Entrepreneurship Training and Economic Resilience: Evidence on Building Skills for Women*

Megan Lang† and Julia Seither‡

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Abstract

Raising incomes while lowering income volatility are two core objectives for rural development. Despite substantial, existing literature, it is unclear whether entrepreneurship trainings can achieve both objectives by easing entry into entrepreneurship and improving enterprise performance. We randomize access to an entrepreneurship training among women in Uganda. Treated women are 19% more likely to run profitable businesses 18 months post-program. High-frequency data shows that they fare significantly better during the COVID-19 lockdown than women in the control group. Exploiting social network data, we detect significant network-based spillovers to the control group and provide novel tools to adjust estimates accordingly.

KEYWORDS: Entrepreneurship, Rural Development, Poverty Alleviation, Uganda.
JEL CODES: D13, D23, D91, J16, O12.

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†Economics Research, World Bank Group. Email: mlang@worldbank.org
‡Universidad del Rosario. Email: julia.seither@urosario.edu.co

1 Introduction

In rural areas, entrepreneurship is often one of multiple key income sources, potentially mitigating income risk. This is particularly true for poor households, who engage in both agricultural and non-agricultural enterprises (Banerjee and Duflo (2007)). Improving the performance of ultra-poor rural entrepreneurs has the potential to raise incomes while lowering income volatility, two core objectives for rural poverty alleviation.

Skills-based entrepreneurship programs are a common tool that aim to improve the performance of micro-enterprises, but it is unclear how well such programs work for ultra-poor rural entrepreneurs.[1] Existing studies in rural areas primarily sample clients of micro-finance institutions. These entrepreneurs are potentially different in their characteristics and skills compared to many ultra-poor households in rural areas. Entrepreneurship programs focused on rural poverty alleviation instead target households who may be lower ability or have less experience with entrepreneurship. We therefore study the importance of skills for the population of ultra-poor rural entrepreneurs.

Is a skills-based entrepreneurship training program that targets rural, low-income women an effective tool for poverty alleviation? We make progress on this question using a randomized control trial with 601 women in rural Uganda. We focus on women because multiple studies document that women suffer disproportionately from negative shocks (e.g., Asfaw and Maggio (2018), Qian (2008), Eastin (2018), among others) and because women's businesses are systematically less profitable across contexts (Ashraf, Delfino, and Glaeser (2019)). Both gaps highlight the importance of identifying tools to increase women's incomes and improve their resilience to economic shocks.

The program we study closely resembles many interventions in the business training literature. It teaches skills in eight, 2–3 hour modules over six months, complemented with individual mentoring from program instructors. The business practice components target

[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.

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 entrepreneurial skills through modules on identifying business opportunities and performing market research.[2]. Modules on stepping out of one's comfort zone and exposing women to success stories aim to build entrepreneurial soft skills. Critically, the program does not include access to credit or any transfer of cash or capital.[3]

We collect three types of data that allow us to better understand the dynamics of entrepreneurship for rural women. 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. We find evidence that the program leads to positive spillovers specifically through business links. As a result, we present straightforward average treatment effects alongside effects that adjust for positive, network-based spillovers throughout the paper.

The program is effective at enabling more women to generate income through entrepreneurship. At baseline, women in our sample are poorer than the average rural Ugandan household, following the usual targeting strategy of our partner organization.[5] Upon graduating from

[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 mentoring at women's homes or businesses in Lang and Seither (2022). The full design is the one 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.

[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.

the program, treated women are 16pp more likely to have an active business than women in the control group. They own 0.25 more businesses, on average, at endline. The program is also effective at promoting productive entrepreneurship: treated women are 11pp more likely to generate positive profits (off a control mean of 59%) and they are 9pp more likely to have positive profits in additional businesses (off a control mean of 16%). While we see positive effects on profit levels in both the main business and all other businesses, our estimates of intensive-margin effects are noisier and not quite statistically significant.

We examine dynamic treatment effects on savings and investments to understand how women entrepreneurs allocate their earnings. We find no significant effects on savings or investments at midline, right after program completion, and estimates are economically small. Although estimated effects on savings and investment in the main business are larger at endline at 13%–20% of control group means, they remain statistically insignificant. However, treated women invest 97% more in other businesses at endline. Together, results on savings and investment indicate high rates of re-investment in women's business portfolios.

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 overall sales 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 take this as promising evidence of the potential for entrepreneurship programs to increase women's resilience to certain types of shocks.

Improvements in business performance appear to be driven by strengthened entrepreneurial skills. We observe improved business tracking, price management, and more hours spent working in the main business. Effects on business practices persist in the medium-run (18-24 months) after accounting for learning spillovers. Entrepreneurial soft skills also improve, with treated women being 43% more likely to set goals than women in the control group and

exhibiting improvements in grit.

Finally, we examine program impacts on poverty alleviation by examining measures of household consumption expenditures and food insecurity. 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 provides novel evidence on the effects of entrepreneurship skills training as a poverty alleviation tool for women in rural markets, particularly for those from ultra-poor households. The literature on entrepreneurship training is large but inconclusive given differences in program structures, target populations, and market contexts. In examining the systematic review of the literature provided by McKenzie, Woodruff, et al. (2023), thirteen of seventeen studies of programs with existing entrepreneurs in urban markets found no significant effect on profits.[6] By contrast, five out of seven studies on existing entrepreneurs in rural markets find positive effects from training.[7] While this may suggest that entrepreneurship training programs are more effective in rural versus urban areas, differences in the sampled populations and program content make it difficult to draw definitive conclusions. Despite limited research on the impact of entrepreneurship programs on new entrepreneurs, three studies offer insights. Two studies in urban areas initially show positive effects that diminish over time (deMel, McKenzie, and Woodruff (2014), Brudevold-Newman et al. (2023)). Calderone et al. (2022) find sustained positive effects for young rural women participating in an entrepreneurship program for out of school youth. Our results on the effectiveness of an entrepreneurship training program for existing and aspiring women entrepreneurs in rural areas provides valuable insight into the effectiveness of such programs as a poverty allevi-

[6]Berge, 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).

[7]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.

ation strategy, expanding our understanding beyond the traditional focus on private sector development. Our focus on entrepreneurship in the context of rural development further allows us to provide novel evidence on the effect of entrepreneurship programs on women's ability to cope with economic shocks.

Our paper makes two methodological contributions. 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 the spillovers. Running a clustered RCT to measure spillovers experimentally entails high financial and logistical costs. When researchers, policymakers, and NGOs lack the funding and/or capacity for clustered RCTs, the tools we present provide credible, low-cost alternatives for evaluating and measuring network-based spillovers.

Positive, network-based spillovers from the entrepreneurship program we study speak to peer effects. We document positive spillovers to women in the control group via business ties.[8] This stands in contrast to Field et al. (2016), who find positive peer effects from social ties. In line with the results by McKenzie and Puerto (2021) and contrasting those in

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

Cai and Szeidl (2022) and Bakhtiar, Bastian, and Goldstein (2022), our results suggest that successful women entrepreneurs generate positive economic spillovers and highlight that the skills taught in training do not percolate through all types of social networks.

Our final contribution is to clarify the role of skills in anti-poverty programs. Comprehensive poverty reduction programs that combine intensive skills training, cash transfers, and social support have found positive impacts on a range of outcomes in rural contexts (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)). However, such programs are costly to implement precisely because they address multiple constraints simultaneously. The program we study sheds light on the return to the skills-based components of multifaceted poverty reduction programs.

Combined, our results show that entrepreneurship programs targeting rural, low-income women can help them become successful business owners that also transmit skills and economic benefits to other women in their business networks. Evidence from our RCT shows that removing skills-based barriers to entrepreneurship can be an effective tool for both new and existing women entrepreneurs in rural areas to open businesses that yield positive profits and to better cope with negative economic shocks. However, we do not observe significant reductions in poverty within the period of our study.

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.

While 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 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, 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, median daily expenditures are UGX 5,900 (USD 1.63) per household 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.

The composition of our sample reflects the target population of the program: ultra-poor women interested in starting a micro-enterprise or improving an existing one. 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. While 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.

[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.

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 re-start 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

[10] See Figure 1 for more detail on the curriculum.

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 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

[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.

reducing gender barriers. Lastly, program participants are not required to have concrete business ideas, literacy, or technical skills.[13]

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

[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 are selected on having a viable business and business experience.

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 implications 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.

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 2 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 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]

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 of 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. 1.7% of participants in the control group 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).

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

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

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 page 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 and pages to avoid ordering effects in constructing the network data. 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. 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 social 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 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%

[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.

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

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.

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

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

β 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

[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.

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

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

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 η 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 provide 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.

We use the high-frequency SMS data to qualitatively evaluate general equilibrium effects

[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.

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.[20] 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 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 entrepreneurship and alleviating poverty. However, given the potential for spillovers to women in the control group, we evaluate spillovers before presenting our main effects. Doing so allows us to more effectively contextualize our discussion of the results.[21]

5.1 Quantifying spillovers

We find evidence of positive, significant network-based spillovers to women in the control group. Table 1 shows estimated spillovers for owning a business and for profits of the main business at endline. 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

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

[21] As we did not pre-register the adjusted treatment effects, we report both unadjusted and adjusted estimates in all results tables.

spillovers for women connected at baseline through business links. The spillovers are driven by business links between women: friendship links to treated women exhibit no statistically significant effects and coefficients are a fraction of those for business links. The spillovers appear to be economically meaningful. Having baseline business links with a treated woman increases the likelihood of owning a business by 6.1pp, nearly 10%. It increases profits by around 15%, though estimates are not statistically significant.

Given that business links with treated women lead to positive spillovers, we adjust all estimated average treatment effects using spillovers specifically from business links.[22] 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, presenting both throughout the remainder of the paper.

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 3 shows that there are no significant changes in trends for revenues among women in the control group over the period of the experiment. 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. While 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

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

baseline report having a business at midline, and half open a business by endline.

5.2 Program impacts on entrepreneurship

When examining program impacts, we first consider whether the program allows women to generate income from productive entrepreneurship. 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 entrepreneurship and business performance, we proceed to consider potential mechanisms before turning to the effects of women's entrepreneurship on household poverty alleviation.

Column (1) of Table 2 shows that the program removes barriers to entrepreneurship on the extensive margin. As spillovers are negligible at midline, unadjusted and adjusted estimates are similar. Women who participate in the program are 16.4pp (29%) more likely to own a business than women in the control group at midline (16.2pp adjusted). The effect declines to 8pp (12%) at endline, unadjusted, and 9.6pp adjusted, a 14.4% increase. This suggests that ignoring spillovers would substantially under-estimate program success on entrepreneurship. Column (2) shows that the program generates large and persistent impacts on the number of businesses owned, with treated women owning 24.5% (unadjusted) to 28% more businesses than women in the control group at endline. This implies that many treated women open multiple businesses. 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 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). In contrast, 67% of treated women are able to generate income

[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.

from their businesses, a 37% increase. At endline, treated women are 19% more likely to have positive profits from their main business, and 58% 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 and 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 than extensive margin effects. Column (1) shows that the training increases sales in the main business by 14% at midline, adjusted, compared to the control group (UGX 5155 or around USD 1.33 over 3 days). At endline, the adjusted effect grows to UGX 8463 (USD 2.18), an effect of 19%. Effects on business profits show similar patterns. Column (2) of Table 3 show that adjusted monthly profits in the main business are 23% (USD 4.16) higher at midline and 15% (USD 2.92) 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 34% (USD 1.63) higher at midline and 63% (USD 2.82) higher at endline relative to women in the control group. Despite consistently positive effects, none are statistically significant after correcting for multiple inference.

Adjusting for spillovers leads to meaningful differences in proportional terms. For instance, failing to adjust for positive spillovers at endline would understate treatment effects on profits in the main business by 38% and profits in all other businesses by 28%. Such differences are important to understand when evaluating program effectiveness.

Effects on savings and investment show a similar pattern to business profits, with investments in other businesses showing large significant increases at endline. Table 4 shows

that the adjusted value of assets in the main business at midline is 6.5% (USD 2.03) higher at midline relative to the control group, on average. By endline, that increases to 18% (USD 5.75), although estimates are noisy and not statistically significant. However, both the unadjusted and adjusted value of investments in other businesses is nearly double the control group mean at endline, increasing by around USD 7.50 on average. The results on savings and investments coupled with the results on business outcomes suggest that the program is effective at helping women learn how to raise capital and identify profitable business opportunities to grow income opportunities in the medium-run.

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.[24] Figure 4 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. We estimate effects 5 months before the lockdown, during the 6-month lockdown, and for 4.5 months after the lockdown was lifted.[25]

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

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

[25]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.

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.

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 entrepreneurship, we now turn to the underlying mechanisms before considering whether the program is an effective tool for poverty alleviation.

5.4 Mechanisms

Given the nature of the program we study, we first examine program impacts on a range of business practices to determine whether women are implementing the skills taught in the entrepreneurship program. Columns (1) and (2) of Table 5 show that the program has sustained impacts 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. Adjusted measures of both indicators increase by 30%–48% at midline and 21%–29% at endline, and both survive multiple inference corrections at midline but not at endline. We also examine changes in work hours and find large increases at midline and smaller, not quite statistically significant changes at endline.

Next, we examine entrepreneurial "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.[26]

The program causes sustained improvements in goal setting and grit. Treated women are 42% more likely to set goals for their business at midline than women in the control group. While the effect at endline does not survive our multiple inference corrections, the magnitude of the effect remains stable. We also find improvements in grit, where adjusted effects show significant increases at midline and endline, though again increases survive our multiple inference corrections only at midline. Despite some improvements in locus of control measures, we are not sufficiently powered to estimate effects precisely.

Comparing the unadjusted to adjusted values at endline shows that there is some evidence for positive spillovers in both hard and soft skills. For instance, the adjusted effect on the price management index is 16% higher than the unadjusted effect. The adjusted effect on the goal setting index is 12% higher than the unadjusted effect. Such differences suggest that the positive spillovers to women in the control group are not purely economic in nature, despite the fact that we only find evidence of significant spillovers from business ties and not other types of network links. It appears that treated women are transmitting skills learned in the program to women in the control group with whom they share business ties.

Our results suggest that entrepreneurial skills are a relevant constraint for the population of women entrepreneurs who we study. Large effects on skills align with the significant impacts we find on the extensive margin: women appear to use the skills learned in the program to start income-generating enterprises. Our result on grit provides suggestive evidence of a key mechanism underlying our results on the COVID-19 lockdown, as it suggests that the program enhances women's ability to cope with adverse events.

[26] We pre-registered two additional psychometric measures: self-efficacy and aspirations regarding social status. For brevity, we present these results in Table A9. Neither shows evidence of significant impacts from the program.

5.5 Program impacts on poverty

Our final set of results is motivated by our focus on entrepreneurship programs as tools for poverty alleviation. As such, we expand our focus from women's enterprises to women's households. We examine three measures of consumption expenditures, a binary indicator for food insecurity, and the amounts of remittances received by households.

Table 6 shows that there are 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 participant'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 12.1pp, a 45% 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. Taken together, 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 (see for example Bandiera, Burgess, et al. (2017)).

6 Discussion and Conclusion

Our study provides strong evidence that skills are a relevant barrier to entrepreneurship among rural women. We show that 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. Most impacts appear to come through sustained improvements in a range of hard and soft entrepreneurial skills. 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 entrepreneurship 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. While 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 do not appear to be solely driven by economic activities between businesses: we find suggestive 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.

While our study highlights the importance of skills-based barriers for women on the margins of entrepreneurship, reconciling our work with the large literature on interventions with existing entrepreneurs will require understanding the first-order barriers faced by entrepreneurs at each stage of business growth. For instance, the women in our sample

learned how to raise enough capital to start a micro-enterprise. Is there a point at which the capital-building skills taught become insufficient and credit constraints bind? We similarly have limited visibility into the barriers faced by women attempting to start and run businesses in different sectors. Building a granular understanding of the ladder of constraints different women entrepreneurs face is critical to evaluating the overall value proposition of entrepreneurship as a tool for rural development.

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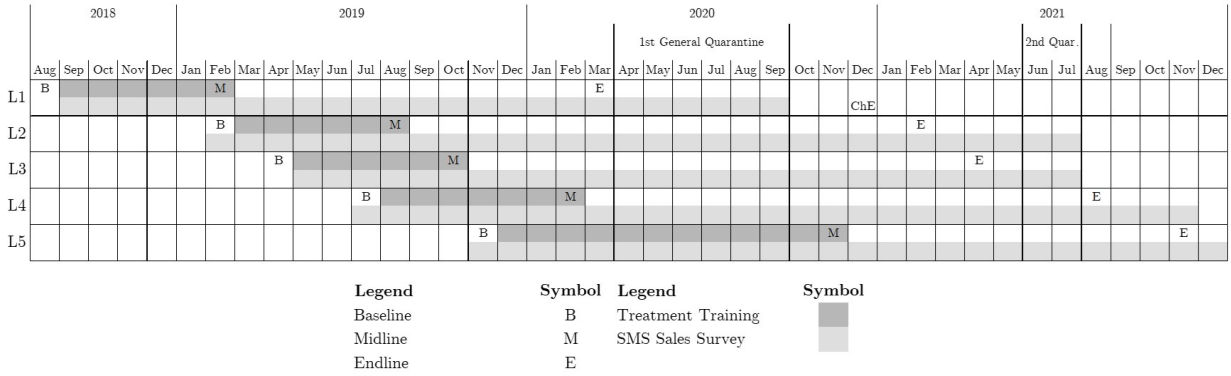
7 Figures

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

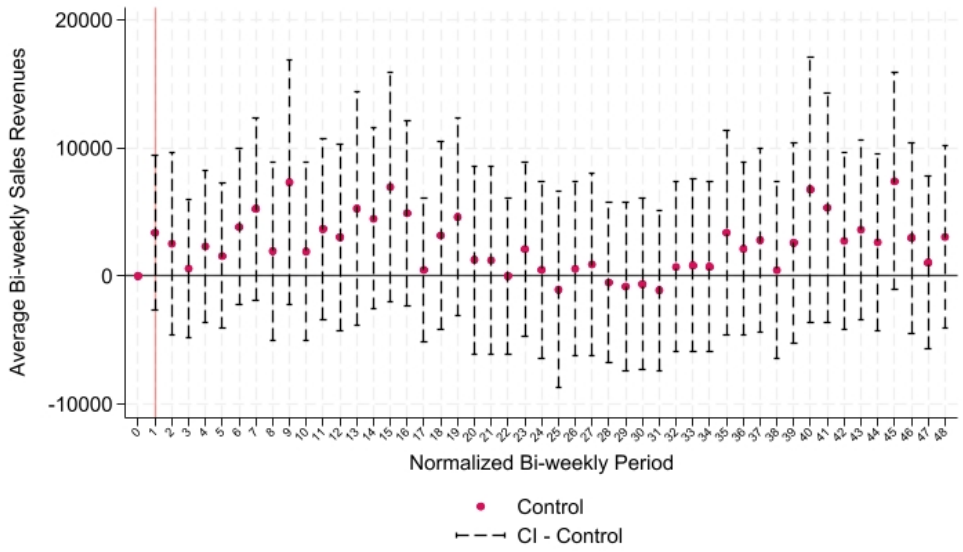
Note: Each module is between 2–3 hours long and taught at a central training venue such as a school or church. Women participate in groups of 50–70.

Figure 1: Training Module Content

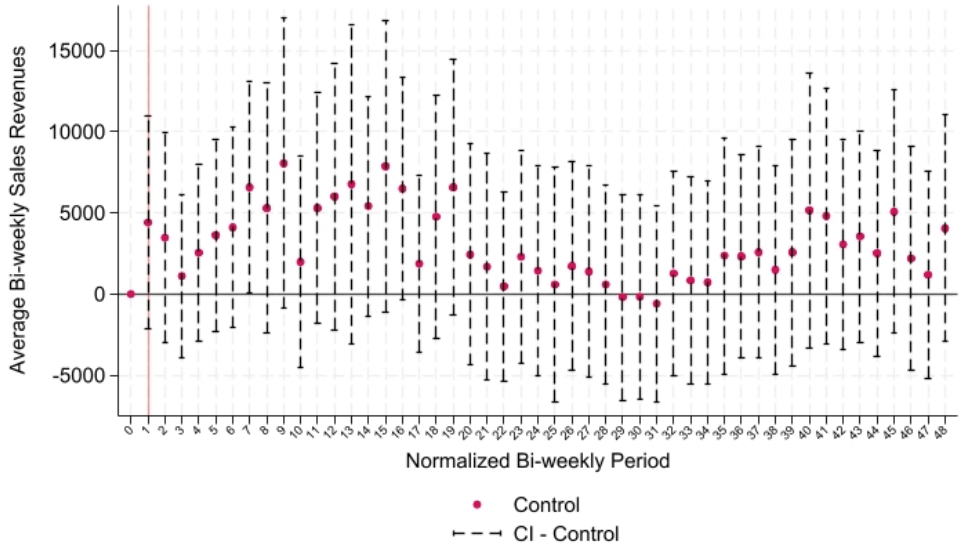


Note: Each line of the timeline refers to a study location. Letters denote in-person surveys. Light grey denotes SMS surveys. Dark grey shows the period of active treatment, or the period when the program took place.

Figure 2: Project Timeline



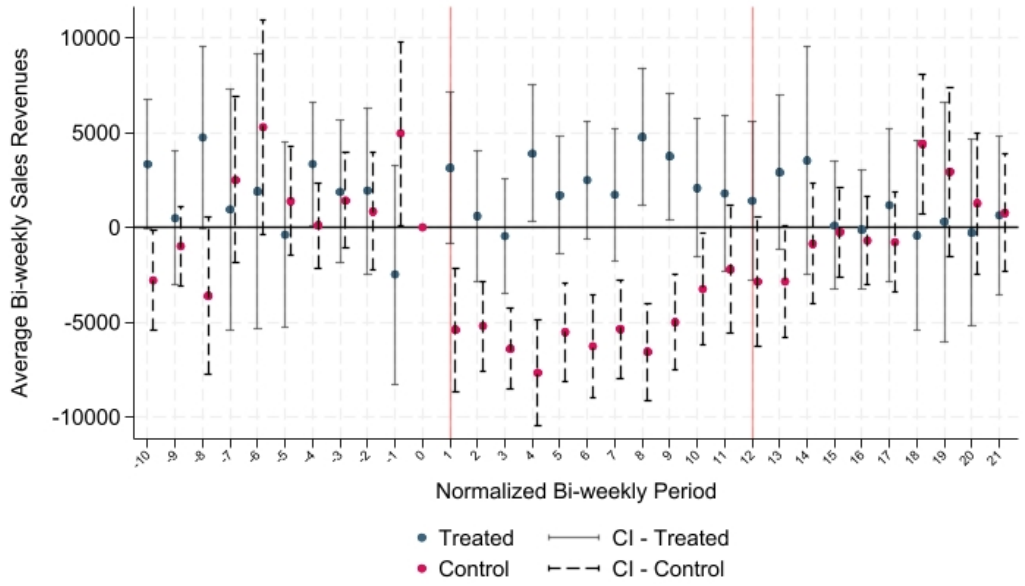
(a) Unweighted



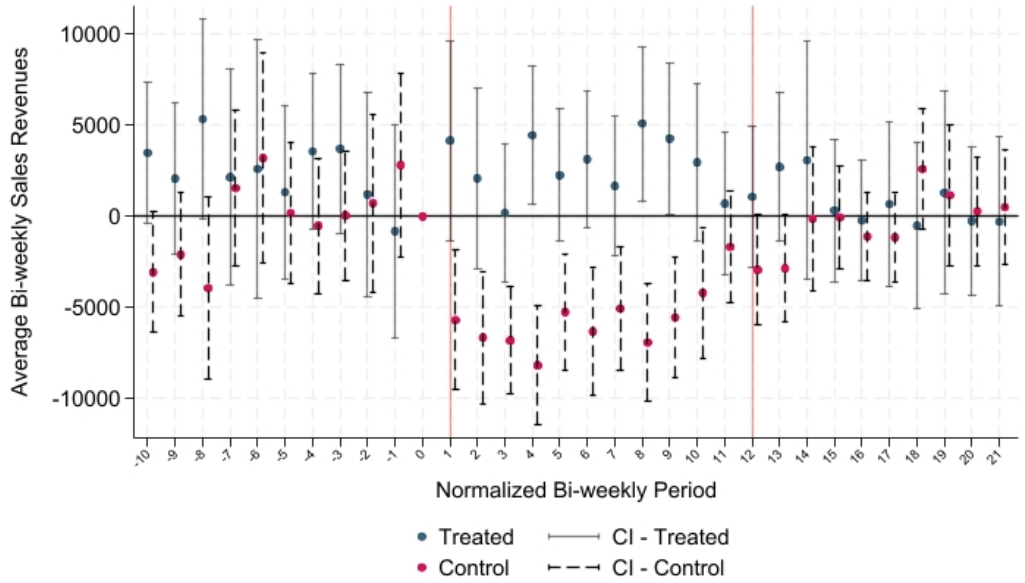
(b) Weighted

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 3: SMS Bi-weekly Average Reported Sales



(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. The red lines indicate the beginning and end of the first nationwide Ugandan COVID-19 lockdown.

Figure 4: Reactions to COVID-19 Lockdown by Treatment Group

8 Tables

Table 1: Spillover Effects on Business Outcomes at Endline

	Control group only			
	Own a Business		Profits	
	(1)	(2)	(3) Levels	(4) Levels
Panel A: Any Network Link				
Link - Treated	0.028	0.032	7756.044	7047.946
	(0.014)	(0.018)	(4794.610)	(5064.315)
Observations	253	253	247	247
Control Mean	0.493	0.493	41637.681	41637.681
Adj. R^2	0.133	0.112	0.162	0.152
Panel B: Friendship Link				
Link - Treated	0.019	0.023	399.254	1637.884
	(0.016)	(0.018)	(4822.311)	(5248.650)
Observations	253	253	247	247
Control Mean	0.635	0.635	74508.333	74508.333
Adj. R^2	0.124	0.101	0.152	0.144
Panel C: Business Link				
Link - Treated	0.057	0.061	15938.093	11618.014
	(0.030)	(0.036)	(14436.655)	(12657.535)
Observations	253	253	247	247
Control Mean	0.627	0.627	72549.541	72549.541
Adj. R^2	0.130	0.107	0.161	0.149
Controls		✓		✓

Note: We winsorize profit measures at the 99th percentile. Coefficients are ANCOVA estimates that control for the outcome at baseline, and overall network size at baseline in all regressions. Additionally, we control for the respondent's location, marital status, household size, number of children, age, and level of education for columns where controls are included. These results correspond to spillover effects for the control group at endline. Values in columns marked with heading *Levels* are values in UGX. We report White robust standard errors.

Table 2: 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
			Panel A: Midline (6 months)			
ITT	0.164	0.256	0.140	0.183	0.055	0.171
	(0.038)	(0.063)	(0.040)	(0.040)	(0.033)	(0.039)
	[0.010]	[0.010]	[0.010]	[0.010]	[0.119]	[0.010]
Adj. ITT	0.162	0.253	0.151	0.186	0.059	0.167
	(0.039)	(0.064)	(0.041)	(0.040)	(0.033)	(0.040)
	[0.001]	[0.001]	[0.001]	[0.001]	[0.013]	[0.001]
Observations	546	546	547	547	547	547
Control Mean	0.566	0.833	0.420	0.486	0.179	0.521
Adj. R^2	0.206	0.293	0.177	0.190	0.139	0.186
			Panel B: Endline (18–24 months)			
ITT	0.080	0.221	0.140	0.095	0.083	0.103
	(0.039)	(0.067)	(0.042)	(0.042)	(0.034)	(0.041)
	[0.059]	[0.010]	[0.010]	[0.040]	[0.040]	[0.030]
Adj. ITT	0.096	0.253	0.150	0.111	0.090	0.111
	(0.039)	(0.067)	(0.043)	(0.041)	(0.034)	(0.041)
	[0.010]	[0.001]	[0.002]	[0.007]	[0.007]	[0.007]
Observations	545	544	545	545	545	545
Control Mean	0.667	0.903	0.457	0.589	0.155	0.593
Adj. R^2	0.093	0.163	0.097	0.066	0.041	0.079

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. 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 the reported 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. Romano-Wolf multiple hypothesis test q-values are presented in brackets.

Table 3: Adjusted Treatment Effects on Business Outcomes

	Main Business Sales	Main Business Profits	Other Businesses Profits	All Businesses Profits
	(1) Levels	(2) Levels	(3) Levels	(4) Levels
	Panel A: Midline (6 months)			
ITT	4087.60	16949.96	7015.56	27589.10
	(6389.44)	(11177.33)	(5308.29)	(13417.41)
	[0.545]	[0.327]	[0.327]	[0.158]
Adj. ITT	5155.40	16067.61	6315.06	24958.95
	(6727.24)	(11432.22)	(5524.28)	(14211.93)
	[0.471]	[0.463]	[0.463]	[0.463]
Observations	532	525	547	547
Control Mean	37714.29	69693.20	18589.11	88718.68
Adj. R^2	0.153	0.186	0.080	0.214
	Panel B: Endline (18–24 Months)			
ITT	9380.31	8214.84	8606.68	18640.44
	(7537.53)	(10737.99)	(5242.53)	(12850.84)
	[0.337]	[0.475]	[0.248]	[0.248]
Adj. ITT	8463.50	11301.73	10938.79	24456.09
	(7484.88)	(10439.54)	(5021.51)	(12518.50)
	[0.163]	[0.163]	[0.113]	[0.113]
Observations	538	530	545	545
Control Mean	45476.26	76934.12	17251.55	94589.53
Adj. R^2	0.146	0.113	0.058	0.126

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. 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. Romano-Wolf multiple hypothesis test q-values are presented in brackets.

Table 4: Adjusted Treatment Effects on Re-Investment Outcomes

	Savings	Business Assests	Investments in Other Businesses
	(1) Levels	(2) Levels	(3) Levels
	Panel A: Midline (6 months)		
ITT	1193.12	3558.45	11562.89
	(22888.91)	(20766.07)	(14493.53)
	[0.970]	[0.970]	[0.832]
Adj. ITT	4603.86	7866.86	3176.11
	(22079.99)	(21319.16)	(16018.14)
	[1.000]	[1.000]	[1.000]
Observations	529	547	547
Control Mean	166976.10	119889.56	47501.17
Adj. R^2	0.290	0.325	0.087
	Panel B: Endline (18–24 Months)		
ITT	26805.09	17021.32	27773.52
	(25795.38)	(24926.97)	(12709.47)
	[0.455]	[0.455]	[0.050]
Adj. ITT	31812.95	22286.89	28878.05
	(25438.22)	(25338.68)	(12228.25)
	[0.268]	[0.339]	[0.058]
Observations	532	545	545
Control Mean	162703.16	125942.20	29836.05
Adj. R^2	0.194	0.188	0.029

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. 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. Romano-Wolf multiple hypothesis test q-values are presented in brackets.

Table 5: Mechanisms of Adjusted Treatment Effects

						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
					Panel A: Midline (6 months)				
ITT	0.319	0.472	13.507	0.340	1.014	-0.004	0.115	0.362	13807.09
	(0.142)	(0.165)	(4.537)	(0.139)	(0.484)	(0.220)	(0.409)	(0.339)	(252781.76)
	[0.099]	[0.050]	[0.050]	[0.089]	[0.109]	[1.000]	[0.990]	[0.683]	[1.000]
Adj. ITT	0.283	0.489	13.410	0.272	1.340	-0.027	0.209	0.546	96189.10
	(0.139)	(0.154)	(4.443)	(0.137)	(0.490)	(0.211)	(0.399)	(0.342)	(236621.01)
	[0.060]	[0.012]	[0.012]	[0.060]	[0.015]	[0.429]	[0.346]	[0.093]	[0.346]
Observations	290	281	228	243	544	543	543	544	430
Control Mean	0.957	1.012	27.909	0.643	29.488	15.836	-12.914	-14.645	1504624.45
Adj. R^2	0.167	0.114	0.265	0.145	0.108	0.029	0.146	0.094	0.225
					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
	(0.147)	(0.176)	(4.617)	(0.148)	(0.476)	(0.242)	(0.437)	(0.349)	(196431.19)
	[0.475]	[0.446]	[0.475]	[0.475]	[0.386]	[0.475]	[0.495]	[0.525]	[0.475]
Adj. ITT	0.235	0.361	8.150	0.296	1.161	0.187	0.603	0.064	-346290.49
	(0.138)	(0.165)	(4.222)	(0.138)	(0.477)	(0.241)	(0.430)	(0.350)	(198272.93)
	[0.108]	[0.108]	[0.108]	[0.108]	[0.108]	[0.196]	[0.137]	[0.260]	[0.108]
Observations	286	274	229	231	541	540	540	541	441
Control Mean	1.133	1.239	34.850	0.688	30.094	15.801	-12.121	-14.191	1577941.91
Adj. R^2	0.060	-0.011	0.145	0.047	0.110	0.043	0.037	0.046	0.062

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. 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. Romano-Wolf multiple hypothesis test q-values are presented in brackets.

Table 6: Adjusted Treatment Effects on Household Outcomes

	Daily Expenditure				Remittances
	(1) HH Levels	(2) Participant Levels	(3) MUE	(4) Food Insecurity	(5) Levels
Panel A: Midline (6 months)					
ITT	-184.35	-644.36	0.075	0.111	-12313.70
	(466.14)	(307.24)	(0.086)	(0.039)	(7615.29)
	[0.644]	[0.129]	[0.574]	[0.040]	[0.386]
Adj. ITT	-153.14	-490.28	0.040	0.121	-5065.95
	(449.79)	(311.95)	(0.086)	(0.039)	(7563.54)
	[0.787]	[0.303]	[0.787]	[0.010]	[0.787]
Observations	544	541	481	543	528
Control Mean	7205.16	3565.36	-0.026	0.270	34309.80
Adj. R^2	0.198	0.041	0.074	0.124	0.169
Panel B: Endline (18–24 Months)					
ITT	-123.61	-171.88	0.126	0.016	760.15
	(448.69)	(306.17)	(0.096)	(0.040)	(5932.07)
	[0.980]	[0.970]	[0.604]	[0.980]	[0.980]
Adj. ITT	-278.88	-44.93	0.142	0.006	-497.52
	(441.47)	(289.34)	(0.096)	(0.040)	(6399.34)
	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]
Observations	544	540	476	543	526
Control Mean	6929.52	3443.70	-0.076	0.310	25547.62
Adj. R^2	0.183	0.013	0.047	0.091	0.072

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. 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. Romano-Wolf multiple hypothesis test q-values are presented in brackets.

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 recognize 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 Tables

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)
Treat	-0.080
	(0.716)
Age	0.105
	(0.034)
Married	0.383
	(1.407)
Divorced	1.028
	(1.420)
Single	2.680
	(1.926)
Primary Ed.	-1.821
	(0.772)
Secondary Ed.	-0.432
	(1.451)
Father Primary Ed.	0.400
	(1.078)
Father Secondary Ed.	-3.080
	(1.615)
Mother Primary Ed.	0.857
	(1.184)
Mother Secondary Ed.	-0.105
	(2.393)
Employed	0.209
	(0.919)
HH Size	-0.496
	(0.318)
Minors in HH	1.054
	(0.377)
Daily Expenditure HH	0.000
	(0.000)
Savings	0.000
	(0.000)
Remittances	-0.000
	(0.000)
Own a Business	-0.204
	(0.960)
Sales - MB	0.000
	(0.000)
Profits - MB	-0.000
	(0.000)
Profits - OB	-0.000
	(0.000)
Network Size	0.110
	(0.099)
Locus - Internal	0.507
	(0.162)
Locus - PO	-0.080
	(0.096)
Locus - Chance	0.118
	(0.122)
Observations	568

Table A5: Treatment Effects on Business Outcomes by Baseline Business Ownership

	Own a Business	No. Businesses	Sales	Profits	Profits
	(1)	(2)	(3) Levels	(4) Levels	(5) Levels
Panel A: Midline (6 months)					
Treat	0.211	0.325	-1148.240	28006.175	17381.013
	(0.062)	(0.085)	(9324.163)	(12297.603)	(5551.735)
Baseline Business (BB)	0.402	0.177	-6880.450	16490.528	30376.489
	(0.058)	(0.115)	(9367.011)	(16036.404)	(6977.082)
Treat x BB	-0.087	-0.128	9873.460	-20855.899	-18180.905
	(0.076)	(0.123)	(12749.693)	(22047.855)	(10053.637)
Observations	546	546	532	525	547
Treat x BB p-value	0.008	0.035	0.327	0.690	0.926
Control Mean	0.566	0.833	37714.286	69693.200	18589.105
Adj. R^2	0.207	0.293	0.151	0.185	0.110
Panel B: Endline (18–24 Months)					
Treat	0.115	0.205	10524.848	15045.224	4438.098
	(0.064)	(0.098)	(10389.037)	(12529.987)	(6188.708)
Baseline Business (BB)	0.308	0.120	9184.212	26954.841	7815.456
	(0.059)	(0.129)	(12446.509)	(16897.282)	(7758.034)
Treat x BB	-0.065	0.032	-1922.152	-13061.184	8143.646
	(0.079)	(0.134)	(15066.229)	(20745.928)	(11267.245)
Observations	545	544	538	530	545
Treat x BB p-value	0.285	0.009	0.437	0.905	0.151
Control Mean	0.667	0.903	45476.265	76934.118	17251.550
Adj. R^2	0.093	0.163	0.144	0.115	0.065

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. Values in columns marked with heading *Levels* are values in UGX. We report robust standard errors in parentheses. Treat x BB p-value corresponds to the null hypothesis Treat + Treat x BB = 0.

Table A6: Treatment Effects on Psychometric Measures by Baseline Business Ownership

			Locus of Control			Aspirations	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Self-Efficacy	Grit	Internal	PO	Chance	Income Levels	Social Status
Panel A: Midline (6 months)							
Treat	0.194	2.191	-0.085	-0.844	-0.036	-157701.426	0.107
	(0.917)	(0.714)	(0.343)	(0.602)	(0.455)	(401083.839)	(0.134)
Baseline Business (BB)	1.130	2.183	0.350	-0.120	0.080	76279.831	0.010
	(0.832)	(0.729)	(0.309)	(0.591)	(0.497)	(366555.543)	(0.125)
Treat x BB	0.627	-2.152	0.175	1.817	0.762	338807.939	0.009
	(1.161)	(0.950)	(0.439)	(0.808)	(0.665)	(551399.610)	(0.170)
Observations	543	544	543	543	544	430	536
Treat x BB p-value	0.247	0.951	0.741	0.070	0.142	0.598	0.309
Control Mean	38.605	29.488	15.836	-12.914	-14.645	1504624.454	3.079
Adj. R^2	0.104	0.121	0.033	0.158	0.096	0.224	0.078
Panel B: Endline (18–24 Months)							
Treat	-0.586	0.444	0.403	-0.456	-0.694	-405766.188	-0.187
	(0.901)	(0.745)	(0.333)	(0.646)	(0.523)	(250931.605)	(0.132)
Baseline Business (BB)	0.614	0.617	0.070	-0.421	0.302	373240.083	-0.377
	(0.866)	(0.729)	(0.359)	(0.642)	(0.525)	(340961.767)	(0.138)
Treat x BB	2.101	0.915	-0.137	1.699	0.897	160886.784	0.413
	(1.131)	(0.941)	(0.474)	(0.859)	(0.688)	(404304.636)	(0.180)
Observations	540	541	540	540	541	441	535
Treat x BB p-value	0.037	0.025	0.448	0.035	0.659	0.420	0.066
Control Mean	39.289	30.094	15.801	-12.121	-14.191	1577941.909	2.992
Adj. R^2	0.122	0.117	0.039	0.043	0.054	0.070	0.052

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. We measure generalized self-efficacy following Schwarzer and Jerusalem (1995). Out measures of grit follow Duckworth, Peterson, et al. (2007) and Duckworth and Quinn (2009). We draw our locus of control measures from Levenson (1973) and our measure of subjective social status from Adler et al. (2000). Values in columns marked with heading *Levels* are values in UGX. We report White robust standard errors in parentheses. Treat x BB p-value corresponds to the null hypothesis Treat + Treat x BB = 0.

Table A7: Treatment Effects on Re-Investments by Baseline Business Ownership

	Savings	Business Assests	Investments in Other Businesses
	(1) Levels	(2) Levels	(3) Levels
Panel A: Midline (6 months)			
Treat	-4504.742	-17368.834	13709.917
	(23529.280)	(24709.578)	(15496.826)
Baseline Business (BB)	36622.658	-1041.770	55706.188
	(32968.489)	(31216.561)	(19861.804)
Treat x BB	12986.420	39430.528	-1180.755
	(45208.732)	(38641.614)	(27170.401)
Observations	529	547	547
Treat x BB p-value	0.814	0.484	0.595
Control Mean	166976.096	119889.556	47501.167
Adj. R^2	0.292	0.325	0.109
Panel B: Endline (18–24 Months)			
Treat	57139.007	43495.240	19341.602
	(29580.427)	(29383.893)	(12536.782)
Baseline Business (BB)	55602.226	50438.201	34976.625
	(34219.780)	(35658.431)	(16364.434)
Treat x BB	-56211.228	-50303.745	16897.560
	(52089.593)	(49148.123)	(23939.550)
Observations	532	545	545
Treat x BB p-value	0.982	0.863	0.069
Control Mean	162703.159	125942.202	29836.047
Adj. R^2	0.194	0.188	0.048

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. 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. Values in columns marked with heading *Levels* are values in UGX. We report White robust standard errors in parentheses. Treat x BB p-value corresponds to the null hypothesis Treat + Treat x BB = 0.

Table A8: Treatment Effects on Household Outcomes by Baseline Business Ownership

	Daily HH Expenditure				Remittances
	(1) HH Levels	(2) Participant Levels	(3) MUE	(4) Food Insecurity	(5) Levels
Panel A: Midline (6 months)					
Treat	-112.133	-864.712	0.002	0.046	-17192.862
	(670.415)	(460.889)	(0.142)	(0.059)	(11521.910)
Baseline Business (BB)	635.446	668.699	-0.065	-0.133	-14204.748
	(678.184)	(485.825)	(0.121)	(0.054)	(13128.242)
Treat x BB	-101.709	455.524	0.128	0.117	8663.971
	(937.964)	(601.037)	(0.177)	(0.078)	(15258.911)
Observations	544	541	481	543	528
Treat x BB p-value	0.739	0.321	0.219	0.002	0.443
Control Mean	7205.162	3565.362	-0.026	0.270	34309.804
Adj. R^2	0.197	0.055	0.071	0.130	0.169
Panel B: Endline (18–24 Months)					
Treat	-399.704	-192.087	0.211	-0.050	-5741.107
	(664.370)	(467.732)	(0.146)	(0.059)	(8943.710)
Baseline Business (BB)	-283.748	381.045	-0.062	-0.115	-11058.094
	(682.529)	(473.727)	(0.137)	(0.056)	(7495.498)
Treat x BB	518.029	49.897	-0.157	0.122	11888.163
	(915.549)	(610.255)	(0.194)	(0.080)	(11540.867)
Observations	544	540	476	543	526
Treat x BB p-value	0.848	0.712	0.669	0.185	0.462
Control Mean	6929.521	3443.703	-0.076	0.310	25547.619
Adj. R^2	0.180	0.012	0.049	0.095	0.072

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. IHS indicates that we present results using an inverse hyperbolic sine transformation. We calculate the MUE using consumption expenditures over the past week on seventeen food items, following the methods outlined in Ligon (2020). 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. Values in columns marked with heading *Levels* are values in UGX. We report White robust standard errors in parentheses. Treat x BB p-value corresponds to the null hypothesis Treat + Treat x BB = 0.

Table A9: Mechanisms of Adjusted Treatment Effects

	(1) Self-Efficacy	(2) Social Status Aspirations
Panel A: Midline (6 months)		
ITT	0.477	0.111
	(0.578)	(0.086)
	[0.416]	[0.416]
Adj. ITT	0.865	0.140
	(0.580)	(0.089)
	[0.158]	[0.158]
Observations	543	536
Control Mean	38.605	3.079
Adj. R^2	0.096	0.081
Panel B: Endline (18–24 Months)		
ITT	0.497	0.032
	(0.586)	(0.089)
	[0.703]	[0.762]
Adj. ITT	0.827	0.041
	(0.582)	(0.091)
	[0.452]	[0.484]
Observations	540	535
Control Mean	39.289	2.992
Adj. R^2	0.105	0.039

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. Adjusted ITT shows effects adjusted for positive spillovers to the control group. We measure generalized self-efficacy following Schwarzer and Jerusalem (1995) and our measure of subjective social status from Adler et al. (2000). We report White robust standard errors in parentheses. Romano-Wolf multiple hypothesis test q-values are presented in brackets.

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.[27] 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.

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

Table A10: Lasso Selected Variables - OLS

	(1) 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.