025) | (0.023) |
| Age | -0.002 | -0.003 |
| | (0.001) | (0.001) |
| Married | -0.001 | -0.018 |
| | (0.040) | (0.040) |
| Divorced | -0.026 | -0.002 |
| | (0.040) | (0.042) |
| Single | 0.002 | 0.049 |
| | (0.067) | (0.074) |
| Primary Ed. | 0.016 | 0.021 |
| | (0.025) | (0.024) |
| Secondary Ed. | -0.060 | -0.091 |
| | (0.058) | (0.059) |
| Father Primary Ed. | 0.009 | -0.010 |
| | (0.037) | (0.039) |
| Father Secondary Ed. | 0.003 | 0.032 |
| | (0.061) | (0.060) |
| Mother Primary Ed. | -0.001 | -0.019 |
| | (0.039) | (0.044) |
| Mother Secondary Ed. | -0.015 | -0.019 |
| | (0.089) | (0.091) |
| Employed | 0.023 | 0.023 |
| | (0.033) | (0.030) |
| HH Size | 0.028 | 0.019 |
| | (0.013) | (0.011) |
| Minors in HH | -0.037 | -0.024 |
| | (0.015) | (0.012) |
| Daily Expenditure HH | -0.000 | -0.000 |
| | (0.000) | (0.000) |
| Savings | -0.000 | -0.000 |
| | (0.000) | (0.000) |
| Remittances | -0.000 | -0.000 |
| | (0.000) | (0.000) |
| Own a Business | -0.002 | -0.005 |
| | (0.034) | (0.031) |
| Sales - MB | -0.000 | -0.000 |
| | (0.000) | (0.000) |
| Profits - MB | 0.000 | 0.000 |
| | (0.000) | (0.000) |
| Profits - OB | 0.000 | 0.000 |
| | (0.000) | (0.000) |
| Network Size | -0.002 | -0.001 |
| | (0.003) | (0.003) |
| Locus - Internal | 0.006 | -0.000 |
| | (0.005) | (0.005) |
| Locus - PO | 0.004 | 0.004 |
| | (0.003) | (0.003) |
| Locus - Chance | -0.002 | -0.005 |
| | (0.004) | (0.004) |
| Observations | 568 | 568 |

*Note:* For the marital status, the omitted dummy is the Widowed status.

Table A3: Balance Table - SMS

| Variable | (1) Treatment N | (1) Treatment Mean/SD | (2) Control N | (2) Control Mean/SD | T-test P-value (1)-(2) |
|---|---|---|---|---|---|
| No. of Responses | 143 | 18.259 | 141 | 17.809 | 0.560 |
| | | (5.840) | | (7.095) | |
| Age | 143 | 37.993 | 141 | 37.319 | 0.638 |
| | | (11.973) | | (12.106) | |
| Married | 143 | 0.671 | 141 | 0.624 | 0.407 |
| | | (0.471) | | (0.486) | |
| Divorced | 143 | 0.175 | 141 | 0.177 | 0.956 |
| | | (0.381) | | (0.383) | |
| Single | 143 | 0.091 | 141 | 0.092 | 0.970 |
| | | (0.288) | | (0.290) | |
| Widowed | 143 | 0.063 | 141 | 0.106 | 0.189 |
| | | (0.244) | | (0.309) | |
| Primary Ed. | 143 | 0.503 | 141 | 0.397 | 0.072 |
| | | (0.502) | | (0.491) | |
| Secondary Ed. | 143 | 0.902 | 141 | 0.858 | 0.256 |
| | | (0.298) | | (0.350) | |
| Father Primary Ed. | 143 | 0.797 | 141 | 0.766 | 0.526 |
| | | (0.403) | | (0.425) | |
| Father Secondary Ed. | 143 | 0.930 | 141 | 0.908 | 0.493 |
| | | (0.256) | | (0.290) | |
| Mother Primary Ed. | 143 | 0.895 | 141 | 0.809 | 0.040 |
| | | (0.307) | | (0.395) | |
| Mother Secondary Ed. | 143 | 0.972 | 141 | 0.943 | 0.230 |
| | | (0.165) | | (0.232) | |
| Employed | 143 | 0.580 | 141 | 0.546 | 0.561 |
| | | (0.495) | | (0.500) | |
| HH Size | 143 | 4.517 | 141 | 4.262 | 0.427 |
| | | (2.742) | | (2.664) | |
| Minors in HH | 143 | 3.350 | 141 | 2.957 | 0.141 |
| | | (2.287) | | (2.184) | |
| Daily Expenditure HH | 142 | 9766.012 | 141 | 8735.542 | 0.448 |
| | | (11889.677) | | (10898.364) | |
| Savings | 142 | 1.57e+05 | 140 | 1.64e+05 | 0.866 |
| | | (3.32e+05) | | (3.58e+05) | |
| Remittances | 141 | 29290.780 | 137 | 39301.460 | 0.602 |
| | | (1.41e+05) | | (1.77e+05) | |
| Own a Business | 143 | 0.483 | 141 | 0.567 | 0.153 |
| | | (0.501) | | (0.497) | |
| Sales - MB | 142 | 26366.901 | 138 | 35753.623 | 0.258 |
| | | (65591.680) | | (72915.892) | |
| Profits - MB | 140 | 38627.143 | 139 | 59651.799 | 0.088 |
| | | (87381.549) | | (1.16e+05) | |
| Profits - OB | 143 | 15962.937 | 141 | 17812.766 | 0.822 |
| | | (68880.303) | | (69831.447) | |
| Network Size | 143 | 5.294 | 141 | 4.851 | 0.338 |
| | | (4.136) | | (3.615) | |
| Locus - Internal | 142 | 16.352 | 140 | 16.471 | 0.654 |
| | | (2.167) | | (2.296) | |
| Locus - PO | 142 | -14.331 | 140 | -13.057 | 0.027 |
| | | (4.851) | | (4.774) | |
| Locus - Chance | 142 | -15.303 | 141 | -14.766 | 0.211 |
| | | (3.491) | | (3.710) | |
| F-test of joint significance (F-stat) | | | | | 1.014 |

*Notes*: Mean baseline covariates by treatment group. Standard deviations are in parentheses. The last column reports p-values associated with T-tests of joint equality between the groups.

Table A4: SMS Attrition

| | (1) | (2) |
|---|---|---|
| Treat | -0.080 | 0.172 |
| | (0.716) | (0.207) |
| Age | 0.105 | 0.031 |
| | (0.034) | (0.010) |
| Married | 0.383 | 0.231 |
| | (1.407) | (0.404) |
| Divorced | 1.028 | 0.820 |
| | (1.420) | (0.411) |
| Single | 2.680 | 0.819 |
| | (1.926) | (0.537) |
| Primary Ed. | -1.821 | -0.198 |
| | (0.772) | (0.224) |
| Secondary Ed. | -0.432 | -0.069 |
| | (1.451) | (0.425) |
| Father Primary Ed. | 0.400 | -0.108 |
| | (1.078) | (0.316) |
| Father Secondary Ed. | -3.080 | -0.316 |
| | (1.615) | (0.463) |
| Mother Primary Ed. | 0.857 | 0.102 |
| | (1.184) | (0.367) |
| Mother Secondary Ed. | -0.105 | -0.245 |
| | (2.393) | (0.698) |
| Employed | 0.209 | -0.129 |
| | (0.919) | (0.266) |
| HH Size | -0.496 | -0.183 |
| | (0.318) | (0.093) |
| Minors in HH | 1.054 | 0.334 |
| | (0.377) | (0.111) |
| Daily Expenditure HH | 0.000 | 0.000 |
| | (0.000) | (0.000) |
| Savings | 0.000 | 0.000 |
| | (0.000) | (0.000) |
| Remittances | -0.000 | -0.000 |
| | (0.000) | (0.000) |
| Own a Business | -0.204 | 0.233 |
| | (0.960) | (0.282) |
| Sales - MB | 0.000 | -0.000 |
| | (0.000) | (0.000) |
| Profits - MB | -0.000 | -0.000 |
| | (0.000) | (0.000) |
| Profits - OB | -0.000 | -0.000 |
| | (0.000) | (0.000) |
| Network Size | 0.110 | 0.038 |
| | (0.099) | (0.028) |
| Locus - Internal | 0.507 | 0.124 |
| | (0.162) | (0.047) |
| Locus - PO | -0.080 | -0.003 |
| | (0.096) | (0.028) |
| Locus - Chance | 0.118 | 0.037 |
| | (0.122) | (0.036) |
| Observations | 568 | 568 |

*Notes*: Associations between the number of SMS surveys responded to and baseline covariates. Column (2) shows associations only during periods of COVID-19 lockdowns

Table A5: Adjusted Treatment Effects on Business Outcomes Extensive Margin

| | Business Creation | | Main Business | | Other Businesses | All Businesses | |
|---|---|---|---|---|---|---|---|
| | (1) Own a Business | (2) No. Businesses | (3) $>0$ Sales | (4) $>0$ Profits | (5) $>0$ Profits | (6) $>0$ Profits | (7) Index |
| *Panel A: Midline (6 months)* | | | | | | | |
| Adj. ITT | 0.162*** | 0.253*** | 0.151*** | 0.186*** | 0.059* | 0.167*** | 0.317*** |
| | (0.039) | (0.064) | (0.041) | (0.040) | (0.033) | (0.040) | (0.081) |
| | [0.001] | [0.001] | [0.001] | [0.001] | [0.011] | [0.001] | [0.001] |
| Observations | 546 | 546 | 547 | 547 | 547 | 547 | 547 |
| Control Mean | 0.566 | 0.833 | 0.420 | 0.486 | 0.179 | 0.521 | -0.051 |
| Adj. $R^2$ | 0.206 | 0.293 | 0.177 | 0.190 | 0.139 | 0.186 | 0.183 |
| *Panel B: Endline (18–24 months)* | | | | | | | |
| Adj. ITT | 0.096** | 0.253*** | 0.150*** | 0.111*** | 0.090*** | 0.111*** | 0.285*** |
| | (0.039) | (0.067) | (0.043) | (0.041) | (0.034) | (0.041) | (0.087) |
| | [0.009] | [0.002] | [0.002] | [0.006] | [0.006] | [0.006] | [0.002] |
| Observations | 545 | 544 | 545 | 545 | 545 | 545 | 545 |
| Control Mean | 0.667 | 0.903 | 0.457 | 0.589 | 0.155 | 0.593 | 0.022 |
| Adj. $R^2$ | 0.093 | 0.163 | 0.097 | 0.066 | 0.041 | 0.079 | 0.063 |

*Note:* We winsorize all sales and profit measures at the 99th percentile. Coefficients are ANCOVA estimates that control for the outcome at baseline, the respondent's location, marital status, household size, number of children, age, network size and level of education. Adjusted ITT shows effects adjusted for positive spillovers to the control group. We record revenues and profits for women without a business as zero to preserve the balance from randomization. Column (3) presents a dummy variable equal to 1 if the sales for the 3 days prior the survey are greater than 0, columns (4)–(6) present a dummy variable equal to 1 if the self-reported profits for the last month are greater than 0, for either the main business, other businesses or all businesses. We report White robust standard errors in parentheses. Index is an Anderson index of all outcomes using the swindex command in Stata. Multiple hypothesis test q-values (computed following Anderson (2008)) are presented in brackets. Q-values include the same controls as above and are computed within pre-registered families of outcomes.

Table A6: Adjusted Treatment Effects on Business Outcomes: Intensive Margin

| | Main Business Sales | Main Business Profits | Other Businesses Profits | All Businesses Profits | |
|---|---|---|---|---|---|
| | (1) Levels | (2) Levels | (3) Levels | (4) Levels | (5) Index |
| | | | *Panel A: Midline (6 months)* | | |
| Adj. ITT | 5155.40 | 16067.61 | 6315.06 | 24958.95* | 0.24*** |
| | (6727.24) | (11432.22) | (5524.28) | (14211.93) | (0.09) |
| | [0.363] | [0.247] | [0.247] | [0.188] | [0.031] |
| Observations | 532 | 525 | 547 | 547 | 547 |
| Control Mean | 37714.29 | 69693.20 | 18589.11 | 88718.68 | -0.05 |
| Adj. $R^2$ | 0.153 | 0.186 | 0.080 | 0.214 | 0.037 |
| | | | *Panel B: Endline (18–24 Months)* | | |
| Adj. ITT | 8463.50 | 11301.73 | 10938.79** | 24456.09* | 0.17*** |
| | (7484.88) | (10439.54) | (5021.51) | (12518.50) | (0.06) |
| | [0.126] | [0.126] | [0.063] | [0.073] | [0.025] |
| Observations | 538 | 530 | 545 | 545 | 545 |
| Control Mean | 45476.26 | 76934.12 | 17251.55 | 94589.53 | 0.06 |
| Adj. $R^2$ | 0.146 | 0.113 | 0.058 | 0.126 | 0.058 |

*Note:* We winsorize all sales and profit measures at the 99th percentile. Coefficients are ANCOVA estimates that control for the outcome at baseline, the respondent's location, marital status, household size, number of children, age, network size and level of education. Adjusted ITT shows effects adjusted for positive spillovers to the control group. We report White robust standard errors in parentheses. Column (1) presents the reported sales for the 3 days prior the survey, columns (2) to (4) present the self-reported profits for the last month for either the main business, other businesses, or all businesses. Values in columns marked with heading *Levels* are values in UGX. Index is an Anderson index of all outcomes using the swindex command in Stata. Multiple hypothesis test q-values (computed following Anderson (2008)) are presented in brackets. Q-values include the same controls as above and are computed within pre-registered families of outcomes.

Table A7: Adjusted Treatment Effects on Re-Investments Outcomes

| | Savings | Business Assests | Investments in Other Businesses | |
|---|---|---|---|---|
| | (1) Levels | (2) Levels | (3) Levels | (4) Index |
| | *Panel A: Midline (6 months)* | | | |
| Adj. ITT | 4603.86 | 7866.86 | 3176.11 | 0.05 |
| | (22079.99) | ( 21319.16) | (16018.14) | (0.08) |
| | [1.000] | [1.000] | [1.000] | [1.000] |
| Observations | 529 | 547 | 547 | 547 |
| Control Mean | 166976.10 | 119889.56 | 47501.17 | 0.06 |
| Adj. $R^2$ | 0.290 | 0.325 | 0.087 | 0.351 |
| | *Panel B: Endline (18–24 Months)* | | | |
| Adj. ITT | 31812.95 | 22286.89 | 28878.05** | 0.21** |
| | (25438.22) | ( 25338.68) | (12228.25) | (0.08) |
| | [0.164] | [0.234] | [0.038] | [0.038] |
| Observations | 532 | 545 | 545 | 545 |
| Control Mean | 162703.16 | 125942.20 | 29836.05 | 0.00 |
| Adj. $R^2$ | 0.194 | 0.188 | 0.029 | 0.212 |

*Note:* We winsorize all savings and investment measures at the 99th percentile. Coefficients are ANCOVA estimates that control for the outcome at baseline, the respondent's location, marital status, household size, number of children, age, network size and level of education. Adjusted ITT shows effects adjusted for positive spillovers to the control group. Savings is the total amount held in all financial savings instruments. Business assets is the estimated monetary value of all assets held in the main business. Investments in other businesses is the total estimated monetary value of all investments in businesses other than the main business in the last 6 months. Values in columns marked with heading *Levels* are values in UGX. We report White robust standard errors in parentheses. Index is an Anderson index of all outcomes using the swindex command in Stata. Multiple hypothesis test q-values (computed following Anderson (2008)) are presented in brackets. Q-values include the same controls as above and are computed within pre-registered families of outcomes.

Table A8: Adjusted Treatment Effects: Skills

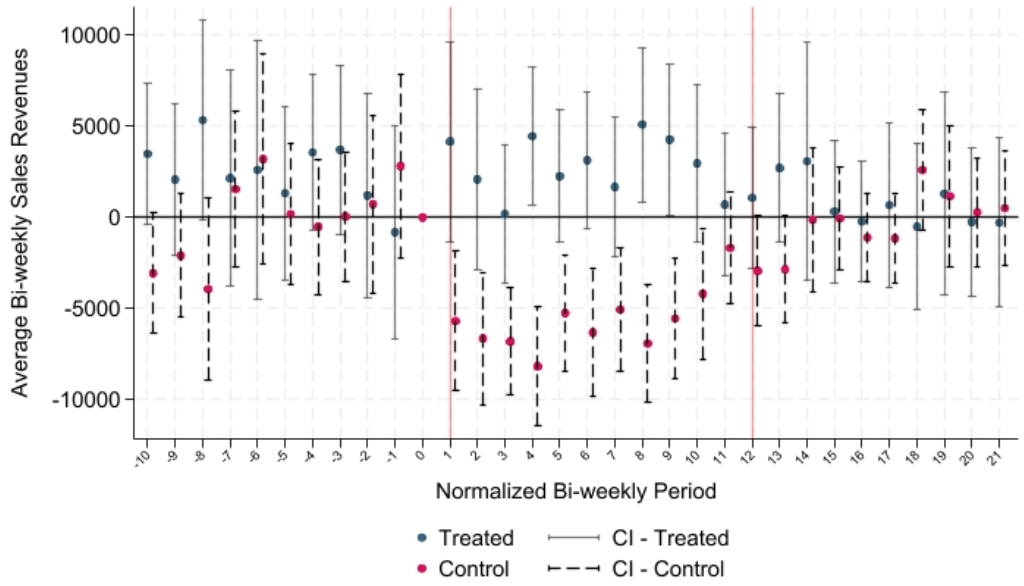
| | | | | | | Locus of Control | | | Income Aspirations | |
|---|---|---|---|---|---|---|---|---|---|---|
| | (1) Tracking | (2) Price Mgmt. | (3) Work Hours | (4) Goal Setting | (5) Grit | (6) Internal | (7) PO | (8) Chance | (9) Levels | (10) Index |
| *Panel A: Midline (6 months)* | | | | | | | | | | |
| Adj. ITT | 0.283** | 0.489*** | 13.410*** | 0.272** | 1.340*** | -0.027 | 0.209 | 0.546 | 96189.10 | 0.22*** |
| | (0.139) | (0.154) | (4.443) | (0.137) | (0.490) | (0.211) | (0.399) | (0.342) | (236621.01) | (0.08) |
| | [0.050] | [0.013] | [0.013] | [0.050] | [0.013] | [0.370] | [0.292] | [0.085] | [0.296] | [0.013] |
| Observations | 290 | 281 | 228 | 243 | 544 | 543 | 543 | 544 | 430 | 546 |
| Control Mean | 0.957 | 1.012 | 27.909 | 0.643 | 29.488 | 15.836 | -12.914 | -14.645 | 1504624.45 | -0.10 |
| Adj. $R^2$ | 0.167 | 0.114 | 0.265 | 0.145 | 0.108 | 0.029 | 0.146 | 0.094 | 0.225 | 0.197 |
| *Panel B: Endline (18–24 Months)* | | | | | | | | | | |
| Adj. ITT | 0.235* | 0.361** | 8.150* | 0.296** | 1.161** | 0.187 | 0.603 | 0.064 | -346290.49* | 0.15* |
| | (0.138) | (0.165) | (4.222) | (0.138) | (0.477) | (0.241) | (0.430) | (0.350) | (198272.93) | (0.08) |
| | [0.121] | [0.121] | [0.121] | [0.121] | [0.121] | [0.171] | [0.144] | [0.251] | [0.121] | [0.121] |
| Observations | 286 | 274 | 229 | 231 | 541 | 540 | 540 | 541 | 441 | 544 |
| Control Mean | 1.133 | 1.239 | 34.850 | 0.688 | 30.094 | 15.801 | -12.121 | -14.191 | 1577941.91 | 0.01 |
| Adj. $R^2$ | 0.060 | -0.011 | 0.145 | 0.047 | 0.110 | 0.043 | 0.037 | 0.046 | 0.062 | 0.116 |

*Note:* Coefficients are ANCOVA estimates that control for the outcome at baseline, the respondent's location, marital status, household size, number of children, age, network size and level of education. Adjusted ITT shows effects adjusted for positive spillovers to the control group. Tracking combines multiple questions about record keeping for the business with a maximum value of 3. Price management combines multiple questions about setting prices, running promotions, comparing prices with competitors, and negotiating for better prices with suppliers with a maximum value of 4. Goal setting combines multiple questions about setting goals for the business over various time horizons with a maximum value of 3. Work hours is the number of hours the respondent typically works in her main business. Our measures of grit follow Duckworth, Peterson, et al. (2007) and Duckworth and Quinn (2009). We draw our locus of control measures from Levenson (1973). Internal, PO, and Chance is the dimension of the locus of control score. Positive values for Internal, PO and Chance provide evidence of improvements in locus of control measures independent of type. We winsorize the income aspirations variable at the 99th percentile. Values in columns marked with heading *Levels* are values in UGX. We report White robust standard errors in parentheses. Index is an Anderson index of all outcomes using the swindex command in Stata. Multiple hypothesis test q-values (computed following Anderson (2008)) are presented in brackets. Q-values include the same controls as above and are computed within pre-registered families of outcomes.

Table A9: Adjusted Treatment Effects on Household Outcomes

| | Daily Expenditure | | | | Remittances | |
|---|---|---|---|---|---|---|
| | (1) HH Levels | (2) Participant Levels | (3) MUE | (4) Food Insecurity | (5) Levels | (6) Index |
| *Panel A: Midline (6 months)* | | | | | | |
| Adj. ITT | -153.14 | -490.28 | 0.040 | 0.121*** | -5065.95 | 0.09 |
| | (449.79) | (311.95) | (0.086) | (0.039) | (7563.54) | (0.07) |
| | [0.819] | [0.409] | [0.819] | [0.012] | [0.819] | [0.534] |
| Observations | 544 | 541 | 481 | 543 | 528 | 546 |
| Control Mean | 7205.16 | 3565.36 | -0.026 | 0.270 | 34309.80 | -0.12 |
| Adj. $R^2$ | 0.198 | 0.041 | 0.074 | 0.124 | 0.169 | 0.041 |
| *Panel B: Endline (18–24 Months)* | | | | | | |
| Adj. ITT | -278.88 | -44.93 | 0.142 | 0.006 | -497.52 | 0.09 |
| | (441.47) | (289.34) | (0.096) | (0.040) | (6399.34) | (0.07) |
| | [1.000] | [1.000] | [1.000] | [1.000] | [1.000] | [1.000] |
| Observations | 544 | 540 | 476 | 543 | 526 | 544 |
| Control Mean | 6929.52 | 3443.70 | -0.076 | 0.310 | 25547.62 | -0.17 |
| Adj. $R^2$ | 0.183 | 0.013 | 0.047 | 0.091 | 0.072 | 0.093 |

*Note:* We winsorize daily expenditures, MUE, and remittances at the 99th percentile. Coefficients are ANCOVA estimates that control for the outcome at baseline, the respondent's location, marital status, household size, number of children, age, network size and level of education. Adjusted ITT shows effects adjusted for positive spillovers to the control group. We calculate the MUE using consumption expenditures over the past week on seventeen food items, following the methods outlined in Ligon (2020). Higher values of the MUE indicate higher marginal utilities of expenditure, indicating that households are worse off. Food insecurity is a binary variable equal to one if the woman reports not having enough food more than once over the six months before the survey. Column (2) is included in column (1), however, we wanted to show the effect on the participating women expenditures as the information on the household corresponds to estimates the respondent thinks other members of the household have. Values in columns marked with heading *Levels* are values in UGX. We report White robust standard errors in parentheses. Index is an Anderson index of all outcomes using the swindex command in Stata. Multiple hypothesis test q-values (computed following Anderson (2008)) are presented in brackets. Q-values include the same controls as above and are computed within pre-registered families of outcomes.




*Note:* Each point represents a coefficient from a treatment group-specific event study where the reference period is the two weeks prior to the first COVID-19 lockdown in Uganda, using data from weekly SMS surveys. Therefore, estimates for each treatment group are normalized by the reference period mean for the group and cannot be directly interpreted as treatment effects. The SMS survey elicits daily revenues for the day preceding the survey. Each woman receives the survey on a randomly assigned day of the week. We winsorize at the 99th percentiles, weight all responses by the inverse probability of a woman responding as predicted by baseline covariates selected using LASSO, then take means at the bi-weekly level. The red lines indicate the beginning and end of the first nationwide Ugandan COVID-19 lockdown.

Figure A3: Reactions to COVID-19 Lockdown by Treatment Group (Weighted Effects)

# C Appendix C - SMS LASSO and Photobook Weights

## C.1 SMS Lasso

To generate weights to account for non-responses in the SMS data, we perform a LASSO regression to select the best predictors of baseline SMS responses among participants.[28] After running a LASSO regression analysis on multiple baseline covariates such as the treatment status, demographic characteristics, and all the outcomes presented in the paper, and a set of imputed dummies to flag missing responses, we take the LASSO-selected variables and run a simple OLS regression on responses to the SMS survey in the first month of the study (see Table A10). We use the estimated coefficients from that regression to predict each participant's response probability. After calculating the response probability and given the use of a linear probability model, we then ensure that all estimated probabilities lie between 0 and 1. If a probability was negative, we replaced it with the smallest non-zero value in the distribution, and if it was greater than 1, we replaced it with 1. After this, we calculate the weights as follows:

$$\text{SMS Weight}_i = \frac{1}{P\{\text{Response at Baseline}\}_i}$$

Finally, we winsorize the weights at the 95th percentile.

## C.2 Photo book Weights

Although photo books are meant to be less cognitively taxing than other methods for collecting network data, women located on later pages or further down on a page are less likely to be identified than those appearing on earlier pages, or higher on a page. Randomly assigning women's position within each photobook ensures respondent fatigue does not bias our results, but to use our estimates to adjust our estimated treatment effects we need to be able to interpret the magnitude of our estimated spillover effects. To calculate the weights to correct for the probability of being identified if a woman is located on earlier pages in the photo book, we regress the number of times each woman was identified in a certain photo book in each survey round against a set of dummies that identify the page and page position her photo is in said photo book and control for the survey round. Afterward, we use the estimated coefficients shown in Table A11 to produce a set of weights.

Let $w_p$ be -1 multiplied by the coefficient in Table A11 Column (1) that corresponds to the page on which an identified woman appears. Let $w_s$ be -1 multiplied by the coefficient in Table A11 Column (2) that corresponds to the position on the page where an identified woman appears. We compute the woman's weight as $w = 1 + w_p + w_s$ such that women appearing on the first page in the upper left corner have a weight of 1 and women appearing elsewhere in the photo book get up-weighted to adjust for respondent fatigue.

[28] Baseline for the SMS data is considered the first month of SMS surveys for each location

Table A10: Lasso Selected Variables - OLS

| | (1)<br>Baseline Response |
|---|---|
| Age | -0.005 |
| | (0.000) |
| Widow | -0.117 |
| | (0.019) |
| Primary Ed. | -0.057 |
| | (0.011) |
| Secondary Ed. | -0.098 |
| | (0.020) |
| HH Size=4 | 0.110 |
| | (0.014) |
| HH Size=5 | 0.222 |
| | (0.017) |
| HH Size=9 | -0.002 |
| | (0.032) |
| HH Size=15 | -0.224 |
| | (0.094) |
| HH Size=17 | -0.260 |
| | (0.167) |
| Minors in HH=5 | -0.190 |
| | (0.017) |
| Minors in HH=10 | 0.470 |
| | (0.081) |
| Parents had a Business | 0.051 |
| | (0.011) |
| Father Secondary Edu. | -0.143 |
| | (0.020) |
| Employed | 0.020 |
| | (0.012) |
| Location=3 | 0.171 |
| | (0.014) |
| Location=5 | 0.133 |
| | (0.014) |
| Pay Rent | 0.093 |
| | (0.012) |
| School Insecurity | 0.069 |
| | (0.011) |
| Got business money stolen | -0.005 |
| | (0.030) |
| Got personal money stolen | -0.406 |
| | (0.041) |
| Robbed | 0.177 |
| | (0.030) |
| Any Link to Treated | -0.009 |
| | (0.002) |
| Family Link to Treated | -0.062 |
| | (0.010) |
| Friend Link to Treated | -0.046 |
| | (0.005) |
| Business Link to Treated | -0.042 |
| | (0.006) |
| > 0 Sales | -0.089 |
| | (0.015) |
| Work Hours | -0.001 |
| | (0.000) |
| Grit | 0.011 |
| | (0.001) |
| Locus of Control - Internal | 0.014 |
| | (0.002) |
| Locus of Control - PO | 0.008 |
| | (0.001) |
| Social Status Aspirations | -0.062 |
| | (0.005) |
| Constant | 0.735 |
| | (0.060) |
| Observations | 7642 |
| Adj. $R^2$ | 0.226 |

Table A11: Estimated Coefficients for Photo books Weights

| | (1) Page | (2) Page Position |
|---|---|---|
| 1 | 0.000 | 0.000 |
| | (.) | (.) |
| 2 | -0.514 | -0.370 |
| | (0.048) | (0.062) |
| 3 | -0.535 | -0.269 |
| | (0.051) | (0.066) |
| 4 | -0.724 | -0.171 |
| | (0.051) | (0.066) |
| 5 | -0.848 | -0.173 |
| | (0.050) | (0.065) |
| 6 | -0.832 | -0.454 |
| | (0.052) | (0.065) |
| 7 | -0.958 | -0.269 |
| | (0.049) | (0.068) |
| 8 | -0.865 | -0.259 |
| | (0.054) | (0.067) |
| 9 | -1.028 | -0.293 |
| | (0.051) | (0.064) |
| 10 | -1.148 | -0.334 |
| | (0.053) | (0.065) |
| 11 | -1.009 | -0.245 |
| | (0.052) | (0.070) |
| 12 | -1.069 | -0.443 |
| | (0.062) | (0.067) |
| 13 | -0.954 | -0.330 |
| | (0.064) | (0.061) |
| 14 | -0.887 | -0.301 |
| | (0.065) | (0.067) |
| 15 | -1.300 | -0.413 |
| | (0.076) | (0.067) |
| 16 | -1.084 | -0.363 |
| | (0.144) | (0.067) |

# D Appendix D - Robustness

Table A12: Treatment Effects on Business Outcomes Extensive Margin - Other Specifications

| | Midline (6 months) | | | | Endline (18–24 Months) | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| **Own a Business** | | | | | | | | |
| Treat | 0.165 | 0.153 | 0.196 | 0.173 | 0.062 | 0.076 | 0.075 | 0.088 |
| | (0.041) | (0.042) | (0.046) | (0.044) | (0.039) | (0.041) | (0.048) | (0.044) |
| Observations | 546 | 546 | 1090 | 539 | 545 | 545 | 1088 | 536 |
| **No. Businesses** | | | | | | | | |
| Treat | 0.247 | 0.230 | 0.287 | 0.247 | 0.194 | 0.215 | 0.187 | 0.234 |
| | (0.072) | (0.074) | (0.069) | (0.075) | (0.070) | (0.070) | (0.077) | (0.074) |
| Observations | 547 | 547 | 1092 | 539 | 545 | 545 | 1088 | 535 |
| $> 0$ **Sales** | | | | | | | | |
| Treat | 0.152 | 0.140 | 0.154 | 0.142 | 0.128 | 0.143 | 0.118 | 0.145 |
| | (0.042) | (0.045) | (0.047) | (0.046) | (0.043) | (0.044) | (0.050) | (0.048) |
| Observations | 547 | 547 | 1094 | 540 | 545 | 545 | 1090 | 536 |
| $> 0$ **Profits - MB** | | | | | | | | |
| Treat | 0.183 | 0.167 | 0.228 | 0.194 | 0.076 | 0.090 | 0.100 | 0.102 |
| | (0.042) | (0.044) | (0.046) | (0.046) | (0.041) | (0.043) | (0.051) | (0.047) |
| Observations | 547 | 547 | 1094 | 540 | 545 | 545 | 1090 | 536 |
| $> 0$ **Profits - OB** | | | | | | | | |
| Treat | 0.024 | 0.033 | 0.078 | 0.035 | 0.068 | 0.075 | 0.094 | 0.085 |
| | (0.034) | (0.035) | (0.037) | (0.040) | (0.033) | (0.034) | (0.041) | (0.038) |
| Observations | 547 | 547 | 1094 | 540 | 545 | 545 | 1090 | 536 |
| $> 0$ **Profits - All Businesses** | | | | | | | | |
| Treat | 0.168 | 0.160 | 0.207 | 0.187 | 0.086 | 0.102 | 0.092 | 0.113 |
| | (0.041) | (0.043) | (0.047) | (0.045) | (0.041) | (0.042) | (0.051) | (0.047) |
| Observations | 547 | 547 | 1094 | 540 | 545 | 545 | 1090 | 536 |
| Controls | | ✓ | | ✓ | | ✓ | | ✓ |
| FE | | | ✓ | | | | ✓ | |

*Note:* Columns (1), (2), (5) and (6) present the results of the OLS specification, columns (3) and (7) present the results of the two-way fixed effects specification, and columns (4) and (8) present results from the ANCOVA IV specification where we instrument for actual treatment with assigned treatment to account for imperfect compliance. Coefficients in columns (2), (4), (6), and (8) control for the respondent's location, marital status, household size, number of children, age, network size and level of education. We report standard errors clustered at the individual level in parentheses in columns (3) and (7) and White robust standard errors in all other columns.

Table A13: Treatment Effects on Business Outcomes Intensive Margin - Other Specifications

| | Midline (6 months) | | | | Endline (18–24 Months) | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| **Sales (Levels)** | | | | | | | | |
| Treat | 3535.540 | 4331.985 | 6790.256 | 3984.237 | 5926.893 | 8153.968 | 10470.980 | 6955.170 |
| | (6828.464) | (6977.811) | (7015.283) | (7299.266) | (8080.487) | (8272.707) | (7940.279) | (8940.185) |
| Observations | 539 | 539 | 1064 | 526 | 542 | 542 | 1076 | 529 |
| **Profits (Levels) - MB** | | | | | | | | |
| Treat | 10190.337 | 15166.311 | 16635.381 | 17199.031 | 3353.953 | 7844.023 | 6805.508 | 3880.827 |
| | (11856.613) | (12019.553) | (11827.408) | (13082.122) | (10929.415) | (10860.622) | (12138.506) | (12970.861) |
| Observations | 535 | 535 | 1050 | 518 | 540 | 540 | 1060 | 521 |
| **Profits (Levels) - OB** | | | | | | | | |
| Treat | 1359.171 | 5276.096 | 8490.113 | 2112.334 | 4220.923 | 8341.647 | 5584.500 | 7994.837 |
| | (5336.540) | (5765.658) | (6672.752) | (6387.000) | (5200.632) | (5287.438) | (7117.110) | (5921.579) |
| Observations | 547 | 547 | 1094 | 540 | 545 | 545 | 1090 | 536 |
| **Profits (Levels) - All Businesses** | | | | | | | | |
| Treat | 10425.488 | 20554.422 | 26666.648 | 20208.742 | 7482.591 | 16653.734 | 14291.850 | 13869.706 |
| | (14591.215) | (15244.561) | (14428.720) | (15210.981) | (13330.422) | (13291.107) | (14980.294) | (15360.610) |
| Observations | 547 | 547 | 1094 | 540 | 545 | 545 | 1090 | 536 |
| Controls | | ✓ | | ✓ | | ✓ | | ✓ |
| FE | | | ✓ | | | | ✓ | |

*Note:* Columns (1), (2), (5) and (6) present the results of the OLS specification, columns (3) and (7) present the results of the two-way fixed effects specification, and columns (4) and (8) present results from the ANCOVA IV specification where we instrument for actual treatment with assigned treatment to account for imperfect compliance. We winsorize all sales and profit measures at the 99th percentile. Coefficients in columns (2), (4), (6), and (8) control for the respondent's location, marital status, household size, number of children, age, network size and level of education. Values in rows marked with heading *Levels* are values in UGX. We report standard errors clustered at the individual level in parentheses in columns (3) and (7) and White robust standard errors in all other columns.

Table A14: Treatment Effects on Possible Mechanisms I - Other Specifications

| | Midline (6 months) | | | | Endline (18–24 Months) | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| **Tracking** | | | | | | | | |
| Treat | 0.375 | 0.403 | 0.342 | 0.195 | 0.213 | 0.300 | 0.141 | 0.382 |
| | (0.106) | (0.109) | (0.110) | (0.179) | (0.106) | (0.110) | (0.124) | (0.186) |
| Observations | 544 | 544 | 1082 | 281 | 542 | 542 | 1080 | 279 |
| **Price Mgmt.** | | | | | | | | |
| Treat | 0.402 | 0.393 | 0.360 | 0.491 | 0.286 | 0.284 | 0.193 | 0.482 |
| | (0.116) | (0.120) | (0.124) | (0.207) | (0.120) | (0.128) | (0.139) | (0.215) |
| Observations | 536 | 536 | 1056 | 273 | 535 | 535 | 1054 | 268 |
| **Work Hours** | | | | | | | | |
| Treat | 12.457 | 11.051 | 13.534 | 14.899 | 5.999 | 6.044 | 4.815 | 6.158 |
| | (3.185) | (3.348) | (3.419) | (6.332) | (3.213) | (3.343) | (3.916) | (5.899) |
| Observations | 514 | 514 | 942 | 218 | 519 | 519 | 952 | 220 |
| **Goal Setting** | | | | | | | | |
| Treat | 0.250 | 0.269 | 0.261 | 0.109 | 0.161 | 0.157 | 0.188 | 0.219 |
| | (0.089) | (0.091) | (0.101) | (0.175) | (0.094) | (0.098) | (0.120) | (0.201) |
| Observations | 517 | 517 | 974 | 234 | 496 | 496 | 934 | 224 |
| **Grit** | | | | | | | | |
| Treat | 0.877 | 0.988 | 0.829 | 1.289 | 0.809 | 0.907 | 0.517 | 0.958 |
| | (0.494) | (0.508) | (0.559) | (0.544) | (0.485) | (0.501) | (0.558) | (0.575) |
| Observations | 546 | 546 | 1088 | 537 | 543 | 543 | 1082 | 532 |
| **Self-Efficacy** | | | | | | | | |
| Treat | 0.663 | 0.635 | -0.087 | 0.617 | 0.551 | 0.600 | -0.293 | 0.528 |
| | (0.580) | (0.591) | (0.688) | (0.645) | (0.595) | (0.610) | (0.706) | (0.693) |
| Observations | 546 | 546 | 1086 | 536 | 543 | 543 | 1080 | 531 |
| Controls | | ✓ | | ✓ | | ✓ | | ✓ |
| FE | | | ✓ | | | | ✓ | |

*Note:* Columns (1), (2), (5) and (6) present the results of the OLS specification, columns (3) and (7) present the results of the two-way fixed effects specification, and columns (4) and (8) present results from the ANCOVA IV specification where we instrument for actual treatment with assigned treatment to account for imperfect compliance. Coefficients in columns (2), (4), (6), and (8) control for the respondent's location, marital status, household size, number of children, age, network size and level of education. We report standard errors clustered at the individual level in parentheses in columns (3) and (7) and White robust standard errors in all other columns.

Table A15: Treatment Effects on Possible Mechanisms II - Other Specifications

| | Midline (6 months) | | | | Endline (18–24 Months) | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| **Locus of Control - Internal** | | | | | | | | |
| Treat | 0.054 | 0.004 | 0.148 | 0.018 | 0.262 | 0.302 | 0.351 | 0.518 |
| | (0.211) | (0.214) | (0.283) | (0.245) | (0.239) | (0.248) | (0.283) | (0.278) |
| Observations | 546 | 546 | 1086 | 536 | 543 | 543 | 1080 | 531 |
| **Locus of Control - PO** | | | | | | | | |
| Treat | -0.282 | -0.109 | 0.432 | 0.018 | 0.184 | 0.389 | 0.721 | 0.378 |
| | (0.422) | (0.429) | (0.472) | (0.449) | (0.429) | (0.449) | (0.530) | (0.505) |
| Observations | 546 | 546 | 1086 | 536 | 543 | 543 | 1080 | 531 |
| **Locus of Control - Chance** | | | | | | | | |
| Treat | 0.258 | 0.251 | 0.475 | 0.245 | -0.209 | -0.239 | -0.234 | -0.456 |
| | (0.343) | (0.351) | (0.407) | (0.394) | (0.345) | (0.361) | (0.425) | (0.396) |
| Observations | 546 | 546 | 1088 | 537 | 543 | 543 | 1082 | 532 |
| **Aspirations - Income (Levels)** | | | | | | | | |
| Treat | 114208.879 | 15753.424 | 303292.458 | 79198.836 | -370986.853 | -336510.967 | -289112.165 | -536708.811 |
| | (247202.188) | (232981.234) | (593411.042) | (321978.368) | (179533.167) | (172739.638) | (561647.407) | (265120.173) |
| Observations | 490 | 490 | 860 | 424 | 508 | 508 | 882 | 435 |
| **Aspirations - Social Status** | | | | | | | | |
| Treat | 0.113 | 0.101 | 0.184 | 0.128 | 0.057 | 0.039 | 0.167 | 0.003 |
| | (0.085) | (0.089) | (0.113) | (0.101) | (0.088) | (0.090) | (0.119) | (0.103) |
| Observations | 540 | 540 | 1072 | 529 | 539 | 539 | 1070 | 526 |
| Controls | | ✓ | | ✓ | | ✓ | | ✓ |
| FE | | | ✓ | | | | ✓ | |

*Note:* Columns (1), (2), (5) and (6) present the results of the OLS specification, columns (3) and (7) present the results of the two-way fixed effects specification, and columns (4) and (8) present results from the ANCOVA IV specification where we instrument for actual treatment with assigned treatment to account for imperfect compliance. We winsorize all income aspirations measures at the 99th percentile. Coefficients in columns (2), (4), (6), and (8) control for the respondent's location, marital status, household size, number of children, age, network size and level of education. Values in rows marked with heading *Levels* are values in UGX. We report standard errors clustered at the individual level in parentheses in columns (3) and (7) and White robust standard errors in all other columns.

Table A16: Treatment Effects on Re-Investments - Other Specifications

| | Midline (6 months) | | | | Endline (18–24 Months) | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| **Savings (Levels)** | | | | | | | | |
| Treat | -3694.117 | 6213.178 | 1534.547 | -5511.010 | 40917.894 | 45814.849 | 27268.776 | 33344.390 |
| | (27149.801) | (26235.749) | (25674.769) | (24976.074) | (28231.039) | (30401.398) | (28638.152) | (28901.309) |
| Observations | 534 | 534 | 1058 | 522 | 537 | 537 | 1064 | 523 |
| **Business Assets (Levels)** | | | | | | | | |
| Treat | -26727.422 | -14731.468 | 14562.657 | -9100.691 | -12845.686 | 584.854 | 30508.026 | 9999.528 |
| | (24613.631) | (24494.953) | (22332.233) | (25065.008) | (26759.950) | (26389.503) | (26756.761) | (29130.440) |
| Observations | 547 | 547 | 1094 | 540 | 545 | 545 | 1090 | 536 |
| **Investments in Other Businesses (Levels)** | | | | | | | | |
| Treat | -320.133 | 4212.485 | 10451.303 | 7493.207 | 23556.985 | 25433.318 | 28056.284 | 31545.027 |
| | (14560.390) | (16142.455) | (14115.811) | (17632.514) | (12247.027) | (11785.101) | (13048.892) | (13269.449) |
| Observations | 547 | 547 | 1094 | 540 | 545 | 545 | 1090 | 536 |
| Controls | | ✓ | | ✓ | | ✓ | | ✓ |
| FE | | | ✓ | | | | ✓ | |

*Note:* Columns (1), (2), (5) and (6) present the results of the OLS specification, columns (3) and (7) present the results of the two-way fixed effects specification, and columns (4) and (8) present results from the ANCOVA IV specification where we instrument for actual treatment with assigned treatment to account for imperfect compliance. We winsorize savings, business assets and investments measures at the 99th percentile. Coefficients in columns (2), (4), (6), and (8) control for the respondent's location, marital status, household size, number of children, age, network size and level of education. Values in rows marked with heading *Levels* are values in UGX. We report standard errors clustered at the individual level in parentheses in columns (3) and (7) and White robust standard errors in all other columns.

Table A17: Treatment Effects on Household Outcomes - Other Specifications

| | Midline (6 months) | | | | Endline (18–24 Months) | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| **Daily HH Expenditure (Levels)** | | | | | | | | |
| Treat | 16.610 | -135.870 | -957.489 | -451.674 | -162.462 | -70.046 | -945.273 | -289.685 |
| | (501.932) | (461.638) | (950.098) | (498.376) | (479.155) | (451.284) | (936.550) | (510.215) |
| Observations | 545 | 545 | 1088 | 537 | 545 | 545 | 1088 | 535 |
| **Participant Expenditure (Levels)** | | | | | | | | |
| Treat | -593.038 | -683.341 | -588.985 | -917.951 | -204.709 | -157.819 | -119.526 | -212.584 |
| | (303.357) | (316.015) | (671.550) | (326.788) | (297.364) | (294.898) | (664.154) | (347.856) |
| Observations | 545 | 545 | 1082 | 534 | 544 | 544 | 1080 | 531 |
| **MUE** | | | | | | | | |
| Treat | 0.119 | 0.121 | -0.008 | 0.104 | 0.218 | 0.172 | 0.122 | 0.187 |
| | (0.084) | (0.087) | (0.106) | (0.093) | (0.092) | (0.095) | (0.113) | (0.113) |
| Observations | 507 | 507 | 962 | 476 | 505 | 505 | 952 | 468 |
| **Food Insecurity** | | | | | | | | |
| Treat | 0.115 | 0.114 | 0.094 | 0.114 | 0.035 | 0.015 | 0.012 | 0.030 |
| | (0.040) | (0.041) | (0.048) | (0.046) | (0.040) | (0.040) | (0.053) | (0.046) |
| Observations | 545 | 545 | 1086 | 536 | 545 | 545 | 1086 | 534 |
| **Remittances (Levels)** | | | | | | | | |
| Treat | -15260.508 | -15781.895 | -5439.108 | -12722.804 | -1931.422 | -362.991 | 4365.103 | 509.311 |
| | (8540.463) | (8800.798) | (14549.571) | (8291.846) | (5993.904) | (6374.433) | (16594.074) | (7099.060) |
| Observations | 539 | 539 | 1056 | 521 | 536 | 536 | 1052 | 518 |
| Controls | | ✓ | | ✓ | | ✓ | | ✓ |
| FE | | | ✓ | | | | ✓ | |

*Note:* Columns (1), (2), (5) and (6) present the results of the OLS specification, columns (3) and (7) present the results of the two-way fixed effects specification, and columns (4) and (8) present results from the ANCOVA IV specification where we instrument for actual treatment with assigned treatment to account for imperfect compliance. We winsorize daily expenditure measures and remittances at the 99th percentile. Coefficients in columns (2), (4), (6), and (8) control for the respondent's location, marital status, household size, number of children, age, network size and level of education. Values in rows marked with heading *Levels* are values in UGX. We report standard errors clustered at the individual level in parentheses in columns (3) and (7) and White robust standard errors in all other columns.